# Stuck in a Paradigm

A Kuhnian interpretation of economics during the Great Financial Crisis


## Abstract

The Great Financial Crisis (GFC) came as a surprise for most economists, and henceforth was at the same time a crisis for economics. This thesis attempts to offer an explanation for this that goes beyond scapegoating. In order to do so, an interpretation of economics before and during the GFC is developed, based on the ideas of Thomas Kuhn. The paradigm of mainstream economics is analysed. The main ingredients of this paradigm – the DSGE model, the Efficient Market Hypothesis and the Great Moderation – did not prepare economist for the GFC. The GFC is an anomaly, something that does not fit in the existing paradigm. The fact that economists did not anticipate the GFC is a consequence of the paradigm within which they worked. To avoid a similar scenario in the future, economists have to think beyond the borders of the existing paradigm. This requires an adjustment of mainstream economics, or even a completely different paradigm.



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Date: 24/5/2017

## Acknowledgements

Writing this thesis has been a difficult process, in which I have wandered between heights and lows, not in the least because I had to deal with my own personal ‘paradigm shift’. I am relieved that I am finally done with it, to be honest – at times I thought this moment would never arrive! The infinite patience of my supervisor, Bas Jacobs, is almost beyond credibility; and his willingness to support me in writing about a topic which is somewhat outside his area of expertise, and to critically assess the performance of macroeconomics, has given me hope that economics might be redeemable after all. I am grateful to Jack Vromen for giving me valuable feedback on the more philosophical parts of this thesis. All the remaining mistakes are – of course – mine.

On a personal level, I would like to thank my family – parents, brothers and sisters (in law), nieces, nephew – for their love and support during difficult times. Many thanks to my friends, without whom life would be so much less enjoyable. Much gratitude to the people of the Ichthuskerk, for being such a warm community.

# Content

## 1. Introduction

It is a commonplace to say that we should learn from our mistakes, but that doesn't make it less true. Unfortunately, we often opt for the easier but less wise option of ignoring our mistakes, convinced that facing them would be too painful. Indeed, some mistakes are too painful too face and Freud poignantly pointed out the ingenuity with which we avoid these traumas. However, Freud also knew that, in order to avoid insanity, we have to face the truth about ourselves eventually. We may take heart in the aphorism of one of the other 'masters of suspicion, Nietzsche (the third, ironically, being Karl Marx): 'What does not kill me makes me stronger'. So, this thesis takes up the difficult task of making sense of our (economist's) recent failure – not to ridicule those economists (which is, sadly, the tone of many commentaries), but in order to learn from it, so that economics may come out of this situation not traumatised – but stronger.

The 'failure' spoken of in the previous paragraph is – of course – the astonishment about the Great Financial Crisis (GFC), also known as the Great Recession. The GFC was an exceptionally large disturbance of economic growth (World Bank, 2017), which affected households, businesses, the government and especially the financial sector (Hellwig, 2008). This impact generated much attention for economics, most of it negative. Most economists had not seen this crisis coming, and it took them quite a long time before they realized the gravity of the problems; Ben Bernanke, the chairman of the FED, declared in 2007 (when problems on the U.S. housing market where already getting more serious): "Overall, the economy appears likely to continue to expand at a moderate pace over coming quarters" (Bernanke, 2007c). This unconvincing act of economists not only reflected badly on their abilities, but also on the models on which their predictions and analyses were based; anticipating what is yet to come we could say that it questioned the economic paradigm. The fact is that the models in use did offer little or no guidance when it came to dealing with the consequences of the crisis, which forced economists to regress to "hand-waving common-sense remedies" (Colander, et al., 2009, p. 250) or the ancient wisdom of the likes of Keynes and Minsky, painfully accentuating the "uselessness of most 'state of art' academic monetary economics" (Buiter, 2014).

All of this would not have been that much of a problem for economists weren't it for the fact that, before the GFC, macroeconomists were quite proud of their own ideas. This confidence forms an ironic and sharp contrast with the poor performance of economics just a few months later. Leading macroeconomist Olivier Blanchard declared in August 2008 that ‘the state of macro is good’, despite some issues that were not ‘deadly’ (Blanchard, The State of Macro, 2008) and Robert Lucas, another important figure in modern macroeconomics, declared in his 2003 presidential address to the AEA that “the central problem of depression prevention has been solved” (Lucas, 2003); Mishkin, member of the FED board from 2006 to 2008, spoke in 2007 about the growing scientificity of monetary economics and how developments in monetary economics had contributed significantly to “the policy successes that so many countries have been experiencing in recent years” (Mishkin, 2007, p. 31). The paradigm these economists were so satisfied with is what we will call the ‘mainstream’ (also known as the Standard Model, Conventional Wisdom, etc.), a consensus between Saltwater (Keynesian) economics and Freshwater (Neoclassical) economics (see Goodfriend, 2007 for a description of this consensus). These economists are cited because they represent a shared sentiment, to highlight the irony of the confidence economists had in the accuracy and explanatory power of their theories and models, while at the same time economic circumstances were about to put into question so many aspects of these models. Our question is Krugman’s (2009) question: “How did economists get it so wrong?”

In this thesis we hope to untie the impossible knot by making more sense of this apparent contradiction. It is relatively easy but not very useful to put all the blame on ignorance of individual economists or economists as a group. It is more difficult but at the same time far more instructive to look for an explanation of the contradiction that is based on the plausible assumption that the average economists is both sincerely committed to finding the truth and capable of doing so (i.e. whatever we can say of economists, they were not ignorant slaves of a Wall Street neoliberal consensus). The question then becomes: how could sincere and capable scientists so seriously misjudge the merits of their work? And how could a whole field of research get caught in pursuing the wrong (i.e. not most relevant) research track? In this thesis a plausible interpretation of the developments in economics in the past few decades is proposed.

This interpretation is based in Thomas Kuhn’s description of scientific activity and scientific crises in his seminal work *The Structure of Scientific Revolutions* (1962 [1996]). The primary focus of this book are the natural sciences (Kuhn was originally schooled as a physicist), but social sciences can (and have been) be successfully analysed within this framework. The most obvious candidate for a scientific revolution on modern economics is the Keynesian revolution after the publication of the *General Theory* in 1936 (on this subject, see Coats, 1969, and Stanfield, 1974). Nevertheless, a one-on-one duplication of a description of the natural sciences to the social sciences is not straightforward (Kunin & Weaver, 1971), and Kuhn's description and interpretation of scientific activity is certainly not the only possibility, nor necessarily in every case the best (Bronfenbrenner, 1971, Blaug, 1975).

These ambiguities with regards to the merits of Kuhn's interpretation however are of relatively little importance to my thesis. I do not claim to make a case for the general applicability of his hypothesis to the social sciences or even just economics; nor do I pretend to offer the only possible interpretation. The humble claim I attempt to make is that Kuhn's *Structure of Scientific Revolutions* is one of the possible frameworks within which this particular episode of the history of economics can be interpreted; this interpretation, moreover, is (in my opinion) one that is illuminating. A short reflection in methodology is included to discuss some of the possible weaknesses of the chosen method. Yet I believe that the merits of this interpretation far outweigh the objections against it. Whether I am right in this regard is of course up to the reader of this work to judge for himself. And the extent to which the pattern I claim to have discovered can be generalized to economics as a whole is for others to decide.

## 2. Reflection on methodology

In this thesis an interpretation of the developments in economics before, during and after the Great Recession is proposed. This interpretation is based on the work of Thomas Kuhn. Although this is not an attempt to argue that Kuhn's conception of science is universally applicable, it is still helpful to reflect on the possible pitfalls of using Kuhn's ideas as an interpretative framework in economics. Such a reflection might help the reader to judge the merit of the interpretation developed in this thesis and could be considered as a 'disclaimer'.

Thomas Kuhn is one of the most influential philosophers of science of the 20th century, alongside Karl Popper and Imre Lakatos: "It is no more than conventional wisdom that Kuhn's account of scientific change (…) has had an extremely important influence on subsequent work" (Barnes, 2003, p. 123). His insistence on the complex process of scientific change (involving many factors) corrects the idea that science is strictly 'rational', i.e. merely a matter of weighing the pros and cons of scientific theories. Science is much more complex than such a simplistic picture of scientific practice would make us believe. Science is performed in a community which is guided by much more than 'objective rationality'. In this thesis we have used the idea that behaviour in a scientific community is highly complex to develop a way to understand the behaviour of the economic community. However, despite the importance of Kuhn's philosophy of science, his ideas are not flawless, and the flaws in his ideas may affect the analysis developed here.

Colander, Holt, & Rosser Jr. (2004) discuss 'the changing face' of macroeconomics in an article appearing before the Great Recession. The offer an alternative to a Kuhnian perception of scientific development in economics, "a modification of the standard view of paradigm shifts" (p. 488) that is more gradual. The authors argue that when members of the 'elite' (highly influential academic economists) are open to new ideas, these ideas will integrate into the profession, 'data set by data set'. Slowly but steadily the orthodoxy is changed. Contrary to Kuhn (according to the authors) changes often come from within and will go unnoticed for years, "stealth changes", "so gradual that the profession often does not notice that the change has occurred" (Colander, Holt, & Rosser Jr., The Changing Face of Mainstream Economics, 2004, p. 489). Changes such as these are made possible by work at 'the edge of economics', in areas like evolutionary game theory, psychological economics and complexity theory (Colander, Holt, & Rosser Jr., 2004, p. 496).

The authors legitimitely criticize Kuhn's focus on the conservativeness and monolithicness of scientific communities, which was partly meant as a reaction to the belief that scientists are constantly embracing new ideas and coming up with radical new theories. The determinism of Kuhn's thought (once a scientific community has embraced a paradigm, its course is more or less unalterable) is a weakness that may lead to a to strong emphasis on the suddenness of scientific revolutions and the inability of 'insiders' to initiate such a revolution. The one-sided focus on the conservativeness of scientific communities may make one overly pessimistic about the ability of mainstream economics to be self-corrective. The self-corrective possibilities of a scientific community might very well be larger than Kuhn thought.

On the other hand, David Colander (one of the authors of the article) has written another article serveral years later (Colander, et al., 2009) in which he speaks of the 'systemic failure of the economic profession'. His belief in the self-corrective power of economics seems to have disappeared during the Great Recession. It is thus possible that the self-corrective power of a scientific community does have its limits both in scope and in speed. Thus, with regard to the Great Recession, the changes in the economic community did not go far of fast enough, leading to serious gap in the theoretical toolbox of mainstream economics that might very well be called a 'systemic failure'. We can conclude that Kuhn might have ignored the possibility of 'gradual revolutions', but that these revolutions have their limits and at these limits Kuhn's relatively sudden revolutions enter into the picture. To which extent a gradual revolution will do in economics so that it can deal with phenomena such as the Great Recession (involving a complex financial sector) is up to now unclear. Mainstream economics has responded to the Great Recession, but is it enough?

In a much earlier paper, Kunin & Weaver (1971) comment on the possibility of applying the thesis advanced by Kuhn to economics and see two basic difficulties. The first difficulty concerns the looseness of the paradigm-concept, which we will discuss in parahgraph 2 of chapter 3. The question is: what is at stake in a scientific revolution? One particular idea (if one opts for a narrow definition of paradigm) or a complete worldview (if one opts for a broad, all-encompassing definition)? In answering this objection, the authors refer to an earlier paper by Bronfenbrenner, in which he introduced the idea of a synthesis (Bronfenbrenner, 1971). Rather than a radical overthrow of the old paradigm, a new paradigm is a synthesis of new elements and elements of the old paradigm. This allows for degrees of paradigm change and provides "both continuity and discontinuity within a unified model of developmental change" (Kunin & Weaver, 1971, p. 393). The Kuhnian catastrophic revolution is an extreme case in an array of continuity and discontinuity. The notion of 'degrees of revolution' resembles Colander, Holt, & Rosser Jr.'s (2004) notion of 'gradual revolutions'. In both cases the sudenness and radicalness of Kuhnian revolutions is criticized and questioned. It is helpful to think of Kuhnian revolutions as an extreme on a broad scale. This allows for openness towards the innovative forces within a paradigm. So, as was said above, we should probably be a bit more optimistic about changes *within* paradigms.

The second difficulty concerns the transfer of a philosophy of the natural sciences (which was the primary focus of Kuhn) to the social sciences, in this case economics. The difference lies, according to the authors, in the fact that economics is a historical science. It deals with an object of study that undergoes changes over time: the economy of the 1950s is not the same as the economy of the 2000s. This means that economic research is not only vulnerable to anomalies resulting from internal dynamics (e.g. anomolous discoveries) but also to anomolies resulting from external changes, namely changes in the object of study, the economy. We could say that paradigms in the social sciences are time-bound: what may be right at one moment in time may be incorrect a decade later. This adds another dimension to the explanation we have given for the behaviour of the economic community: it might be so that the 'old' paradigm did a very good job at explaining the 'old' economy. Such a thesis is supported by the fact that one of the major difficulties economists experienced after the Great Recession was understanding the complexity of the financial sector – a sector that had seen an unbelievable development in one or two decades. A more historically minded research might come to the conclusion that the paradigm simply could not keep up with the external changes. This does not invalidate our interpretation, but merely adds another explanatory layer, namely external changes as invalidating the paradigm.

To summarize the criticism: Kuhn's story (at least such as it was originially written and received – Kuhn has in later writings softened the radical edges of his ideas, as we will see in our discussion of inter-paradigm communication in chapter 7) is exaggerated, as Kuhn himself has conceded (Hausman, 1994, p. 200). To assess to which extent the weaknesses of Kuhn's account have had an influence on the validity of the interpretation of this thesis, we have to know to which extent Kuhn's exaggeration has influenced this interpretation. I concede that the picture of economics that is sketched below may be too monolothic, i.e. more attention could have been given to the way in which mainstream economics was and is open to research at the edges of economics. Moreover, the self-corrective potentiality of paradigms might have remained under the radar in this thesis: much work is done now to implement an accurate description of the financial sector in mainstream models.

Be that as it may, as a defense it could be argued that, to make one's point, one has to exaggerate a bit (as Kuhn did when writing his book). In order to point out the blind spots of the economic community due to its commitment to a particular paradigm, one sometimes has to ignore (for clarity's sake) the nuances. Moreover, the extent to which these nuances really had an influence on the mainstream is questionable, given its poor performance during the Great Recession. Whether the mainstream paradigm is self-corrective is a matter that can ultimately only be resolved by time – but the current developments in economics do show that a paradigm is more flexible than we sometimes (in our gloomiest moments) think.

## 3. Normal Science

### *3.1. Ordinary science*

This thesis is an attempt to give an interpretation of scientific activity in the economic community that allows us to make sense of the unexpected crisis within this community after the GFC shattered so many illusions. In order to get there, however, we first have to form ourselves an image of what scientific activity actually is – in order to answer the question how this activity could have led to an unjustified confidence in scientific achievements among economists. Before we can understand the extraordinary (the Great Financial Crisis as an extreme outlier, i.e. one of those rare events that questions the cherished interpretation of the world) we first have to understand the ordinary, the daily routine of economists.

Even to speak of scientific activity as 'ordinary', as a 'daily routine' counters a commonly held misconception of science as a succession of spectacular discoveries, and subsequently as the scientist as a wildly creative thinker, who constantly challenges the status quo (what Kuhn would call a 'divergent thinker', see Kuhn (1977a)). This misconception is understandable, as those spectacular discoveries get the most attention in 'popular' histories of science. Ask the average layman about science, and he comes up with a story about Newton and Einstein.

It cannot be denied that spectacular discoveries are an essential element of science, but by exclusively focussing on this element, another important aspect of science is forgotten. It is actually this second aspect of science that is the sole preoccupation of most scientists (those who do not make these spectacular discoveries) and that is also the fertile soil for the more striking discoveries. This second aspect is thus actually the more fundamental aspect of science, both because it is what most scientists are preoccupied with most of the time and because it is here that revolutionary discoveries originate. Normal science is less spectacular and more conservative than we might think (scientists are to a large extent 'convergent thinkers', according to Kuhn (1977a)). Scientists try to preserve the old, rather than discovering the new.

### *3.2. What is a paradigm?*

Kuhn famously uses the term 'paradigm' to describe that which ties the scientific community together, that which the community cherishes and tries to conserve and elaborate upon. The concept 'paradigm' is notoriously ill-defined in *The Structure of Scientific Revolutions*[1], with

[1] Kuhn, T. S. (1996 (1962)). *The Structure of Scientific Revolutions (Third Edition).* Chicago: The University of Chicago Press.

one commentator (Masterman (1970)) finding no less than 21 definitions in Kuhn's own book, ranging from 'a universally recognized scientific achievement' to 'a set of political institutions'! Which of these meanings should we follow? Is a paradigm merely a particularly influential example, or is it an all-encompassing set of rules and tools one is obliged to use if one wants to be taken seriously as a scientist?

Obviously this is not the occasion to mingle in the debate on what exactly is a paradigm, and yet we have to have at least a provisional idea of what a paradigm is. In a reflection 15 years after the publication of *The Structure*, Kuhn admits that the initial meaning of the word 'paradigm' (which originally meant something like an 'exemplary case') had gone lost in his gradual expansion of the term. Yet Kuhn makes two remarks which are important for us now. First, he stresses the fact that a paradigm cannot be seen apart from the scientific community which is gathered around it (Kuhn, 1977c, p. 294); second, he points out that shared examples (the original meaning of the word paradigm) "can serve cognitive functions commonly attributed to shared rules", such that it may eventually shape a whole set of commitments of a scientific community (what is considered 'scientific': methodology, tools, criteria for selecting problems and solutions, etc.), which he would now have called 'disciplinary matrix'.

Those two points can serve as a point of departure for our analysis of economics and its 'paradigm'. We have to keep in mind that a paradigm is 'embodied' in a scientific community, while the other way around a scientific community is only community by virtue of a 'paradigm' (however one initially defines it) that is shared. And considering the second point: it is often true that a scientific community is originally gathered around a 'shared example'; this example, however, may gradually bring forth elaborate sets of criteria for belonging to that particular community (e.g. certain methodological standards). Take an example from economics: Keynesianism. Initially, it was based on a paradigm in the original sense of the word: Keynes's *General Theory*. Over time this was expanded, until Keynesianism designated a particular view on economics, including a methodology, institutions, journals, political commitments, etc. It is important to keep in mind that it is difficult to tell which of these two is the 'paradigm' – the borders are blurry – but that one constant identifying mark is the community-forming character, and the way in which this community was drawn around a common way of looking at the world, whether through using an exemplary case, or through using the same methodology and being aligned to the same institutions. What remains the same is the community that shares a worldview by virtue of its 'paradigm'. As a consequence, we will from now on investigate how the economic community's worldview was shaped by its 'paradigm', which may encompass a broad array of elements (its 'disciplinary matrix').

*3.3. The emergence of a paradigm*

Normal science is defined by being paradigmatic, so that Kuhn can define it as "research firmly based upon one or more past scientific achievements that some particular scientific community acknowledges for some time as supplying the foundation for its further practice" (Kuhn, 1996 (1962), p. 10). The original example starts the research performed in the community gathered around it. Of course, not every scientific achievement is potentially paradigmatic, and over the course of time scientists will no longer consciously base all their research on this one example (though this example has shaped the tradition within which the scientist is grounded). A paradigmatic scientific achievement is 1) sufficiently unprecedented as to be able to attract an enduring group of adherents willing to base their research on this achievement and 2) sufficiently open-ended such that it leaves all kinds of question to be solved in the future (otherwise its adherents would have nothing to do). Take again Keynesianism: Keynes's analysis of business cycles was both revolutionary and open-ended (such that today still researchers claim to develop a Keynesian understanding of the economy).

A mature scientific discipline typically has only one paradigm at a time which defines the field (its problems, methodology, etc.), though in very rare cases two paradigms exist side-by-side (Kuhn, 1996 (1962), p. xi); at the margins there are some dissenters, who are ignored or clustered together in an alternative group (in economics, we know small sub-groups: Austrians, Post-Keynesians, Marxists, etc.). The dominant paradigm has emerged out of a battle with other paradigms in a pre-paradigmatic phase – other paradigms which may be as 'scientific' as the victorious one (methodologically) but differentiated by their "incommensurable ways of seeing the world and practicing science in it" (Kuhn, 1996 (1962), p. 4). These other paradigms will gradually disappear because its adherents join the community gathered around the dominant paradigm and those who remain are ignored. Note that the dominant paradigm is not perfect: it does not solve all the problems. It 'merely' is considered superior to the others in the eyes of the scientific community, for example by solving a very pressing problem (Kuhn, 1996 (1962), pp. 18,19).

*3.4. Science within a paradigm*

Why would a scientific community restrict itself by committing itself to only one paradigm, ignoring other (no less scientific, probably fruitful) alternatives? Well, because significant benefits are endowed on those who decide to stick to one paradigm. By agreeing with a certain way of practicing science, scientists avoid tiresome discussions about the fundamentals of science; these are, as Kuhn remarks, almost absent in the daily routines of normal science. Being committed to the same paradigm, scientists reduce the likelihood of disagreement about fundamentals, for example questions of methodology (Kuhn, 1996 (1962), p. 11). Now that scientists no longer have to worry about these distracting questions, the focus can shift to more specialized and more esoteric problems which are not yet solved. One can even see this shift in the mode of scientific publication: scientists no longer publish whole books (as Newton did, or Keynes) but publish short articles in specialized journals, both intellectually and logistically inaccessible for the specialized layman (Kuhn, 1996 (1962), p. 20). In economics, this shift is also perceptible, though leading economists still publish books (yet often not on pure science, but on the borders of the political); and the specialization is for example visible in the mathematization of economics.

Though a paradigm serves as an example for an entire field of scientific research, it is also open-ended, with many loose ends; this was precisely what made it such a good paradigm. Paradigmatic science is initially ill-defined, with many unsolved problems. The goal of normal science is to provide further articulation and specification of the paradigm: normal science is the actualization of the promise contained in the original paradigm, an actualization that may be done in various ways; from simply extending the number of relevant facts known to improving the match between those facts and the paradigm's predictions (e.g. fine-tuning existing models or creating new models) – all the way to the further articulation of the paradigm by way of theoretical advancements.

This does not sound very spectacular: it is not the composition of new symphonies, merely the creation of variations on a theme. Kuhn calls it "mop-up work" (Kuhn, 1996 (1962), p. 24), which is necessary because the open-endedness of the paradigmatic discovery leaves a lot of work to be done. Science is not the open-minded exploration of the world: "[It] seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies", says Kuhn (1996 (1962, p. 24). Even worse: scientists are not that interested in new phenomena. Those that cannot be fitted into the box are ignored or not seen at all. Scientists are also not interested in new theories, up to the point of being hostile – they are too busy fine-tuning the ‘old’ theories. While this might, from a science-as-revolutionary point of view, be considered a major weakness, it is actually essential to scientific development. Without the (self-imposed) restrictions of a paradigm, a scientific community would get stuck in endless discussion over fundamentals without making any actual progress. By focussing on a small range of problems – those provided by the paradigm – a scientific community investigates nature with a depth otherwise impossible. Normal science is small, but deep.

Within the confines of paradigmatic science, fact-gathering is limited to those facts relevant to the paradigm. For example because these, according to the paradigm, reveal the nature of things; or because they can be compared to predictions or help to articulate the paradigm, e.g. discovering quantitative laws. Fact-gathering may lead to theoretical developments, further expanding the theoretical foundations of the paradigm. The result is a more precisely defined paradigm with fewer ambiguities (Kuhn, 1996 (1962), pp. 25-28, 34).

Science, at least according to Kuhn, is conservative. The goal is not to perceive major novelties, factual or theoretical. No, normal science is heavily prejudiced towards the familiar and the known, that which can be made sense of within the familiar framework. It is revealing how a scientific community ordinarily deals with outcomes lying outside the familiar range. These outcomes are regarded as research failures, reflecting badly on the researcher rather than on the paradigm; or they remain mere facts, without further significance (Kuhn, 1996 (1962), p. 35). Failure to solve a scientific puzzle just discredits the scientist (later we will modify this slightly: persistent or deep failure might discredit a paradigm).

Why would anyone get excited about mopping-up someone other’s mess? Why would you devote your life to the further articulation of someone else’s idea, with only a very small change of making a revolutionary discovery yourself? Kuhn answers that, essentially, the scientist is a puzzle solver, driven by the challenge of the puzzle. It is not the outcome of the puzzle that is interesting – oftentimes the outcome is more or less known – but the solving itself. It is about the road rather than the destination: “bringing a normal research programme to a conclusion is achieving the anticipated in a new way” (Kuhn, 1996 (1962), p. 36). It is only within the confines of a scientific community gathered around a common paradigm that the scientist can be a puzzle solver, for it is the paradigmatic that determines which problems are worthy and probable to be solved. Scientists do not randomly pick a problem without any clue about a possible solution; rather, they know the solution can probably be found in what the paradigm provides, and they know it is a significant problem because the paradigm tells them it is. ‘Being allowed by the paradigm’ is the most important consideration when determining the relevance of a problem, trumping most other criteria. Even socially important problems can be ignored if they cannot be stated in the conceptual and instrumental tools of a scientific community, i.e. if they are neither relevant nor solvable according to the reigning paradigm, as is the case in some of the social sciences according to Kuhn (Kuhn, 1996 (1962), p. 37).

*3.5. Paradigm and perception*

Thus far, we have discussed how a paradigm directs a scientific community on a certain path, but we have not yet spoken about the way in which it influences what scientists see when they look at the world (apart from a reference to facts that are dismissed or ignored). Perception is not neutral: we always see the world in a certain way. Of course, the world as it is, is simply a given, but the way in which that given world is perceived may differ from one person to another. Paradigms are an important factor in this perceptual process, and thus paradigms are not only constitutive of science, but also of the world as it is perceived by us (Kuhn, 1996 (1962), p. 110). We may, according to Kuhn, even go as far as to say that, after the adoption of a different paradigm, the scientific community responds to a different world. Compare this, analogically, to the famous duck-rabbit *Gestalt*-switch, a picture which shows either a rabbit or a duck, but never both at the same time. Paradigms are a prerequisite for perception itself: there is no such thing as non-paradigmatic perception: we simply *always* have to make sense of the world in one or the other way such that what we see is determined both by what we look at (the world) and the ‘lenses’ through which we look at the world. Ultimately, there is no higher, neutral, authority (something like non-paradigmatic sense-data) to which a scientists could appeal to confirm his or her vision (Kuhn, 1996 (1962), pp. 111-114).

So, while there is an outside world which is there for all of us, and the same for all of us – in the absence of a neutral observer with a god-like perspective it makes no sense at all to refer to that world to confirm our own vision (as if ‘we’ would see the world ‘as it is’ while the others are misguided by their prejudices). So, while one could attempt to explain the perceptual changes that accompany a paradigm-change by appealing to the way in which we interpret a stable set of data differently (i.e. we see the same, but we give a different interpretation), Kuhn rejects this attempt. The perceptual disagreement between two people working in different paradigms is not wholly reducible to interpretative disagreements between the two (Kuhn, 1996 (1962), pp. 120, 121). The disagreement is more profound: the two of them see different things: one of them a rabbit, the other a duck. Neither of them can refer to a shared perception in order to convince the other. The world as the scientific community perceives it is profoundly affected by its paradigm. Hence it is extremely difficult for such a community to think ‘out of the box’, to take a critical stance towards its paradigm. Paradigmatic scientific activity is predisposed towards confirming and articulating a paradigm: not because scientists are bigots, but because the impact of the paradigm on perception makes it extremely difficult to criticize it once you are ‘in’ it. Kuhn draws the rather extreme conclusion that paradigms are not corrigible by normal science (Kuhn, 1996 (1962), p. 122). We do not necessarily have to follow this radical conclusion. However, the perceptual bias of normal science (which is arguably there) explains the stubbornness of paradigms.

The considerable impact of certain predispositions in scientific communities, not only on scientific activity, but also on the world perceived, underlines the importance of conceptualizing paradigms in order to identify ‘blind spots’, biases in scientific perception. Kuhn was not the first one to point out how difficult it is to criticize one’s own deeply held convictions because of their impact on what we see. Another philosopher of science, Michael Polanyi, had some interesting thoughts about the relationship between worldview and perception, for example in his article *The Stability of Beliefs* (1952). Important for us is the impact of implicitly held beliefs, “by reliance on a particular conceptual framework by which all experience is interpreted” (Polanyi, 1952, p. 217), in contrast to explicitly held beliefs (consciously, e.g. some written-down manifesto). While paradigms may not be as all-encompassing as Polanyi’s ‘interpretative frameworks’ (though this depends also on the definition of paradigm one chooses), paradigms at least resemble interpretative frameworks to a lesser degree: both are about the way in which we see the world. Therefore we can say that paradigms function as an interpretative framework for a scientific community, though the extent to which they do this is open for debate.

Interpretative frameworks have a high adhesive power and can absorb a large amount of shocks before they are dismissed. They are extremely flexible and can thus accommodate almost everything and be disproved by almost nothing (Polanyi gives us the examples of Freudianism and Marxism, which can in principle explain everything in their own favour). Compare this with paradigms, which are also quite resilient. Interpretative frameworks can be embodied in something as basic as language. The sincere and confident use of a certain language or idiom reflects adhesion to a particular interpretative framework (someone sincerely using the worlds ‘witchcraft’ and ‘oracle’ inhabits an enchanted world; someone speaking of 'the invisible hand' and 'efficient markets' inhabits a capitalist world; etc.) Polanyi discusses the primitive African Azande tribe. Though having no formal doctrine forcing them to believe in witchcraft, their beliefs are all the more firmly held by being part of their daily language, enabling them to interpret all relevant phenomena in terms of witchcraft and oracular utterances; their framework is resilient up to the point of being able to accommodate contradictory phenomena. Being capable only of speaking their own language and using their own idiom, the Azande are not capable to step outside of this framework to question it from the outside. In the absence of a different language they have no other option than to stretch the boundaries of the familiar framework. Can we call them unreasonable? Polanyi thinks not, for they are capable of reasoning excellently; but only within their own framework.

Polanyi argues that, once we are embedded in an interpretative framework, we do not have the capacity to reason against it – for we can only speak the language of that framework. Kuhn's conviction that normal science cannot correct itself (i.e. the paradigm which it tries to articulate) closely resembles this idea. A scientific community cannot employ the tools of a paradigm to criticize the same paradigm: it is like trying to criticize the grammar of a language – in order to do so one has to employ these rules, and so never escapes them. Of course, paradigms in most cases are not as pervasive as a language, but to a lesser extent the dynamics are the same: scientists are embedded in the idiom of their own worldview, and this limits their capacity to criticize it.

In the next chapter, the theoretical framework developed in this chapter will be applied to economics. The paradigm of mainstream economics will be analysed to see what influence it had on the research of economists. The different aspects of paradigmatic science (its conservativeness, how it narrows the scope of research, how it influences the interpretation of the world, etc.) are found in this paradigm in economics.  In the subsequent chapters, we will further discuss how the GFC relates to this paradigm, and what this can tell us about (the future of) economics.

# 4. A Paradigm in Economics

## *4.1. Introduction*

The previous chapter taught us that scientific communities are conservative. Once a paradigm is embraced other possibilities are out of the picture: All the energy is directed at the articulation of the dominant paradigm, while facts and theories irrelevant to this enterprise are ignored. Though paradigms foster the depth of scientific investigation, at the same time the scope of investigation is seriously narrowed. These conclusions make the articulation of the dominant paradigm in late 20$^{th}$ and early 21$^{st}$ century economics all the more relevant to our problem. After all, the lack of interest in phenomena relevant to the GFC betrays the existence of blinds spots in economics, blind spots that may very well have been caused by the limits imposed on scientific investigation by the dominant paradigm.

Of course there are disagreements between mainstream economists. Yet the literature suggests that a core of agreement can be identified beneath the surface of disagreement. We read about a ‘general consensus’ (Kirman, 2010, p. 500), ‘a single model that came to dominate’ (Stiglitz, 2011, p. 593), ‘a working principle on the core principles of monetary policy’ (Goodfriend, 2007, p. 48), ‘convergence’ (Quiggin, 2011, p. 355), and of a ‘core’ against a ‘periphery’ (Caballero, 2010). In this chapter we will try to pin down this consensus by identifying three characteristic elements that together form a paradigm. These three elements are:

1. Dynamic Stochastic General Equilibrium (DSGE) approach to modelling the economy.
2. A description of financial markets based on the Efficient Market Hypothesis (EMH).
3. The idea that we came to the end of the business cycle, a.k.a. the Great Moderation.

The first two elements are theoretical; the third has to do with the perception of the world, as described in paragraph 5 of chapter 3.

## *4.2. The standard model: DSGE*

There are many variants of the DSGE model, yet they all share the same basic structure. Olivier Blanchard (2008) describes it succinctly: “A macroeconomic article today often follows strict, haiku-like rules. It starts from a general equilibrium structure, in which individuals maximize their value, and markets clear. Then, it introduces a twist, be it an imperfection or the closing of a particular set of markets, and works out the equilibrium implications. It then performs a numerical simulation based on calibration, showing that the model performs well. It ends with a welfare assessment". The basics of the DSGE are thus relatively straightforward: general equilibrium, a utility-maximizing (rational) representative individual/household/agent, and a twist. This structure is a starting point for the majority of modern macroeconomics. It allows for an endless number of variations, depending on the chosen variables (consumer preferences, which 'twist', etc.). But the basic structure is the same.

A widely discussed element of the DSGE model – in the aftermath of the GFC – is the representative agent. The representative agent was introduced in macroeconomic models to underpin these models with microeconomic foundations. Microeconomic foundations are a methodological cornerstone of the DSGE model: macroeconomic behaviour has to be derivable from microeconomic behaviour, it is argued. It is obvious that it matters a lot which microfoundations are chosen. So in order to understand the DSGE models and the criticism against it, I will now elaborate somewhat more on these microfoundations.

The basis of the DSGE model is a "single immortal consumer-worker-owner [who] maximizes a perfectly conventional time-additive utility function over an infinite horizon, under perfect foresight or rational expectations" (Solow, 2008, p. 243), a consumer-worker-owner who is moreover perfectly rational, thus capable of flawlessly maximizing his utility function. The assumption of this representative individual with these capacities has enormous implications, not in the least with regards to the GFC.

First, the agent as *representative*, i.e. the single agent represents the multiplicity of agents in the real economy. Thus the world that is peopled with savers, borrowers, workers, pensioners, and so on, with different desires, preferences and beliefs, is in the model simplified into one representative agent. This means that, in those models, many relevant real-world issues simply could not occur. For example: information asymmetry. In the absence of more than one agent, it is logically impossible for one agent to know more than the other. Information asymmetry is one of the main reasons for the existence of financial markets. Banks serve as intermediaries between lenders and borrowers, for example by checking the creditworthiness of borrowers on behalf of lenders (who are initially ignorant about this). Not to mention the fact that with only one agent there cannot even be a lender and a borrower in the first place! Among other relevant aspects that cannot be understood accurately in those models are the implications of unemployment. The representative individual at most reduces his labour supply and his ability to smooth consumption out over time reduces the impact even further.

Yet in the real world a reduction in labor demand is not evenly distributed, and the shift from a full-time job to unemployment has a huge impact on the individual worker – and his capacity to pay his debts, to mention just one aspect relevant during the GFC (Stiglitz, 2011, pp. 598, 599).

A second relevant aspect of the microfoundations is the assumption of rationality on the part of the representative agent, or the Rational Expectations Hypothesis (REH). John Muth, in a seminal paper on rational expectations, defines them as "essentially the same as the predictions of the relevant economic theory" (Muth, 1961, p. 316). Thus a model based on the REH states that the expectations about the future of the representative agent are the same as the expectations derived from the model. Such a model ignores the myriad ways in which expectations of real agents may differ from those of the model (e.g. due to imperfect information or various psychological idiosyncrasies – see also paragraph 2.3.) Colander, et al. (2009) discuss the problematic implications of this assumption. It ignores that market particpants' own models of markets, the ways in which these deviate from *the* model. It implies that everyone in the economy behaves as if he had a complete understanding of the economy – an implication that is far removed from the real world (Colander, et al., 2009, p. 256).

Models are simplifications, but we may question the extent to which simplification has been at the expense of accuracy when it comes to the assumption of the rational representative agent. Hence the doubts of Robert Solow: "To ignore all this [the differences between individuals] in principle does not seem to qualify as mere abstraction – that is setting aside inessential details. It seems more like the arbitrary suppression of clues merely because they are inconvenient for cherished preconceptions" (Solow, 2008, p. 244).

These highly idealized models still had to account for the existence of economic fluctuations. In order to do so, the 'twist', about which Blanchard (2008) speaks, is introduced. Deviations from equilibrium levels of demand and unemployment are presumably caused by so-called 'autocorrelated shocks' (a change in preferences, labour productivity, etc.). Autocorrelation accounts for the longevity of fluctiations: the effects of a shock in period 1 persists in period 2. The economy does not return to its equilibrium immediately. Such autocorrelation is caused by market imperfections, for example wage stickiness: wages do not adjust immediately to changing circumstances (e.g. because of longer-term agreements with labour unions). This reduces the flexibility of the economy. The shocks are exogenous, i.e. not explained within the model. As a result, DSGE models do not really explain business cycles, but generate them with a 'deus-ex-machina mechanism'. (Fagiolo & Roventini, 2012, p. 86). Besides the disturbing prospect of a macroeconomic model that does not explain business cycles, it is also worrying that this misses the essence of the GFC: "It is hard to explain in a plausible manner this crisis—or most other major downturns—in terms of exogenous shocks to an economy which in the absence of such shocks would have grown smoothly. This crisis, like most major preceding ones, is man-made: the economic system itself created a bubble, the inevitable bursting of which led to the recession" (Stiglitz, 2011, p. 610).

### *4.3. Financial markets: the EMH*

Whereas most macroeconomic models had nothing substantial to say about financial markets, economists did investigate the dynamics on financial markets and the creation of asset prices. The leading research programme in this field is based on the efficient market hypothesis (EMH). It has been severely criticized and singled out as one of the factors leading to the Great Recession by stimulating irresponsible behaviour of market participants (Ball, 1).

What is the EMH? The EMH combines two basic insights: the first is that competition enforces a balance between revenues and costs. Excessive profits are impossible because new entry will reduce or eliminate them. The second insight is that asset prices are a function of information-flows to the market place. These two insights lead to the basic intuition of the EMH: information does not lead to profits; all information is put into the asset price. "Competition among market participants causes the return from using information to be commensurate with its cost" (Ball, 2009, p. 9). This means that publicly available information (which is free) does not yield profits, for it is already reflected in asset prices. Hence, asset prices are a reflection of all publicly available information.

One of the claims made by critics of the EMH, who link it to the Great Recession, is that it led market participants to irresponsible behaviour: Since they knew (because of the EMH) that all information is reflected in prices, they did not have an incentive to gather information themselves – why bother if it is already there, in the price? However, according to Ball this in incorrect. The EMH is a statement about the outcome of the market participant's behaviour, including obtaining information: "The misunderstanding arises from confusing efficiency as a statement about the equilibrium resulting from investors' actions with the actions themselves" (Ball, 2009, p. 10). Neither does the collapse of large financial institutions indicate that markets are inefficient – on the contrary, it shows that even size will not protect you from the forces of competition (Ball, 2009, p. 11).

Another objection against the EMH is that it led to a laxity on the part of market supervisors (Ball, 2009, p. 11), for they relied on the market, convinced that the market does a good job at correcting itself. However, it market supervisors had followed the EMH, they would have been more sceptical about the exceptionally high returns of some large financial institutions. In competitive markets these returns are probably attributable to high leverage, high risk, inside information (rather than public information) or dishonest accounting. In short, supervisors would have been triggered to investigate the source of these profits (Ball, 2009, p. 11).

*4.4. The weakness of mainstream economics*

The weakness of the EMH is not so much what it does say, but what it does not say. Or, rather, the EMH on itself is not enough to understand financial markets; it describes only a part of what is going on. Thus on its own it falls short as a description of financial markets. The EMH says nothing about the 'supply side' of the information exchange. It only says that, given the information that is supplied, the gains from public information are zero. It does not take into account the quality or amount of information that is available. Thus financial markets might rely on partial or incorrect information and still be efficient in that this information is entirely reflected by asset prices. So the extremely complex financial products that were developed before the Great Recession might have been efficiently priced, given the information available. But the available information was very limited, given the complexity of the product. Furthermore, the EMH models information as an objective commodity, the same for all investors. Yet in reality investors have different information, respond to it differently and hold various beliefs about the state of the world, or about the beliefs of others. Thus information is at least partly subjective, expectations are not necessarily in line with those of the model (Ball, 2009, p. 13; see also paragraph 4.2. above). The subjectivities and irrationalities of behaviour on financial markets is the object of study of behavioural finance, which incorporates insights from psychology to understand and model behaviour.

Thus, while the EMH has offered substantial insights into the dynamics of financial markets and asset prices, it does have its blind spots. If it is taken as a comprehensive description of financial markets (which it is not) it offers a picture of market participants as objective, rational 'computers' that are constantly processing information; and thus a picture of financial markets as ‘almost-always-correct’. Yet this picture ignores that imperfect supply of information, an imperfection that worsened as financial markets became more complex; and it ignores the fact that information-processing is subjective; each individual responds differently to the same piece of information, due to all kinds of ‘irrationalities’ and individual characteristics such as risk preferences, which are described in behavioural finance literature.

Likewise, the weakness of the DSGE model is not so much in what it does say, as in what it does not say. The problem of mainstream macroeconomics was not the DSGE model per se, but the fact that it restricted itself solely to DSGE models based on a representative agent. As Colander (2010) remarks, the DSGE model was an advance on previous macroeconomic models, by including forward looking individuals. The problem was that mainstream economics did not go beyond the DSGE model towards more complex models (Colander, 2010, p. 421).

### *4.5. Mainstream economics as a paradigm*

As a paradigm, mainstream economics was a consensus on fundamentals. One of the main functions of a paradigm, according to Kuhn, is to end the potentially endless discussion about those fundamentals, so that scientists can focus on solutions to questions that actually foster scientific development. And indeed, within mainstream economics a discussion about some fundamental issues did not have a high urgency. We have seen how DSGE models were based on microeconomic foundations that were empirically inaccurate. Yet there was no urgency to ‘update’ the models and go beyond them.

Mainstream macroeconomics has thus proven to be relatively immune to criticism (up until the GFC). Ricardo Caballero usefully distinguishes a ‘core’ and a ‘periphery’ in macroeconomics (Caballero, 2010), the core being the mainstream, the periphery all other, often less systematized, insights. Divergent literature that could have contributed to the development of economics was systematically ignored by ‘core economists’. Peripheral economists were indeed “swimming against the tide, unable to make much headway against a pervasive and, in retrospect, foolish complacency” (Krugman, How Did Economists Get It So Wrong?, 2009). There was a substantial body of literature criticizing this ‘foolish complacency’, taking serious the issue of financial instability, yet “the lines of discourse that take up these questions have been marginalized, shunted to the sidelines within academic economics” (Galbraith, 2009, p. 87). While peripheral economics could have been making a valuable contribution to the core, mainstream macroeconomic research was in what Caballero

(2010, p. 85) calls ‘the fine-tuning mode of the dynamic stochastic equilibrium mode’. Even flat-out empirical contradictions of the model could not disprove it: “Dramatic differences between the model’s behavior and empirical data are not taken as evidence against the model’s underlying axioms” (Colander, et al., 2009, p. 260).

Kuhn’s concept of scientific paradigms is the key to untie the Gordian knot of economics: why did economists ignore counterevidence and alternative theories and cling to a ‘flawed’ model? Why did they not go further? With the knowledge of paradigms in the back of our head, this is relatively easy to understand. Economists had little or no use for a new discussion about microeconomic foundations, or alternative theories and contradictory facts. As long as a true ‘anomaly’ (a phenomenon we will discuss in the next chapter) fails to occur, the paradigm suffices. Meanwhile, economists held fast to what was in their paradigm: the ‘old’ microeconomic foundations of a utility maximizing, rational representative household, a DSGE model with shocks and frictions (but without a serious financial sector), and the EMH. The relative simplicity of the model is one of its strengths, and thus in the absence of a compelling counterargument, macroeconomists had no reason to add various complexities to their model. And, with education mostly aimed at training convergent rather than divergent thinkers (see Mirowski (2010) on the absence of history and philosophy in the average economics curriculum) we should not expect economists to ‘think out of the box’ if they are not required to do so. This is not meant as criticism, or to mark economics as a ‘pseudo-science’. On the contrary: the behavior of economists can only be called ‘scientific’ according to Kuhnian standards. Economists were convergent thinkers, desiring to develop the mainstream macroeconomic paradigm. The fact that they added relatively little importance to theoretical innovation or counterfactuals fits this picture.

A similar line of argument is developed by Colander (2010). The problem was not that economists did not recognize the problems, but that there were little to no incentives to do something about them, to study a wide variety of models. Even though Colander does not formulate it as such, he clearly perceives the pressure to stick to the paradigm: “Too many macroeconomists felt that if they did not toe the DSGE line, they were unlikely to be published in journals that would lead to their advancement. The result was that they did not have an incentive to explore alternative models to anywhere near the degree that would have made sense to a neutral observer with educated common sense” and “the belief was that academic economists who introduced models that were complex enough to incorporate the possibility of crises would find that these models were difficult to publish in top journals because those journals were committed to a certain type of so-called micro-grounded DSGE models as the only legitimate approach" (Colander, 2010, p. 420).

Macroeconomists were solving puzzles. Confronted with a problem (say, the business cycle) they came up with a solution within the confines of the paradigm (e.g. autocorrelated shocks, frictions). It was simply a matter of a puzzle (the business cycle), a paradigm (the DSGE model) and a solution (shocks). Some – socially relevant – issues could not be part of the paradigm: the complexities of the financial sector were not a major component of the mainstream research programme; and thus economists spend relatively little time thinking about this issue (Borio, 2014, p. 182; Colander, et al., 2009, p. 264). Or, as Krugman (2009) puts it provocatively: "this romanticized and sanitized version of the economy led most economists to ignore all the things that can go wrong". But is this a problem? In one sense, it is, because some of the issues ignored by economists became crucially important. But in another sense, it is not: economists were not 'unscientific'. And thus we might have to conclude that this is simply part and parcel of paradigmatic science. This does not mean that behavior of economists cannot be the subject of profound criticism. For example: can a scientific community that claims to value microeconomic foundations be satisfied when these foundations are outdated? And was there really no room, within the mainstream, to develop more profound insights into the complexities of a modern financial sector? But this is not necessarily a problem for economists as such, but probably for all scientists working in a paradigm. It is about finding a balance between convergent and divergent thinking.

### *4.6. The worldview of mainstream economics: the Great Moderation*

The third relevant aspect of the mainstream paradigm concerns the way in which it influenced perception of economists, their worldview. Many economists, at the beginning of the 21$^{st}$ century, saw a Great Moderation. Economists do not just come up with new theoretical inventions. Interpreting the world (the economy) is part and parcel of doing economics. Economists tell a story about economic development over time. The Great Moderation was – before the crisis – the latest episode in this story.

At the borders of the 20$^{th}$ and the 21$^{st}$ century, the economy had experienced a prolonged period of relative stability and low volatility, as is shown by Blanchard & Simon (2001). These authors concluded that the period of low output volatility (a standard measure of economic stability) had started already in the 1950s, with interruptions in the 1970s and early 1980s. Thus, in the early 2000s economists had witnessed over two decades of economic growth, with only some minor drops (two recessions in 1990-1991 and 2001, both lasting for only eight months). Many economists were convinced that this marked the end of the business cycle, and Bernanke (2004), then president of the FED, popularized the term 'Great Moderation' (which already existed in the literature).

This phenomenon is open to three kinds of explanations (Bernanke, 2004). The first refers to structural changes in the economy, for example the development of important institutions (a more powerful central bank, sophisticated financial markets, etc.) or technology (e.g. the internet, computer technology). Better performance of macroeconomic policies is another possible explanation. It could be argued that the central bank had reduced inflation volatility, which fosters economic stability. The third explanation is that it is simply a matter of good fortune in the absence of large shocks. Even though Bernanke believed there was indeed a certain degree of luck involved, he also believed that the first two factors (structural changes and improved policy) were at least as influential. This means that, given these improvements, the likelihood of an economic crisis was now significantly lower than in previous decades. Economists were thus (justifiably) optimistic about the future.

The eagerness with which economists believed in the end of the business cycle is an example of the human habit to believe that 'this time is different'. The influential economist Irving Fisher reportedly said in October 1929, just before the Great Depression, that "stock prices have reached what looks like a permanently high plateau" (Quiggin, 2010, p. 5). The 1990s and the early 2000s were an era of unbounded optimism: the cold war had ended and Francis Fukuyama prophesied 'the end of history', the victory of capitalist liberal democracy. Economists were influenced by this optimism and moreover had statistical evidence to back them up: "the economy of the 1990s suggested to that generation of students that the business cycle was no longer of great practical importance" (Mankiw, 2006, p. 12). Again, questions rise up immediately: what influence did the paradigm of mainstream economics have on the judgement of economists about the Great Moderation? And how did this interpretation backfire onto theoretical developments in economics and the ability of economists to take a critical stance towards these developments?[2]

### *4.7. The Great Moderation and the paradigm*

Paradigms (or, more in general, that which we know or believe to be true) predispose us to see the world in one way rather than another. Joseph Stiglitz (2011) compares economic theory

[2] On the effects of social mood on investment and business activity, see Nofsinger (2005).

with blinders: it points us in a certain direction, allows us to see things but remain blind to other things (Stiglitz, 2011, p. 594). Krugman (2009) speaks of "the profession's blindness to the very possibility of catastrophic failures in a market economy" and asserts that "the belief in efficient financial markets blinded many if not most economists to the emergence of the biggest financial bubble in history".

The influence of a paradigm on the world we see is a crucial insight that allows us to understand more clearly some of the more puzzling aspects of economics and the GFC. Retrospectively it is obvious that financial markets were out of control, is it not? But then how did a majority of the experts on financial markets manage to miss this while it was happening? And how could economists believe that, while house prices were skyrocketing, there was no such thing as a bubble? Even in 2007 Ben Bernanke put into words a general sentiment when he said that "the increasing sophistication and depth of financial markets promote economic growth by allocating capital where it can be most productive. And the dispersion of risk more broadly across the financial system has, thus far, increased the resilience of the system and the economy to shocks" (Bernanke, 2007a), and in the same year: "Credit market innovations have expanded opportunities for many households. Markets can overshoot, but, ultimately, market forces also work to rein in excesses. For some, the self-correcting pullback may seem too late and too severe. But I believe that, in the long run, markets are better than regulators at allocating credit" (Bernanke, 2007b). What are we to make of these statements, representative of the opinion of many economists? Retrospectively, we can see that the situation at credit markets was not as good as Bernanke sketched: the economy was not resilient to shocks; markets were not that good at allocating credit; and credit market innovations increased risk[3]. In order to make sense of the discrepancy between the optimistic statements by a highly respected economist on the one hand, and the dismal state of the economy at that time as we perceive it now, it is helpful to recall just some of the characteristics of mainstream economics:

1. Crises are caused by shocks
2. Agents are rational
3. Little to no attention for debt and the risk involved
4. Focus on the efficiency of financial markets

[3] An example: Credit securitization increased the appetite for risk (Hansel & Krahnen, 2007). Yorulmazer (2013) concludes that "to generate its full benefit for the society, financial innovation needs to be accompanied by an adequate regulatory framework to set the right incentives." Deregulation has arguably eroded any such framework.

These are some of the characteristics of the interpretative framework of many macroeconomists. Due to the one-sided focus on the efficiency of financial markets, at the expense of attention for ‘irrationalities’, it is only logical that a rapidly expanding financial market is seen as a blessing for the economy. And households finally able to own a house as a result of relaxed lending restrictions are seen as ‘households with more opportunities’ rather than ‘over-indebted households with a high risk of default’. The world encountered by the economic community based on the paradigm of mainstream economics as described in paragraphs 4.2 and 4.3 was a world of expanding possibilities rather than increasing fragility. In fact, given their theoretical predisposition, economists had every reason to believe this story of the Great Moderation: economic volatility was low, financial innovation did enhance possibilities. Only retrospectively can we say with Barry Eichengreen (2009) that “the Great Moderation was an illusion”. And the Great Moderation in return led to a belief that the chosen theoretical path was essentially correct: it confirmed the already existing consensus. Writes Stiglitz: “For those believing in perfect markets, even repeated crises are seen as rare events, accidents that don’t really need to be explained” (Stiglitz, 2011, p. 594). And so economists were stuck in what Stiglitz calls ‘an equilibrium fiction’: theory feeding into worldview, worldview confirming theory, etc.

Both Kuhn and Polanyi believed that the paradigm and interpretative framework were fundamental, so that it is very difficult to adopt a different perspective. This at least partly explains the ‘blindness’ of economists, before 2008, to the risks of financial innovation, a large pile of debt (on the increase in leverage of large commercial banks and investment banks, see Kalemli-Ozcan, Sorensen, & Yesiltas, 2012) and deregulation (Crotty, 2009). It was only when the shortcomings of mainstream economics were laid bare by the GFC that economists could, retrospectively, see the growth of an unprecedented bubble on financial markets. It is not just a matter of interpreting the same set of raw data; more fundamentally, we are dealing with two different visions, two different worlds. The truth is that the economic community had not been able to see a bubble, not even with all the data of the world.

With Kuhn’s hypothesis of paradigms as a set of elements (narrower or broader) that gathers a scientific community around a single worldview, we are able to make sense of developments in economics before the crisis. Economists were indeed quite narrow-minded, sometimes using models based on inaccurate and old-fashioned foundations, ignoring counter-evidence and deviant ideas. Yet these are not necessarily signs of a malfunctioning scientific community, as is sometimes suggested. The aforementioned characteristics are characteristics of a convergent mind, which is required in order to make scientific progress. The extent to which economics was dominated by convergent thinkers, however, is questionable. Had there been more divergent thinking in economics, the ‘blinders’ might have been not as restrictive as they were now. This discussion will be taken up again the last chapter, on the future of economics. But first another topic: the Great Financial Crisis as an anomaly.

## 5. Anomalies

The reader might have gotten the idea that paradigms are completely immune to criticism, and that scientists are unreflective monomaniacs just doing what they have always done. Of course this is not true. We know that scientists can change their mind. Kuhn, however, wanted to correct the one-sided image of scientists as anarchistic inventors, continuously falsifying hypotheses and inventing new theories. In truth, says Kuhn, science is more conservative, more patient probably. Most of the time, scientists try to elaborate on what is given in a paradigm, thinking convergent rather than divergent. Henceforth, paradigms are resilient. Tensions, unsolved puzzles, deviant facts and theories: all these phenomena can, to a certain extent, as long as a paradigm is still viable, be 'ignored' – as 'puzzles to be solved', for example.

Sometimes, however, unsolved puzzles threaten the status quo. Kuhn calls these puzzles 'anomalies', which can potentially subvert an entire tradition of scientific practice (Kuhn, 1996 (1962), p. 6). An anomaly is not necessarily the end of a paradigm (sometimes the puzzle is solved *in ultimo*), but the possibility that it inaugurates the end of a paradigm is large. Kuhn defines an anomaly as a violation of the paradigm-induced expectations that govern normal science. A simple example: if it is believed that all swans are white, then a black swan is an anomaly; given the consensus that a swan must be white, a black swan is unexpected. This tension between the expected and what is actually the case has to be resolved; hence the discovery of an anomaly is the occasion for an exploration that does not end "until the anomalous has become the expected" (Kuhn, 1996 (1962), p. 53). That is: until the paradigm yields predictions that are in line with reality.

It takes some time before an anomaly is seen as an anomaly. Anomalous events are, in the first instance, often categorised as 'normal'. Kuhn refers to a playing card experiment, in which participants were asked to identify a series of playing cards shown to them successively. Most cards were ordinary, but some cards were devious, e.g. a black four of hearts. Initially these 'anomalies' were put into one of the existing categories and identified as normal: the black four of hearts was identified as a black four of spades. Participants only began to hesitate and suspect that something was wrong after repeated exposure to anomalies. Analogously, argues Kuhn, scientists identify anomalous events initially as 'normal'. Novelty only emerges with difficulty (Kuhn, 1996 (1962), p. 64). The anomaly is then recognized to be out of line with the paradigmatic: only the man who knows with precision what to expect can observe the anomaly that does not match his expectations (Kuhn, 1996 (1962), p. 65).

The discovery (or sudden appearance) of an anomaly, i.e. that which cannot be explained within the confines of the paradigm, is the start of a prolonged period of ‘pronounced professional insecurity’, as Kuhn calls it, with unsolvable puzzles (Kuhn, 1996 (1962), p. 68). The ‘professional insecurity’ is even bigger when it involves failures with a problem that was long thought to be solved – but turns out not to be due to new circumstances. In those cases it repeatedly turns out that (partial) solutions to the problem had been anticipated in quieter times – but then, those solutions were ignored: for ‘if it ain’t broke, don’t fix it’. As long as puzzles are solved with ‘paradigmatic means’ there is no need for revision. Not until the scientific community has run out of all usual options will the boundaries of the paradigm be trodden: “retooling is an extravagance to be reserved for the occasion that demands it” (Kuhn, 1996 (1962), p. 76). A scientific crisis is such an occasion.

It is hard to tell when a puzzle stops being a mere puzzle and becomes an anomaly, a reason for a scientific crisis. Each and every problem that normal science regards, from its paradigmatic stance, as a mere puzzle can, from another point of view, look like a source of crisis (Kuhn, 1996 (1962), p. 79). An example: the outdated microeconomic foundations of the DSGE model were for some economists enough reason to pick another model – but up until very recently the majority of economists still worked with the DSGE model without hesitation. Of course, the longer it takes the scientific community to solve a puzzle, the larger the odds that it is seen as unsolvable and induces a crisis. As time passes by, and the puzzle is still not solved, the arguments to stick to the rules of the current paradigm become less and less compelling. Divergent thinking gradually replaces convergent thinking and “the rules of normal science become increasingly blurred” (Kuhn, 1996 (1962), p. 83).

In chapter 7 and 8 the possible responses to anomalies are discussed at length. For now that leaves us with the question what an anomaly is and does. We have seen that an anomaly questions the time-honoured wisdom of a scientific community which we have called a ‘paradigm’. As such, it is a direct attack at what we know and how we arrived at that knowledge: it is an epistemological crisis. It challenges a way of knowing, an interpretative framework (cf. MacIntyre (2006), p.4); by doing so, it may even for the first time make us aware of the relativity of a paradigm, i.e. that it is an interpretation among other possible interpretations, one of the many possibilities of conceptualizing, measuring, categorizing and organizing knowledge. As long as a paradigm is undisputed, it might seem as if it is the only possibility, or at least the only *reasonable* possibility. An anomaly opens up a whole world of possible approaches to the world, to the economy – which is why divergent thinking flourishes in times of crisis, and the focussed convergent thinking less. Scientists in time of crisis "may as a result come to recognize the possibility of systematically different possibilities of interpretation, of the existence of alternative and rival schemata which yield mutually incompatible accounts of what is going on around [them]" (MacIntyre, 2006, p. 4). Anomalies, events that are unexpected and cannot be caught in paradigmatic terms, question the relationship between 'seems' and 'is'; how the world appears to us (in our paradigm) and how it turns out to be. While ordinarily it is (unconsciously) assumed that our idea of how the world (the economy) works is more or less accurate, during an epistemological crisis this assumptions stands under severe stress: do we understand the world? Do we understand the economy, or is it beyond our comprehension?

# 6. The Great Financial Crisis

## *6.1. Introduction*

The anomalous collided with the paradigm when the economic community seemed to be expecting it the least. Economists were engaged in specialist debates about minor details in the model. At this time of unprecedented calm for economics and the economy, all of this was blown away by the GFC, an economic crisis with a magnitude unsurpassed since the Great Depression of the 1930s. Millions of jobs were lost, governments were in trouble and the financial sector was a mess. Suddenly, proponents of the Great Moderation lost all their credibility, and so did the models on which their predictions were based. The economic crisis was just as well a crisis for economics. Economists started to interrogate themselves: how could we have been so wrong? And is one crisis enough to discredit all previous work?

It can safely be said that most economists were surprised by the GFC. Is this a reason to examine the mainstream paradigm? Some would argue that there is hardly a reason to do this, for the fault is not so much in economic theory, as in the economists who used it. Economists almost collectively failed to apply theory correctly to the world. If economists would have paid enough attention to what was going on around them (piling up of debt, irresponsible investments) there would have been no problem for them to identify the increasing instability of the economy. The solution to the problem is, in this case, not an alteration of the course of economics, but merely a re-education of economists. This argument (though partly correct) fails to grasp the 'radicalism' (from the Latin radix, root) of a paradigm. The worldview of the economic community was shaped by paradigm: economists were products of the paradigm within which they were educated. The paradigm really does go to the root of economics. An attempt to re-educate economists, to alter their vision, is thus simultaneously an attempt to reform economics. If one believes that economists should have been more aware of the dangers dormant in the economy (and there is almost general consensus that this is true) then one has to consider the possibility that the paradigm has been inadequate, i.e. that the GFC is an anomaly that questions the validity of mainstream economics.

The failure of economics is not (merely) a failure of individual economists, but at least partly to blame on the consensus of the economic community. We have mentioned how a chosen paradigmatic path can lead scientists to neglect other, socially relevant, problems. Clearly, economics did not give the problem of financial instability enough attention – economists did not even notice it! There is thus every reason to investigate why the chosen path led economists to ignore the impending doom and why they consequently were so unpleasantly surprised by the GFC.  Surely, we must find discrepancies between the characteristics of the economy as revealed by the GFC and the characteristics of the economy according to mainstream economics as defined in chapter 4.

### *6.2. The GFC in a nutshell*

So, what did the GFC reveal about the economy? First, a very short recapitulation of the GFC: the direct cause of the crisis were problems on the housing market in the United States, specifically that part of the market that was dominated by so-called subprime mortgages, mortgages to people whose creditworthiness was very questionable. Quality of loans deteriorated during the years before the crisis (Demyanyk & Hemert, 2011). Securitization of subprime mortgage loans led to a rapid expanse of the amount of credit provided to households with lower incomes (and thus more risk; Mian & Sufi, 2009). Loans were subsequently spread around the globe via innovative financial instruments – which made it more difficult to know who bore which part of the risk and moreover led to a highly interconnected global financial system.

A small disturbance in the economy of the U.S. (and, with markets as fragile as they were, any event could have opened Pandora's Box) the lower-ends of the housing market were in trouble, as people with subprime mortgages could no longer meet their contractual obligations. The 'virtuous circle' of increasing prices and looser financial conditions was broken and transformed into a vicious circle of decreasing prices, more defaults, stricter financial conditions, etc. Due to the complexity of the financial system (with loans sliced, packaged, repackaged and repackaged again) financial institutions were unable to examine their own risk position, let alone that of other parties. The presence of so-called toxic assets drove the price of all derivatives down; insecurity about the value of assets on the balance sheet and a high debt-to-equity ratio forced banks to sell assets and reorder their balance sheet. Troubles spread from one balance sheet to another (balance sheet contagion). Soon financial flows virtually disappeared and not only banks, but also governments, businesses and households were able obtain liquidity on financial markets: the modern capitalist economy that was based on a smoothly functioning financial sector came to a halt.

This (very) short and crude summary does not do full justice to the complexity of causes that led to a crisis with the magnitude of the GFC, but the main ingredients are present: debt, complexity, interconnectedness, risk, and deregulation[4]. What sense could the economic community have made of these circumstances? Was it understandable, or even conceivable? Or was it an anomaly?

### *6.3. The GFC as anomaly*

An economic crisis such as the GFC has not been on the radar of mainstream economics due to its lack of attention for the financial sector and its complexities. As such it is a very serious challenge for the economic community. A juxtaposition of some of the characteristics of the economy according to mainstream economics and the economy 'according to the GFC' clarifies this impossibility. The GFC was a crisis that came about through internal dynamics of a capitalist economy: asset prices and the availability of credit were engaged in an upward movement – a bubble. These dynamics were ruled out in DSGE models, a serious shortcoming for "DSGE models are not able to account for the occurrence of rare economic crises" (Fagiolo & Roventini, 2012, p. 82). Rather than describing the endogenous movement towards fragility and instability, DSGE models described an otherwise stable economy that is affected by an exogenous shock that is in principle not accounted for in the model. Consequently DSGE models do not really explain crises such as the GFC but merely refers to a *deus ex machina* mechanism in order to account for the downward movements of the economy (Fagiolo & Roventini, 2012, p. 86). The assumptions made in these models (e.g. no defaults due to the No Ponzi Game condition, which assures that debt cannot be rolled over to infinity) exclude the possibility of anything like the GFC.

So even though DSGE models had up- and downward movements resembling the business cycle, they described phenomena that were qualitatively different from the GFC or the Great Depression. The gradual destabilization and 'overheating' of the economy is absent in a 'general equilibrium' model. Hence, "there was nothing in the prevailing models suggesting the possibility of the kind of collapse that happened last year" (Krugman, How Did Economists Get It So Wrong?, 2009). The lack of attention for the irrationalities involved in the subjective processing of information (which in itself might be incorrect or incomplete) had a further negative influence. In the real world, however, asset prices were not just based on 'fundamentals', but increasingly on the expectation that asset prices would grow indefinitely: a bubble. Again, notice the discrepancy between economic theory (perfect markets with

[4] For a short history of deregulation in the U.S., see Sherman (2009): "In a completely unregulated market, derivatives trading expanded quickly, increasing from a total outstanding nominal value of $106 trillion in 2001, to a value of $531 trillion in 2008. This rapid growth overwhelmed the legal and technological infrastructure of the industry".

rational individuals) with reality (bubbles, individuals guided by the ‘animal spirit’ that tells them prices will go up).

Then we have the story of the Great Moderation, the supposed end of the business cycle and the triumph of sound economic policy. Again, the GFC clearly does not fit into this picture of the economy as sketched in the paradigm we described in chapter 4. So the GFC contradicts all three major elements of the paradigm as we have defined it: the DSGE model, the theory of financial markets biased by overemphasizing the EMH at the expense of for example behavioural finance, and the Great Moderation, and it is not clear how this paradigm could ever accommodate anything like the GFC without going through a serious transformation. Clearly, this discrepancy makes the GFC a genuine puzzle; a puzzle, moreover, that is very challenging up to the point of being anomalous, of leading to a crisis.

The anomalous character of the GFC can also be proven by an analysis of the response of the economic community. In previous chapters we have seen that the answer to fundamental questions in economics had been agreed upon. Economists abstained from participating in these discussions and focussed on other problems more directly related to the progress of economics, i.e. working on the loose ends of the paradigm. Macroeconomics was in ‘fine-tuning mode’ (Caballero, 2010), thus ignoring valuable (but divergent) contributions from the periphery. An example: the microeconomic foundations of economic models were based on microeconomics of the 1960s and 1970s, and there was no fundamental revision of the microeconomic foundations of the standard economic model (Stiglitz, 2011, p. 596). In recent years, however, we see the debates about those fundamentals being opened again. There is renewed attention for the role of non-rational elements in human decision-making, e.g. the role of ‘animal spirits’ on financial markets (a good example is the work of Robert Shiller, e.g. Akerlof & Shiller (2009)). There is also a discussion about the tenability of DSGE models and its ‘shock-based’ approach to the problem of the business cycle, with renewed attention to alternatives (for example the ideas of Hyman Minsky, whose ideas a neatly summarized in Minsky (1992)).

Again, it must be emphasized that this entails no value judgement: that the current openness is better than the closeness of the past (we will discuss this in more detail in the next chapter). Convergent thinking is part and parcel of science within a paradigm, just as a scientific crisis comes with more divergent thinking. However, the current open-mindedness of the economic community is an indication that the GFC is indeed an anomaly. It has reopened the debate about fundamental questions. Different perspectives on difficult questions get more attention: the earlier mentioned ‘irrationalities’, old-school Keynesian ideas, market inefficiencies (especially on financial markets) and the possible benefits of government intervention in the form of regulation; the bubble phenomenon is subject of many studies; debt and the distribution of debt and savings in the economy are back on the agenda (see for example Eggertsson & Krugman (2012)). In other words: questions that had not been part of the package of question ‘allowed’ in the mainstream paradigm are now (re)introduced in economics, and ‘unorthodox’ answers to those questions as well.

As explained above, a paradigm is not just a theoretical affair: it has a deep influence on the worldview of the scientific community as well.  It is what we have called an ‘interpretative framework’. Thus economists saw a Great Moderation not just because of the data, but also because their paradigm was dominated by the ideas of a perfect market, economic equilibrium and rationality. Anomalies challenge the self-evidence of such interpretative frameworks. Thus we would expect that the GFC challenges the idea that the economy, in and of itself, heads for a peaceful equilibrium in a world filled with rational agents. And we would expect that the GFC makes clear that this was an interpretation of the world, among many other possible interpretations.

And, indeed, the economic community has become aware of the relativity of its worldview. The arbitrariness of the old paradigm became painfully clear. The GFC has opened the eyes of many economists to their own presuppositions and prejudices about the economy. Thus the puzzlement of the question: “How did economists [we] get it so wrong?” The possibility (and in this case, superiority) of other worldviews was clear: the idea that the economy naturally gravitates towards disequilibrium makes more sense in a post-GFC world than the traditional belief that the natural state of the economy is equilibrium.

## 7. Revolution?

### *7.1. Insecurity and crisis*

When an anomaly occurs, discovery commences (Kuhn, 1996 (1962), p. 52). An anomaly is thus no reason for panic or preaching doom: it is the start of an exciting period in science, one of those rare periods in which scientists are not conservative but actually meet up to the stereotype! Whereas the area of anomaly is initially the field of a few specialists, gradually more and more scientists are interested in it, and the anomaly is acknowledged to be *the* problem of a particular field of science. New facts are discovered; new theories are proposed (and often found wanting). The conservativeness of normal science is replaced by a childlike curiosity, a willingness to explore new, previously forbidden areas. "Awareness of anomaly opens a period in which conceptual categories are adjusted, until the anomalous has become the anticipated. Then discovery has been completed" (Kuhn, 1996 (1962), p. 64). However exciting this may be, such a period of discovery is also profoundly terrifying, a period of insecurity. The old rules have failed and science is performed in a vacuum (i.e. without clear rules) until new rules have been found (Kuhn, 1996 (1962), p. 68).

Remarkably enough most crises in the history of science were caused by problems that had long ago been recognized but of which the scientific community believed it had solved them (Kuhn focusses on the natural sciences, but didn't the economic community think it had solved the problem of the business cycles, give or take a few details?). When the old solution is invalidated the sense of failure is even more acute: it is humiliating to acknowledge that it is not a radically new phenomenon that invalidates existing theories (shouldn't economists have learned from the Great Recession?). Moreover, Kuhn discovers that many of the solutions that eventually resolved scientific crises had (partly) been anticipated earlier on – but dismissed as non-paradigmatic.

The shift from closed-mindedness to open-mindedness is not sudden, but a gradual process. The natural conservativeness of scientists leads to an initial resistance to non-paradigmatic approaches so that "even a discrepancy unaccountably larger than that experienced in other applications of the theory need not draw any very profound response" (Kuhn, 1996 (1962), p. 81). So what makes an anomaly pressing enough for scientists to engage in non-conventional attempts to solve it? According to Kuhn, there is no general answer to this question. Even persistent and recognized anomaly does not always induce a crisis, for scientists know that even the most stubborn puzzle might eventually give in and respond to conventional attempts to solve it. Scientists can usually divert themselves with any of the other problems in their field that demand attention. Some anomalies are more than 'just another puzzle' and end in crisis, and this can be so for various reasons: it challenges the fundamentals of a paradigm; it has far-reaching practical consequences; the mere length of the struggle leads to a sense of urgency. Most of the time, it is a combination of factors (Kuhn, 1996 (1962), p. 82).

Given that the circumstances are so that a crisis is unavoidable (finding the solution of the puzzle is imperative) the practices of normal science are no longer satisfying for an increasing number of members of the scientific community, who no longer wish to adhere to the rules of the paradigm. Of course as times goes by this number grows; even the most conservative of scientists come to see the futility of sticking to the paradigm. And the scientific community will recognize that solving *this* particular puzzle is *the* subject matter of the discipline. And over time the rules of the paradigm become decreasingly binding: initially, it is quite clear what is 'done' and 'not done'. Later on, it is not so clear anymore what one is allowed to do, and though there still is a scientific community presumably held together by a paradigm, few entirely agree on what that paradigm consists of (Kuhn, 1996 (1962), p. 83).

Even less orthodox approaches are now scientifically acceptable: research in the crisis somewhat resembles research during the pre-paradigmatic phase, when several paradigms fought for the upper hand (Kuhn, 1996 (1962), p. 84). It is less coordinated, less monolithic and more opportunistic; or, divergent thinking trumps convergent thinking. Confronted with a stubborn anomaly, a scientific community adopts a different attitude towards its paradigm. This attitude is expressed in different ways: proliferation of competing paradigm-articulations (e.g. answers to the questions: What is economics? What is a satisfactory economic explanation?); a readiness to try almost anything to solve the puzzle; expression of explicit discontent with the current state of affairs; recourse to philosophy (of science), and the subsequent debate over the fundamentals of science (Kuhn, 1996 (1962), p. 91).

### *7.2. Ending the crisis*

This situation cannot last forever, or there would be no more scientific progress (remember how agreement on a paradigm is paramount to progress) and in the absence of a paradigm that holds a community together, the scientific field as a shared enterprise would cease to exist. Thus eventually the crisis must come to an end. Kuhn identifies three ways in which a scientific crisis comes to an end (Kuhn, 1996 (1962), p. 84).

The most disappointing outcome is that the problem resists even the most radical attempts to solve it, and the scientific community eventually despairs over ever solving this problem. The problem is then set aside as for the moment being unsolvable, in the hope that future generations might have more advanced tools (or more brilliant minds) at their disposal to solve the problem at last.

A more satisfying outcome is that normal science, against all odds, proves to contain the appropriate methods, tools and concepts to solve the problem. The scientific community then returns to business as usual, with the old paradigm intact (perhaps with some minor alterations, within the scope of the normal development of a paradigm), “despite the despair of those who have seen it as the end of an existing paradigm” (Kuhn, 1996 (1962), p. 84).

The third, and most exciting, outcome is that the crisis is only resolved with the emergence of a new paradigm and a battle over its acceptance – a scientific revolution. The end of the affair is a complete transition in which the scientific community has changed its view of the field, its methods and its goals. The old paradigm is replaced with a new, incompatible, one. This fundamental transition entails a different set of problems and solutions, and the world in which science is done is transformed (Kuhn, 1996 (1962), pp. 6, 85).

Notice how a scientific community cannot just throw away the old paradigm in the absence of a serious alternative. Non-paradigmatic science is impossible. Scientists would rather formulate all kinds of rectifications to an existing paradigm, or just put the puzzle aside, then throw away the old in the absence of a credible alternative. In the latter case, they would cease to be scientists proper: to reject a paradigm without substituting it for another one is to reject science altogether. Henceforth, the decision to reject a paradigm is at the same time a decision to accept another one. The availability of a serious alternative is imperative for an anomaly to lead to the appearance of a radically different paradigm (Kuhn, 1996 (1962), pp. 77-79).

The ultimate consequence of the anarchy of crisis-science is the victory of a new paradigm at the expense of the old one, with a completely different set of rules (Kuhn, 1996 (1962), p. 80). But on what criteria is the decision for one particular paradigm rather than another based? Kuhn’s answer to this question in *The Structure of Scientific Revolutions* is quite radical, and he tones down his answer in later work. We will now first follow Kuhn in *The Structure* and after than nuance this position.

*7.3. How to choose between paradigms?*

Kuhn dismisses the possibility of choosing between paradigms on the basis of the evaluative procedures of normal science, for these too are paradigm-dependent, and are thus in question themselves (Kuhn, 1996 (1962), p. 94). Scientist A may think solution x to be very satisfactory, whereas scientists B, adherent of another paradigm, believes it is completely unsatisfactory. These two scientists have different standards, so “they will inevitably talk through each other when debating over the relative merits of their respective paradigms” (Kuhn, 1996 (1962), p. 109). In the absence of higher, paradigm-transcending, standards both parties are stuck in a circular argument: The convincingness of the arguments depend on whether one accepts the paradigm (Kuhn, 1996 (1962), p. 94). It is as if both scientists use different languages.

The confusion sets in with even greater force through the impact of one’s paradigm on what one sees. A paradigm-shift makes the scientist see the world differently. Thus, in times of revolution the scientist’s perception must be re-educated. Kuhn compares a paradigm-shift to conversion, which is deeper than a superficial change of opinion. One’s whole world is different as a result of this conversion. The incommensurability of vision thus contributes to the confusion in the dialogue between scientists A and B (Kuhn, 1996 (1962), pp. 111, 112).

The radical incongruity between two paradigms makes itself felt in the way in which Kuhn conceives of the replacement of the old paradigm with a new one. The incommensurability is threefold. A new interpretation of scientific practice and nature is initially an idea living in the mind of a few *avant-garde* men or women, often young and relatively new to the field (presumably less committed to the old paradigm). In order to make their interpretation that of the whole community, and thus a paradigm, they have to convince the rest of the community that the new ideas are superior to the old ones. Here, however, they run up against the incommensurability-barrier. Working in different paradigms the two groups “must fail to make complete contact with each other’s viewpoints” (Kuhn, 1996 (1962), p. 148), in three ways. First, there is disagreement about the standards and definitions of science: what counts as a legitimate problem, what must be explained, and what counts as a satisfactory explanation. The second form of misunderstanding originates in conceptual innovations: the new school uses different concepts, or the same concepts in completely different ways (e.g. In Einstein’s new theory terms as ‘space’, ‘time’ and ‘matter’ had completely different connotations that in the old, Newtonian world; in economics, confusion may arise about what one means with ‘rationality’ or ‘fragility, for example). This leads to further misunderstandings (Kuhn, 1996 (1962), pp. 148, 149).

The third and most fundamental reason for confusion roots in the earlier-mentioned idea that paradigms profoundly affect the way the world is seen, so that the early adopters of the new idea and the rest of the community "see different things when the look from the same point in the same direction" (Kuhn, 1996 (1962), p. 150). Cleary, full communication is only possible when the parties in the conversation are at least talking about the same thing; otherwise, the two parties will inevitably fail to understand each other’s arguments.

To overcome the barriers (especially the last one) one of the two groups involved has to undergo a ‘conversion experience’, a “transition between incommensurables”. Precisely because of this incommensurability the transition cannot be made one step at a time, “forced by logic and neutral experience”. It will always involve a leap: it happens “all at once (…) or not at all”. Because this conversion is beyond strict logical proof “the transfer of allegiance from paradigm to paradigm (…) cannot be forced”. Conversion is often blocked by the same conservativeness that, in times of normal science, makes sure scientists do not give up on a paradigm a the first signs of difficulty. Convergent thinking, normally contributing to scientific progress, in scientific crises actually impedes progress: “at times of revolution that assurance [that the old paradigm will eventually succeed, A.G.] seems stubborn (…) [but] that same assurance is what makes normal or puzzle-solving science possible” (Kuhn, 1996 (1962), pp. 150-152).

Reading Kuhn’s description of scientific conversion, one gets the idea that the choice of one paradigm over another is a rather random affair, a matter of taste rather than truth. Kepler became a Copernican partly because he worshipped the sun. Individuals change their minds for all sorts of reasons. A convincing argument is that the new paradigm can solve the problem that led to a crisis in the old paradigm. This argument acquires strength as a scientific community increasingly despairs over their paradigm and have made the solution of the problem the ultimate priority of their profession. It also depends on the extent to which the performances of both paradigms are comparable (if the standards differ radically, the new solution is not acknowledged as a solution according to the old standards). Another set of convincing arguments is based on aesthetics. The new theory is ‘simpler’, ‘neater’, ‘more suitable’. These arguments are specifically effective in the early stages of paradigm adoption, when it is not yet fully worked out and has only solved a few problems it encounters. Early adopters are then persuaded not so much because it outperforms other paradigms, but for aesthetic reasons (Kuhn, 1996 (1962), pp. 152-156).

*7.4. Nuances*

At least two pressing questions remain after this short sketch of Kuhn's position: 1. Is the adoption of a new paradigm really as sudden and 'illogical' as a conversion? 2. Can we still speak of scientific progress, if scientific history is a succession of different paradigms? Both questions are closely related to incommensurability. Starting with the first question: to which extent are the two paradigms comparable, open to engage in a discourse based on common concepts, experiences, etc.? 'Conversion' seems to imply that the breach is broad and unsurpassable. In a later essay, Kuhn takes away some of the rough edges of his ideas (or maybe the misunderstandings of an incomplete or uncharitable reading of his book). Theory choice is based on a number of criteria (Kuhn mentions accuracy, consistency, scope, simplicity and fruitfulness[5]). The choice is never unambiguous: each of them individually is imprecise – individuals may disagree over the accuracy of a theory – and taken together, they may be in conflict – with theory A being more accurate but less fruitful than theory B. These criteria are not strict rules which determine choice, but values that influence a choice: "Two men deeply committed to the same set of values may nevertheless, in particular situations, make different choices" (Kuhn, 1977b, p. 331). So the first answer to the objection that conversion is random and based on subjective judgements (as scientific disagreements show), is that, even though the judgement is individual, it is based on (quasi-)universal values that as such transcend paradigms. Choices are explicable in terms of those values, even if the disagreement about the proper application of those values maintains. These values are thus a basis for common discourse, and it is important that scientists find such a basis to foster a dialogue between them.

Communication between two paradigms takes place by translation. There are limits to what proponents of different theory can communicate to each other. But they can present each other their concrete technical results – witnesses of the successfulness of a paradigm. To these concrete results, value judgements can then be applied independently of paradigm preferences. These results can then persuade others to investigate what made them possible. In order to do so, they must learn to translate, to speak a new language. During this learning process a scientist may come to the conclusion that his own language is superior and abort the

[5] Consistency, both internal and with other domains of knowledge; fruitful of new research findings.

operation; but it is possible that he finds himself being immersed more and more in the strange language and having “ceased to translate and begun instead to speak the language like a native” (Kuhn, 1977b, p. 339). Conscious choice is absent, but the transition has not been irrational: it is a reasonable conversion based on dialogue. Seeking a dialogue, making an effort to understand the other in his or her particular vocabulary is thus important during crises.

This brings us to the second question: Can we speak of progress in science? An easy question with respect to normal science: its progress is clear, as more problems are solved and its scope is extended through the ordinary activity of the scientific community. It is a more difficult question however, when it comes to paradigm shifts. Of course the community belonging to the victorious paradigm asserts that its victory is progress; doing otherwise means admitting that the other paradigm was better. But scientific progress cannot be just power-play, a result of ‘history told by the winners’. There must be another criterion on which to base the judgement that, through scientific revolutions, science progresses. This basis is not an ahistorical vantage point from which objective judgements can be made. It is rather the competence of the scientific community. The scientific community is a group unlike any other group in society – by virtue of education, adherence to (unwritten) rules, peer pressure – and thus uniquely capable of judging over progress: “The scientific community is a supremely efficient instrument for maximizing the number and precision of the problem solved through paradigm change” (Kuhn, 1996 (1962), p. 169). In other words: the scientific community is uniquely capable of applying scientific values to concrete paradigms. During scientific crises a healthy scientific community is of vital importance. A healthy community is a community capable of dialogue, with a willingness to listen to other parties, a certain amount of open-mindedness, adhering to paradigm-transcending scientific virtues.

## 8. Tentative Advice

*8.1. Thinking out of the box*

What is the future of economics? What will the economic community do? What should the economic community do? These are difficult and complicated questions. It is obvious that Kuhn's theory is descriptive rather than predictive or normative. Yet, as an interpretation of the current situation the analysis of the previous chapters does have to tell us something about the future of economics. Even though history never repeats itself, there are discernible historical patterns, also in the history of science. Thus the story thus far, as an interpretation of the situation in which economics finds itself now, can tell us something about the future as well. And from an interpretation of the situation in which we find ourselves, some practical advice inevitably follows. But, even though we have identified the tensions between the old paradigm and the anomaly, we can and will not say too much about the content of the 'new economics'. This is up to the economic community to decide. We can say something about the process that can be followed in finding the resources to deal with this crisis.

As expected, the GFC has attracted a considerable amount of attention in the past few years, as the magnitude of the crisis was recognized throughout the economic community and a resolution failed to occur. In the absence of a fast recovery (both of the economy and of economics) the GFC came to dominate macroeconomic debates and became the sole preoccupation of almost the whole community. This had some remarkable consequences. Ordinary scientific discourse, civil and, in circumstances of 'paradigmatic peace' a matter of agreements rather than disagreements, gave way to verbal violence on blogs and in newspaper columns.

This is in accordance with the pattern as described by Kuhn. The power of a paradigm to bind a community around a single way of doing science – suppressing divergent voices – is diminished during a crisis and so research activities that normally would have fallen outside the scope of the paradigm were allowed to have their say. Economists began to demand radical, out-of-the-box solutions to the problem of the GFC. Buiter (2014) celebrated the fact that the Bank of England had finally shed "the conventional wisdom of the typical macroeconomic training of the past few decades" and replaced it with "an intellectual potpourri of factoids, partial theories, empirical regularities without firm theoretical foundations, hunches, institutions and half-developed insights" (Buiter, 2014, p. 5). The same sentiment is heard in Krugman (2009) when he writes that the major reason why economists went astray was their desire for an “all-encompassing, intellectually elegant approach” rather than the messiness he (temporarily) favours. And in the same spirit Caballero (2010) argues that economists should stop looking for an all-encompassing macroeconomic model (the ideal of the core) and embrace the more fragmentary insights of peripheral economics. Finally Quiggin (2011) believes economist should be more humble.

It is clear that a majority of economists believes something has to change after the collapse of “the whole intellectual edifice” (Greenspan, 2008) of mainstream economics. Before we discuss this ‘something’ we should give an honourable mention to those economists who are so conservative and paradigm-stuck (undoubtedly very good puzzle solvers) that they deny the existence of the puzzle in the first place. According to them the GFC is a black swan, an extreme outlier that is of no concern to economists. Yes, markets are efficient and beneficial, but at the same time “capricious and beset by unpredictable irregularities” (Tzotzes, 2016, p. 26). Eugene Fama, one of the founding fathers of the EMH, has said that what we have seen in the past decade is precisely what we would expect if markets are efficient: “I don’t even know what a bubble means” (Fama as quoted in Cassidy (2010)). Stubbornness is an excellent scientific character trait, but this comes closer to cheating to solve a puzzle. For a financial crisis as the GFC is a regularly recurring phenomenon, as shown by Kindleberger & Aliber (2005). And it is hard to deny the contradiction between the proclamations of the beneficence of markets on the one hand and the destabilization and destruction caused by deregulated financial markets on the other hand.

### *8.2. The future of economics: some possibilities*

The economic community cannot ignore the GFC. It has to solve this puzzle; the question is: how? The threefold distinction made by Kuhn is a simple tool to categorize possible outcomes. Even though we cannot predict the outcome of the struggle of the economic community, we can shortly discuss what each of these possibilities entails in the case of economics tentatively. The first possibility is that the economic community is able to solve the puzzle within the boundaries of the old paradigm. In the case of economics this means that mainstream economics can, with the necessary adjustments, be used to describe, understand or model a financial crisis. The second possibility is that the economic community is unable to come up with a satisfying solution to the problem and starts working on other issues. The third possibility is a scientific revolution, as described in the previous chapter.

Reality eludes simple classifications and therefore a simple either/or is out of the question. Mainstream economics is an amalgam of various, sometimes contradictory, elements. A paradigm consists of rules, methods, maxims, models, concepts, etc. Even a scientific revolution is never a total rejection of all that has been done before. A gradual revolution might be possible, as Kunin & Weaver (1971) have argued. This is not the occasion to decide which elements may stay and which elements should go. But, based on the interpretation of the current state of economics that we have developed we can give advice to the economic community for the road that they should follow, especially about the most fruitful attitude.

First of all, accepting an incomplete solution is better than claiming a complete solution that turns out to be the wrong solution. This is a refreshing insight from Kuhn: accepting a failure can be very much part of the scientific deal. For the economic community this means giving up on striving for the perfect economic model and accepting limited an scattered insights, for "an elegant economic 'theory of everything' is a long way off" (Krugman, How Did Economists Get It So Wrong?, 2009). Caballero (2010) calls this the 'pretense of knowledge syndrome'. Do not try to cram as much modifications into an already flawed model to make it better. Adding irrational agents, more flaws and more frictions to a basic DSGE model is currently not the way forward. Stiglitz (2011) believes such 'Ptolemaic exercises' "will be no more successful than they were in astronomy in dealing with the facts of the Copernican revolution" (Stiglitz, 2011, p. 593). So modesty is the key. The peripheral method (deep insights, but not in a systematic way and not of the economy as a whole) is for the moment to be preferred over the core method (devising an economic theory of everything). Modesty is an important virtue.

The second lesson has to do with the starting point of economic research. All too often an economist analyses a perfect world and comes up with a model for this world. Economic models explained the minor fluctuations of the economy but when it came to explaining the exceptional (when economic models are badly needed) the models were practically useless. Stiglitz (2011) compares economics with medicine to make this point. Modern macro was like medical science that only studies healthy people for the simple reason that 90% of the time the patient is in perfect health. That would obviously not make any sense: we learn much about the well-functioning human body by studying pathologies. Likewise "economists should be learning from the 'pathology' of recessions and crises (Stiglitz, 2011, p. 608). This would make economic models more relevant. Charles Goodhart once said that the DSGE approach "excludes everything I am interested in" (Buiter, 2014). Being based on a perfectly functioning economy (equilibrium, rationality, etc.) modern macro could only begin to understand the pathologic by tweaking a perfect world. The absence of interest in economic diseases means that "research on the origin of instabilities, overinvestment, and subsequent slumps has been considered as an exotic side track from the academic research agenda (and the curriculum of most economic programs)" (Colander, et al., 2009, p. 263) – and Colander et al. (2009) argue that it is time for a 'major reorientation'. Thus: focus on the pathology; do not start with ideal abstractions but with the messiness of the real world.

Next, those economists who believe that the mainstream is bankrupt should come up with a viable alternative. For refusing one paradigm automatically implies accepting another one. While initially this paradigm will not be worked out completely (resembling the periphery rather than the core) its adherents have to be capable to convince the economic community that it is superior to what they have. To come up with an explanation of the GFC is probably the most convincing argument in a field preoccupied with this problem. Do not attempt to rival with the mainstream in building a theory of everything: it is simply beyond the scope of economics, and it is not necessary. But there are many interesting ideas about financial crises that did not (yet) make it in mainstream economics: insights from behavioural economics, about irrationality (e.g. Akerlof & Shiller (2009)), game theory, balance sheet contagion (e.g. Kiyotaki & Moore (2002)), endogenous instability (most famously by Hyman Minsky, (Minsky, 1992)). So focus on the GFC, seek for a solution to that problem, and see what may come from that. Kuhn has shown that times of crises are *the* opportunity for non-paradigmatic ideas to get more attention and to be taken seriously.

### *8.3. Being in a paradigm: Blanchard*

Yet a paradigm is difficult to change or overthrow, as is shown in an interesting paper by Olivier Blanchard. We have seen that the mainstream has responded to the GFC. Articles and themes from the 'edges' and the 'periphery' of economics now have the attention of the mainstream, for example through the work of Markus Brunnermeier (see for example Brunnermeier & Oehmke (2012), Brunnermeier, Eisenbach, & Sannikov (2012). It is to early to say anything conclusive about these developments, but it could be the start of a more gradual revolution, the one we shortly spoke about in chapter 2; or depending on how broadly one wants to define the mainstream (are the edges still mainstream?) it is just the adaptation of the paradigm, that can handle the anomaly after all by shifting its focus.

Yet there is another interesting development which I would like to discuss, and that is the attempt to save DSGE models. One can see this for example in an attempt to model the Greek crisis using a DSGE approach (Gourinchas, Philippon, & Vayanos, 2016). This model carefully introduces some tweaks on earlier DSGE models: households are now divided in two groups (savers and borrowers), there is a possibility of default and there is something that resembles a financial sector. We can certainly speak of a vast improvement over earlier models, but the question arises immediately: is it worth all the trouble to build a DSGE model of this? What can we learn from it?

I suggest that it is really not worth the trouble to improve DSGE models. Olivier Blanchard, however, believes that DSGE models do have a future, so let's take a look at his argument. Blanchard starts by summing up all the reasons to dislike DSGE models: they are based on unappealing assumptions, "profoundly at odds with what we know about consumers and firms"; the standard method of estimation is unconvincing (with many parameters that are determined 'random', with little to no empirical evidence to justify the value of the parameter); normative implications are not convincing; DSGE models are bad communication devices (read: you can even make the most straightforward intuitions obscure by wrapping them up in a DSGE model). So here a four quite convincing reasons not to like DSGE models (Blanchard, 2016, pp. 1-3). Still Blanchard defends the use of these models, but for strange reasons.

The first reason is that it is worthwhile to pursue "a widely accepted analytical macroeconomic core, in which to locate discussions and extensions". That is true, but why necessarily a DSGE? And what is the worth of such a core if it is fundamentally flawed? The second reason is that Blanchard believes that it is good to start from microfoundations, for he does not know where else to start from. Again, his reasoning is unclear. For what is the worth of microfoundations if they are fundamentally flawed? Furthermore, economists were quite content to do macroeconomics without microeconomics before the 1970s. Keynes did not need microfoundations to build his system. Desirable as microfoundations may be, it seems to me that, in the abscense of credible microfoundations, it is better to do without them. Next, thinking in terms of distortions to a competitive economy is far from "a reasonably plausible description of the economy", but again, Blanchard does not know where else to start from (Blanchard, 2016, p. 3). But there certainly are alternatives, e.g. Minsky's financial instability hypothesis, which does not start from a fictitious perfect world, but still gives a more accurate description of a modern economy with a financial sector than any DSGE model does.

Blanchard seems to believe that DSGE models have a future not because there are no better alternatives, but because he is simply ignorant of these alternatives. Although he does argue that other models can be useful as well (DSGE modelling has to become ‘less imperialistic’), he still believes DSGE models are the core, and thus the goal towards which economists should strive (yes, other models can be useful as ‘upstream’ and ‘downstream’ to and from the DSGE models – it all does sound a bit condescending). Paul Krugman, commeting on Blanchards point that DSGE models provide a core for macroeconomic research, writes: “Really? That’s the point of a paradigm that has taken over the field? It sounds, by the way, exactly like the defenses I heard of academic Marxism when I was young: never mind whether it’s right, it provides a framework” and “we should admit to ourselves how very sad the whole story has become” (Krugman, 2016).

However sad this story may have become, it does provide an excellent example of how resilient a paradigm can be, and how someone working within such a paradigm has gotten a single-minded focus on developing that paradigm (at the expense of other approaches). Actually the strongest argument that Blachard can come up with is that the DSGE model at least is something around which research can be organized, i.e. that it is at least something that can serve as a paradigm. Accuracy is a secondary issue. If Blanchard is representative of the whole field, Colander (2010) is right when he states that “the economics profession is unlikely to respond to the crisis with any sense that it should change” and the DSGE model will remain for the time being the dominant approach to macroeconomic problems.

### *8.4. A different attitude*

A different attitude (than that of Blancard c.s.) is demanded of economists. Times of crises ask for open-mindedness. We know from Kuhn that full communication between two paradigms is virtually impossible. Different standards are employed by each group, different expectations are part and parcel of the incommensurability of two paradigms. In economics the major reason for incomprehension is the earlier mentioned desire for completeness and mathematically elegant models such as the DSGE, against the scaterred and less elegant solutions found in the periphery. An example: the assumption of rationality leads to economic models that are relatively easy to solve, so introducing irrationality makes it a lot more difficult to close a model. It is noteworthy that Hyman Minsky was an excellent mathematician, but nonentheless in his work “eschews sophisticated mathematical and econometric methods” (Foley, 1998, p. 6). So open-mindedness that is so necessary in a scientific crisis means letting go of presuppositions about what a 'solution' looks like. Only then is one open to be 'converted'.

Opponents in the economic debate, rather than seeking to blackmail the other's point of view (which is unfortunately what happens quite often), have to seek a common ground for debate in the scientific virtues we mentioned in the previous chapter. Particular choices should be explicable in terms of those virtues. Even if one makes different choices than one's opponent, these choices can in principle be explained in terms of virtues shared (e.g. economists A may opt for a comprehensive model because it is internally consistent, while economist B chooses to go for external consistency and accepts internal contradictions). So even if I believe that the mainstream is foolish and completely out of touch with the real world, I can understand that economists value the simplicity and internal consistency, and then argue that "yes, simplicity is important, but so is external consistency, and I have a solution that is externally consistent, so let us see how we can find a solution that is both internally and externally consistent". Try to understand which scientific virtues are important to your opponent and argue on the basis of those values.

In spite of the limits to translation, concrete results are universally understood, and can speak to everyone in favour of a paradigm. It would greatly benefit the quality of economic debates if the debaters would focus on these concrete results, rather than on abstract concepts. Instead of quarelling about abstract models (however important this may be) economists could better discuss concrete examples – which luckily they often do. So instead of wondering how to build a model of 'the housing market', for example, be concrete: we have witnessed a complex development on the U.S. housing market, with a multiplicity of factors that have influenced the course of these developments. How can we make sense of this particular development? Especially in a scientific crisis, a discussion of a concrete case brings opposite groups closer to each other. It fosters creativity and openness to other viewpoints; let's call this eclecticism. As Colander (2011) argues, economists should focus more on the art of applied economics and less on developing correct theories. Economists should no longer rely on one single model to explain it all, but gather biths and pieces of information and insights from core and periphery and creatively apply those bits and pieces to the economy as it is today. This is a wise approach during a scientific crisis when the standard answers no longer do; but modest eclecticism is also a good idea in normal, paradigmatic science. An attendant advantage is that economics is more concrete, and thus of more practical relevance (perhaps it reduces the gap between academic economics and applied economics?).

Which brings us to a final remark about a distinction hinted at throughout this thesis: the difference between convergent and divergent thinking styles and their scientific relevance. In *The Essential Tension* (Kuhn, 1977a) Kuhn emphasizes the role of convergent or traditionalist thinking styles in science against the idea that scientists are ‘innovators’. But the balance between traditionalists and innovators, or ‘convergent’ and ‘divergent’ thinkers is hard to find: “We must seek to understand how these two superficially discordant modes of problem solving can be reconciled both within the individual and within the group” (Kuhn, 1977a, p. 237). Against the common prejudice that scientists are innovators, Kuhn has rightfully stressed the importance of traditionalism. Yet we must not forget that a critical eye towards a scientific tradition is quintessential. The economic community should consider to which extent divergent thinking has been discouraged through for example the educational system. Mirowski (2010) criticizes the economic curriculum: history and philosophy (subjects that encourage a critical stance towards the contemporary consensus) have gradually disappeared, replaced by more mathematics and statistics. Arguably this is one of the reasons why so much dubious assumtions have survived in economics, and why so much valuable work did not make it into the paradigm. Perhaps economics has been too conservative. The future of economics lies then in training economists who are familiar with the economic tradition, but who do not take this tradition for granted. As G.K. Chesterton said: Tradition does not mean that the living are dead, but that the dead are alive.

## 9. Conclusion

The goal of this investigation was to know how economists could have gotten it so wrong, and to learn from these mistakes. We have done so by comparing Kuhn's ideas about scientific development with specific developments in economics in order to interpret the latter by means of the former. I believe that this interpretation has enrichened our understanding of what happened before and during the GFC, as well as our capacity to empathise with economists. Of course this interpretation is not the only interpretation possible; it is neither exhaustive nor exclusive. Nevertheless, it allows us, more than other interpretations, to understand economists by recognizing in their behaviour something that is shared by many scientists. This is, in my opinion, more fruitful than just concluding that economists were ignorant, or stubborn, or brainwashed by free-market ideology.

Economics of the last decades of the 20$^{th}$ century was increasingly a paradigmatic science, i.e. it was practised by a scientific community gathered around a single idea of what economics is: its methodology, its theory, its worldview. Even though this paradigm was fairly broad and diverse (Kuhn's definition of 'paradigm' is flexible enough to allow for quite some diversity), we have been able to highlight three characteristic elements of this paradigm, that we called the mainstream: DSGE models, the Efficient Market Hypothesis (EMH) and belief in the Great Moderation, the end of the business cycle. Through this characterization of the paradigm, we were then able to explain why the GFC was so problematic and became an anomaly. This means that the current situation can be described as a scientific crisis. Based on this diagnosis, we have given some directions about what economists could do now.

One of the great benefits of this interpretation is the extent to which it allows us to understand the economic community while remaining fairly neutral about the quality of its performance. We can see that a certain degree of convergent or traditionalist thinking is indispensable for scientific progress, yet that at the same time these scientific 'blinders' reduce the opportunity for new ideas to be successful. This lays bare the tension in economics, which is merely an instance of a tension for science in general: between conservatism (which makes scientists stubborn enough not to give up on a puzzle) and open-mindedness (that keeps a paradigm 'up to date'). The extent to which the economic community was too conservative in the past few decades is up to debate. Certainly some of the elements in the paradigm were, with the benefit of hindsight, untenable and should have been the topic of discussion already before the GFC. Yet, at the same time the mainstream has given us a lot (sophisticated models, advanced mathematics in economics, and so on) and this has only been achieved because economists were determined to solve puzzles within the boundaries of this paradigm. It is easy to demand a radical change in economics; yet it is often forgotten that a scientific community that makes radical change with every problem it encounters is a community without direction, and soon ceases to make any noticeable progress.

The challenge in economics is to read the times: when is it time for change, when should we persist and adhere to the existing paradigm, and to which extent is a more gradual revolution possible? There will always be disagreement about these questions: some economists are by virtue of their character more conservative or more innovative; some attach importance to other scientific values than others. The task of the economic community as a whole is to engage in an honest debate. That means being open for the other's point of view; being willing to learn to speak a different scientific language to understand viewpoints different from one's own; accepting that current solutions are always partial – that an eclectic approach to problems is currently better than a monomaniacal focus on a single idea; focussing on concrete problems rather than lofty but abstract idealisations – debates about concrete problems are more fruitful. The challenge for the economic community is to educate a new generation of economists that has the ability to engage in these debates. That requires changes in the economic curriculum. It is up to the economic community to decide about the future of economics, and how to get there. It comes down to this: show that you are proud of the economic tradition not by desperately clinging to it; rather, keep it alive, fresh and vigorous.

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