The London School of Economics and Political Science

The Epistemological Value of the Consumption Based Capital Asset Pricing Model

Jacob Bjorheim

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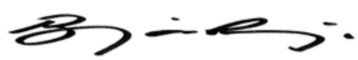
Declaration

I certify that the thesis I have presented for examination for the MPhil/PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

I declare that my thesis consists of 99’853 words.

Jacob Bjorheim
10 July 2014



Dedication
I would like to thank my supervisors Nancy Cartwright and Roman Frigg for staying with me over all these years. Without their inspiration and encouragement, this project would not have been completed.

In gratefulness, I thank my family – Gabriele who knows what it takes to do what I did, as well as my young sons Elias and Johannes who I trust will fight the good fight and hold fast the form of sound words.

Abstract

The thesis is a philosophical analysis of the consumption based capital asset pricing model (CCAPM), investigating in particular its epistemological and methodological foundations.

Financial markets are integral parts of advanced and developing economies. They matter because they channel unspent household income into banks' savings accounts and assets such as bonds and stocks. Financial economists have traditionally taken interest in the pricing mechanism that underlies this capital allocation. The consumption based capital asset pricing model (CCAPM) is a prominent effort to describe, explain and predict such prices. It tells a story of investors' trade-off between consumption now and later and which portfolio of assets to hold. The CCAPM based narrative intuitively makes sense, and the chosen methodology involving theoretical assumption, mathematical models and empirical tests follows the professions' standards of good scientific practise. But does CCAPM's research programme provide knowledge for use?

My thesis seeks to answer this question in a novel way. Instead of embarking on yet another asset pricing research project, I let Philosophy of Science inform my analysis. Following a "primer" introducing essential CCAPM topics and notations, I discuss, in turn, its theoretical foundation, mathematical model, and empirical test results from a philosophy of science perspective. I find that a few fundamental principles and several auxiliary assumptions combine to develop a simplified, partial and idealized theory of investors, financial markets and assets. The model reflects and represents this theory but also makes narrow claims that are distances away from the real situations they target. Unsurprisingly, ideal model assertions fail standard statistical tests of significance.

I conclude that mathematical deductive modelling rooted in orthodox, a priori based fundamental principles create ideal and fictional settings that limit their scope and portability. The development of even more granular models within this orthodox paradigm that searches for "event regularities" will not render the desired knowledge for use. The real situations are possibly too complex to be captured in simplified assumptions, ideal theories and mathematical structures. Novel methodological and ontological approaches to asset pricing are in demand. Hence, claims about tendencies in the real data might replace the current focus on point-forecasts.

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Chapter 1:

Introduction

1. The fascination of financial markets

Financial markets fascinate. They demand our full attention. Hardly a day goes by without a popular media news headline on price developments of financial assets traded in cash-, bond-, stock-, derivative- and currency-markets. It is often reported that the behaviour of such asset prices are related to the way market participants react to real or expected changes in economic variables such as growth, inflation, unemployment, or official monetary policy rates and fiscal measures. Globally interlinked financial markets and their tradable assets are, therefore, more than ever an integral part of our socio-economic reality. [1]

1.1 Three groups of financial market participants

Financial market participants are numerous. They are found in most geographical locations in the urban world. For simplicity, let me group these participants in three sets. The first contains individual and institutional investors, the second holds representatives from public sector entities such as central banks and regulating bodies, and, finally, the third is populated by academically minded financial economists.[2] The members of the three sets have varied and different motivations for their engagement with financial markets. Let me in a stylized way review some of these motivations. Investors first.

Economic theory tells us that household income can be either spent or saved, and those savings equal investments. Savings thus reduce the cash amount available for current

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[1] At the time of writing, financial markets globally have gone through their most challenging times since the so-called "great depression" in the 1920's. Banks have collapsed, some sovereign states have been unable to re-pay debt held by their creditors, economic growth has disappointed and unemployment rates are still above pre-crisis levels. My thesis does not explicitly address these issues. Nonetheless, asset pricing is of great interest to, for example, financial market regulators who are concerned about both micro- and macro-financial stability. In this context, asset price "bubbles" can dislocate financial markets and create negative spill-over effects to the real economic sector.

[2] As I proceed with my discussion, little hinges on this distinction between individual and institutional investors so I will use the term "investor".

expenditure. Households who choose to save may, therefore, be regarded as investors. Investors have traditionally held their unspent income in savings account at a local bank where it has earned a modest, but in most cases, safe return over time and through different economic states. Over the past three decades, the development of financial market instruments has drastically complemented this time-honoured offering. Today, investors can choose from a broad range of investable assets. We find subjects that were once the province of specialists such as stocks, commodity, real estate, hedge-, and private equity-funds have now become small-talk topics among well informed next door neighbours. Although many call this activity saving for the future, others denote it investments, liquidity provision or even outright speculation. Fact is that investments offer households the opportunity to increase, but also to reduce, and, in a worst case scenario, even lose all their savings. This influences their thinking and force decisions upon them about how much income to spend now, how much to save for later, as well as which financial assets to hold in their investment portfolio.

Members of the second set, i.e. representatives of public sector entities are often called upon to protect less sophisticated investors against the lures of the financial markets. Policy makers, such as regulators and central bankers, therefore, keep a close watch on the activities in and around the financial markets. Such institutions are usually mandated to ensure orderly market conditions and to advance non-inflationary economic growth. Of late, public sector employees have been working over-time as they develop policy responses to counter the financial crisis which erupted during the summer of 2007 in the so-called “sub-prime” segment in the market for tradable US mortgaged backed securities. Since then this crisis has spilled over to the European sovereign debt markets and the commercial banking sector. Among the measures that highlight policy makers vigour are, for example, a plethora of new credit facilities provided to domestic and international banks by the Federal Reserve Bank of New York, new capital, liquidity and leverage measures to make banks more resilient under the Basel Committee for Banking Supervision framework, and the set-up of a new special purpose vehicle by the European

Union, i.e. the European Stability Mechanism which could raise funds to support its members states in case of urgent financing needs.[3]

Besides investors and public sector representatives, financial economists within the third set have also taken a keen interest in market-related topics. Their focus is often summarized under the label "Finance", and its content is offered to students at academic institutions around the globe. Standard textbooks usually divide the subject into two; Corporate Finance and Investments.[4] Corporate Finance concerns itself with financial decision-making at the corporate level and deals with topics such as a choice of projects to invest in, ways of financing them, and how much of the net profit generated from these investments should be returned to shareholders in the form of dividends or stock buy-backs. Investments, by contrast, analyse decision-making at the household level. Here, as I alluded to earlier, decisions relate to how much of the current income should be consumed now or saved for later, and how the available savings could be allocated between investments into and within different asset classes such as cash, bonds and stocks.

1.2 Demand for understanding

The members of the three sets - investors, public policy makers and academics - have in common that they want to understand how financial markets work. With this understanding, they can all make more informed, and, hopefully, better decisions about investments, policy actions, and theory development.

These demands for understanding do not seem unreasonable, and, at a first glance, it is certainly conceivable that they can be satisfied. After all, financial markets are man-made and have been purposefully designed in terms of their regulatory frameworks and operational procedures. Likewise, traded financial assets are mostly well understood and must, in most cases, fulfil specific minimum documentary requirements before they can be registered at the stock exchanges where they are subsequently offered to the public.

[3] Sources as of 20 October 2013: www.frbny.org , www.bis.org, www.esm.europa.eu
[4] Ross (2009). Bodie (2007), Brealey (2007). Copeland, Weston, Shastri (2005).

Furthermore, information created through financial market activities is transparent and readily available. Every day, prices of financial market assets are collected in real-time, placed in data-banks by market service providers, and made available through social and professional media. Not only are these various pricing quotes accessible and in many cases analysed by investors and public sector representatives, they are also the objects of more profound analysis by the academic profession. This analysis is frequently labelled "research".

1.3 Research

The research approach adopted by financial market economists is not very different from that encountered in various other social and natural sciences. A worth-while topic is identified, concepts are formed, categories are established, data are collected, explanatory theories for their behaviour are developed, models are constructed, theoretical hypothesis are formed, predictions are compared with the available data, and evidence-based claims are made. If such hypothesis-based predictions are not rejected in standard statistical tests, the research project's theories are, implicitly, viewed as "tentatively accepted" or even "confirmed". A better understanding of the how's and why's of financial market activities can thus be achieved – or so it is claimed.

Examples of such research activities can be found in Finance textbooks, leading academic journals and in papers published, for example, under the umbrella of the reputable National Bureau for Economic Research (NBER) - a private, US based, non-profit research organisation of economists. NBER thus promotes a better understanding of how the economy and financial markets work. Its web-site claims: "The NBER is committed to undertaking and disseminating unbiased economic research in a scientific manner, and without policy recommendations, among public policymakers, business professionals, and the academic community". [5] The web-site also highlights that:

> "Twenty-four Nobel Prize winners in Economics and thirteen past chairs of the President's Council of Economic Advisers have been researchers at the NBER. The more than 1,300

[5] Source as of 20 October 2013: www.nber.org

> professors of economics and business now teaching at colleges and universities in North America who are NBER researchers are the leading scholars in their fields. (...)The research activities of the NBER are organized into a series of nineteen research programs and fourteen working groups".

In 1991, Asset Pricing was included as a separate NBER programme series with a credo to examine: "…the sources and nature of fluctuations in the prices of financial assets including stocks, bonds, and foreign currency. In addition, members of the program analyse the international transmission of fluctuations in asset prices". [6]

In its first year, eight research papers were published. Twenty years later, at the end of 2012, the number was close to 100. There are common red-lines in this population of asset pricing publications. I find that financial market economists mostly focus on the same key elements when approaching this topic. In fact, I identify five: *data, models, theories, tests and claims.* Data are widely regarded as an unproblematic given. Models, their supporting theories and empirical tests are tools in the hands of financial economists. Claims can be considered evidence based statements or assertions about the findings of the various research projects.

Through their research, financial economists aspire to provide a different kind of understanding than that obtained from casual media reports. Their advantage, they claim, lies in a disciplined use of the so-called "scientific method" which in a systematic and rigorous way combines the five elements I identified. When correctly applied, the scientific method generates topic-relevant understanding which aspires to reach the highest level of objectivity, reliability and relevance. In academic circles, this type of understanding is often denoted "knowledge". It is positioned as distinct, meritorious and scientific. As a proxy for measuring the success of this activity, the academic profession of financial economists has produced several Nobel Laureates – the most notable being Harry M. Markowitz, Merton H. Miller and William F. Sharpe in 1990, Robert C. Merton and Myron S. Scholes in 1997, Edward C. Prescott in 2004, as well as Eugene F. Fama, Lars Peter Hansen, and Robert J. Shiller in 2013. [7]

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[6] See footnote 5.
[7] Source as of 20 October 2013: www.nobelprize.org

Academics, as a consequence, play a central role among the three main groups of partakers in financial markets. Should they be unable to live up to their own aspirations in terms of providing scientific knowledge, the consequences for both the users of such knowledge and the profession itself will indeed be serious. Investor's savings could fail to perform according to their return expectations, public policy makers could fail to fulfil their mandates, and academics could fail to create a firm foundation from which others can work. Understandably, the search for certainty related to the price behaviour of financial market assets is high on the agenda for the various stakeholders.

1.4 Asset Pricing Models

A closer review of the asset pricing research endeavour reveals that of the five identified key topics, i.e. data, models, theories, tests and claims, most attention is given to the *asset pricing models*. This certainly does not diminish the role of the other research topics. In fact, data, theories, and, in particular empirical tests are crucial in the way they support the model-building effort. When comparing the model predictions against the real situations, for example, it is common to review both the data points and the theoretical assumptions in the effort to improve the accuracy of the results.

So far, financial market economists have given us a multitude of asset pricing models. They are mostly developed with the same goal in mind; follow the NBER Asset Pricing research credo mentioned above in *section 1.3* and investigate the sources and nature of fluctuations in asset prices. As a consequence, asset pricing is overwhelmingly empirical in nature. Only a few models are "just" theoretical explorations.

Asset pricing models can, conveniently, be grouped into two broad categories. The first is often referred to as fundamental-, absolute-, or *macro-models*. Financial market economists who develop such models claim that the investor and his/her behaviour must be the focal point of any asset pricing research project. An elaborate development of a multi-equation view of, first, the investors' decision making with respect to consumption, savings and investments, second, the financial market structure, and, third, the financial market assets is regarded as necessary to explain asset price movements. For reasons I give in the introduction to *Chapter Two*, my personal interest is directed at a widely used macro-model, i.e. the Consumption Based Capital Asset Pricing Model (CCAPM) and its most vocal advocate, John H. Cochrane's substantial contributions (1991, 1997, 1999, 1999a, 2000, 2005, 2005a, 2006, 2008, 2011, 2011a). In the following *Chapter Two, section 4*, I review its most relevant aspects and let the analysis be the basis for discussions in the final three chapters of my thesis.

The models in the second group are usually referred to as "statistical- , time series-, relative-, or, *factor-models*. Financial market economists, who develop such models, mostly regress a single-equation on cross-sectional time series drawing on a host of specific macroeconomic or financial market related variables as arguments to explain the development of asset prices. Investors' behaviour, financial market structure and their asset are, therefore, not explicitly accounted for and described. The most prominent representatives of this practice are Eugene F. Fama and Kenneth R. French (1992, 1993, 1995, 1996). Their statistical regression analysis show, for example, that stocks issued by small companies, have, over time, significantly outperformed those issued by larger companies. I have more to say about factor-models in *Chapter Two, section 1.3*, but my description will be kept at a minimum. Nonetheless, I will, from time to time, contrast them with the work done on the CCAPM.

2. Complications ahead

Despite the combined research effort undertaken by financial economists working with the two groups of asset pricing models, only modest empirical progress has been made with respect to improving the quality of the available knowledge upon which investors, policy makers and academics can draw. Going forward, I will refer to such knowledge as "knowledge for use" and address it in *Chapter Two, section 1*. Granted, the academic profession has provided plausible *stories* about stock price movements, elegant mathematical models and valid theoretical elaborations within a deductive framework from an a-priori starting point. In particular, they have delivered a reliable track record in *describing* how stock prices behaved in past periods. Nonetheless, financial economists do not portray themselves as historians. They face a more difficult task when seeking *explanations* for the "sources and nature" of these price movements. Furthermore, they seem to be at a loss when seeking to *forecast* the level of and changes in stock prices over time.

Within the academic profession, the challenges related to explaining and forecasting stock price movements are widely acknowledged and evidenced by numerous empirical studies in which most, if not all, asset pricing model-based claims have been formally rejected in standard empirical tests. Financial economists, however, are in general not prepared to question their methodological commitments. They continue to proceed within their traditional macro- and factor-based framework of theorizing and fitting theories to the data. Nonetheless, while the profession tries even harder, the practitioners are growing increasingly impatient. The lack of reliable and accurate knowledge for use has undermined stakeholder's confidence when it comes to applying the advice as they are not systematically rewarded. It seems that insights so rendered seldom go beyond common sense understanding of financial market activities.

In my thesis, I acknowledge the discrepancy between what the profession delivers and what the practitioner requests. I, therefore, ask why the academic profession of financial economists, and in particular Cochrane's version of the CCAPM, has been unable to provide accurate epistemological value.

3. Roadmap for resolution

My starting point takes the NBER credo and its research project on the "sources and nature of fluctuations in the prices of financial assets" at face value. I, therefore, let financial economists describe how they model stock prices, craft accompanying theories, draw data into their models, test hypothesis against the real situations, and, finally, make statements about their findings at the end of their research process.

A description of these crucial elements and their interaction along the research value chain is, however, not sufficient. It only provides a foundation for identifying the central issues related to each of these topics. I, therefore, need to advance my assessment. Consequently, I explore whether these elements and their connections contain complications which might stand in the way of a better explanation of stock price behaviour. I let this part of my analysis be informed by a second group of academics which is not necessary associated with the topics of finance, i.e. philosophers of science. They too have something to say about the key elements identified above, i.e. theorizing and modelling. While financial economists are takers of data, originators of models, developers of theories, conductors of tests, and makers of claims, the philosophers are questioning the very nature of these elements, and their proper applications.

To my knowledge, my thesis is the first attempt of using philosophy of science to identify philosophical questions in the context of financial asset pricing. I, therefore, take a broad view of these topics and seek to identify which philosophical traditions can be applied to the consumption based asset pricing theorizing and modelling effort. In particular, I investigate how dominant themes in asset pricing can be conceptualized in the philosophy of science language. In my effort to connect these two academic traditions, "translation work" is warranted. This philosophically grounded analysis also helps me in structuring appropriate responses to the challenges facing the profession of financial economists.

With respect to Cochrane's consumption based capital asset pricing research effort on describing, explaining and forecasting the behaviour of asset prices, I make the following main assertions.

First; the consumption based asset pricing model, i.e. the model "M" in CCAPM, is an applied mathematical model. It has been given a dual role; it is used for theorizing, i.e. "conceptual explorations" and for econometric analysis, i.e. fitting theories to the data.

Second; in its first role, the model "M" is developed in a "process of isolation" which establishes "simplified", "idealized" and even "fictional" versions of the investor, the financial markets and the financial assets. This a priori starting point is used to deduce more granular models referred to as “analytically convenient special cases”.

Third; in its second role, the model “M” extends its analytically convenient special cases towards the real situations. I draw on the much debated “equity risk premium puzzle” to demonstrate the model’s inability to develop what is referred to as “empirically useful representations”. Given the inadequacies of the model “M” with respect to representing the empirical world, Cochrane suggests incorporating “habit persistency” as a new argument into the model’s mathematical utility function.

Fourth; I argue that the consumption based capital asset pricing effort, in Imre Lakatos’ sense, is a “research programme” that uses a few fundamental, “hard core”, principles, a large flexible set of auxiliary, “protective belt”, assumptions, methodological decision rules in the form of “positive and negative heuristic”, and an established form of assessing whether the research programme is “progressive” or “degenerating”. Cochrane’s “habit-persistency” argument is thus well within the heuristic of the programme, it modestly improves the programme’s predictability and contributes to its progression.

Fifth; although the analytical convenient special cases are to some extent de-idealized and de-fictionalized versions of their a priori starting point, they remain tools for theorizing and find few methodologically sound bridges to the real situations they target. As a consequence, I do not expect the CCAPM research programme to progress by the construction of even more granular, lower-level models as they are too dependent upon the auxiliary, belt assumptions. Hence, these cases lack horizontal portability to situations different from those they are meant to represent.

Sixth; given the research programme’s modest level of epistemic value, it’s advocates are well advised to move away from the model-based “point-forecasts” approach towards one that makes “tendency claims” with respect to empirical situations. This re-direction replaces Cochrane’s own suggestion to reduce the importance of standard statistical tests when evaluating the model-based claims, and, in addition, offer a sounder foundation for emitting knowledge for use to the various stakeholders.

4. Overview of the thesis

I now briefly turn to the structure of my thesis. Following this *First Chapter* which motivates my interest in the consumption based capital asset pricing research topic, I continue with *Chapter Two* on Asset Pricing Models, Theories, and their Assessment. The *purpose* is to introduce a few essential asset pricing related topics that are often found in the vast literature. My *goal* is to provide first time readers a foundation for easy reference as I proceed through the thesis. My review is directed towards three particular topics that will continue to stay with us; CCAPM's theoretical foundation, the model "M" in CCAPM, and CCAPM's application and assessment.

Following the descriptive "primer" in *Chapter Two* which is free of specific criticism, I turn to the philosophical analysis of CCAPM's theoretical basis in *Chapter Three*. The *purpose* is to assess the structure and content of the theories that Cochrane develops to support his explanation of asset price formation and fluctuation. My *goal* is to seek a better understanding of the individual assumptions, how they interactively connect, and what kind of investors, financial markets and financial assets they establish. I let my discussion be informed, primarily, by Imre Lakatos, and I report that the consumption based asset pricing effort can be characterized as a Lakatosian research programme. My analysis also draws on the insights of other philosophers of science, i.e. Uskali Maeki, Mary S. Morgan, Daniel M. Hausman and Nancy Cartwright.

In *Chapter Four*, I shift my focus away from asset pricing theories to the model "M" in CCAPM. The *purpose* of this chapter is to clarify what the model *is*, what it is *used for*, and what it *represents*. My *goal* is to identify possible problem areas that can be held responsible for the model's inability to generate adequate knowledge for use beyond trivial "common-sense" advice. I primarily draw on Roman Frigg and Stephan Hartmann, Alan Gibbard and Hal R. Varian, Milton Friedman and Nancy Cartwright's contributions to inform my discussion. I find that the model "M" is an applied mathematical model with a dual role; it's analytically convenient special cases are, on one hand, used for theorizing, and, on the other, used for reaching out towards the empirical world. I demonstrate how the fundamental, core, principles in conjunction with the auxiliary, belt, assumptions are used to develop such cases and why they mostly fail to also be empirically useful representations.

Building upon the two previous chapters on asset pricing theories and models, I use *Chapter Five* to analyse the third central topic of my thesis, i.e. CCAPM's application and assessment. I illustrate the main issues in the context of the so-called "equity risk premium puzzle". The *purpose* is first to explore why the CCAPM fails to explain the puzzle and then to evaluate Cochrane's response. My *goal* is threefold; show that forecastability, initially, is the accepted "litmus test" for assessing the progress of the CCAPM research programme, point towards the two main obstacles that the programme is confronted with, i.e. unrealistic assumptions and socio-economic complexities, and propose a solution for the programme to consider overcoming the double-trouble. The solution advises Cochrane to de-emphasize point forecasts and consider making claims about tendencies in the empirical situations. I let my analysis be informed by John Dupré, John Sutton, Tony Lawson, Milton Friedman, Alexander Rosenberg and Nancy Cartwright.

Chapter 2:

Asset Pricing Models, Theories and Assessment

Introduction

In his book *A History of the Theory of Investments* (2006), Mark E. Rubinstein tells us that the foundation of investments was laid in what he called the "Ancient Period", i.e. pre 1950. His first reference goes to the Italian number's specialist Leonardo Fibonacci (1170-1240). Rubinstein tells us that Fibonacci not only helped disseminating the use of Arabic numbers in Europe but that he also brought us the calculation of the present and future value of an investment. This innovation is still with us today, and we will hear more about them later in this chapter. Nonetheless, from the 12^{th} century to what Rubinstein denotes the "Classical" and "Modern" periods of investments, i.e. 1950-1980 and post 1980 respectively, much changed, to say the least.

In this focused review of how financial assets are priced, there is no need to uncover ancient manuscripts in an effort to trace the historical development of the topic. The archaeological effort is thus kept to a minimum. In fact, for the purpose of what I want to examine in my thesis, academic asset pricing research started in the mid 1950s with Harry M. Markowitz' publication "Portfolio Selection" (1952) and continued when William F. Sharpe published "Capital Asset Prices: A Theory of Market Equilibrium under Conditions of Risk" (1964). Since then, asset pricing research efforts have developed in two main directions. One path continues to build upon Sharpe's theoretical market equilibrium foundation while the other focuses on statistical time series analysis of return movements. In *Chapter One, section 1.4*, I referred to the pricing models in the first group as *macro-models* and those in the second group as *factor-models*.

Both directional developments, however, exemplify what the two asset pricing research efforts are all about: theories and assumptions, equation-based mathematical modelling, long historical cross sectional time series of asset prices, statistical test-techniques as well as conclusions and claims. As I mentioned in *Chapter One, section 1.3*, these common elements possibly inspired the National Bureau for Economic Research's (NBER) research credo to discover: "…the sources and nature of fluctuations in the prices of financial assets including stocks, bonds, and foreign currency". [8]

My own interest is directed towards the macro models. Particularly, I concentrate on the so-called consumption based capital asset pricing model (CCAPM). There are four main reasons for my choice; (1) the CCAPM is widely recognized and used; (2) the tradition claims it to be broad enough to encompass other asset pricing research efforts - including the factor models; (3) it brings to the forefront the investor and his/her rationale for investing in the financial market assets; and, (4) it is constructed in a multi-equation framework rooted in both micro- and macroeconomic theoretical reasoning. When discussing this specific type of macro-model, I let the analysis be informed by John H. Cochrane – a leading contributor to this field of research (1991, 1997, 1999, 1999a, 2000, 2005, 2005a, 2006, 2008, 2011, 2011a). Throughout his publications, Cochrane often offers more economic explanatory insights than those rendered by other authors within the same tradition. This is most compelling in the context of the upcoming discussion.

Chapter Two can be read as a "primer" on consumption based asset pricing research effort. It familiarizes the reader with a few very specific and selected financial market concepts and serves as a reference for my discussions in *Chapter Three* on CCAPMs underlying theories, *Chapter Four* on the model "M" in CCAPM, and, finally, in *Chapter Five* on its application and assessment.

This chapter has six sections. The *first section* starts with a brief introduction to financial market data and jargon before it continues with a short description of the factor-based asset pricing model. In *section two*, I shift my focus towards the macro-based approach and review how Cochrane uses the asset pricing data to extract empirical facts and a *story*. The *story* is related to investors' behaviour and their demand for financial market assets. The *third section* continues the analysis of Cochrane's asset pricing research effort

[8] Source as of 20 October 2013: www.nber.org

and focuses on main concepts and notations. In particular, I review three specific topics; the theory of the investor, his/her utility and risk preference. This section prepares the ground for my analysis of CCAPM's theoretical framework in *Chapter Three*. In *section four*, I pull Cochrane's facts, *story*, concepts and notations together and describe the consumption based capital asset pricing model in some detail. Now the focus is on the mathematical expressions and their interconnections. The *fifth section* extends the research effort's tradition towards its final destination, i.e. the financial market equilibrium situation. For that to happen, simplification in three areas are necessary; the investor, the market and the assets. Sections four and five are the foundation for my discussions of the model "M" in CCAPM in *Chapter Four*. Finally, in *section six*, I review how the CCAPM is assessed against data originating in the real situations. In this discussion, I focus on how well the model can capture the notorious "equity risk premium puzzle", which was first introduced by Mehra and Prescott (1985). It turns out that the CCAPM in its original form does not fit the data particularly well. In fact, the model-based claims are statistically unsuccessful. Cochrane's response is to "reverse-engineer" the model. This final section paves the way for my discussions in *Chapter Five*.

1. Asset pricing data and models

Cochrane's asset pricing research effort is both empirical and theoretical. In fact, it starts with observations of financial market data. On the basis of these observations, he extracts statistical data-patterns and develops *stories* around them. Cochrane then tells us that these *stories* are made "explicit" with the help of theories and mathematical equations. The model's hypothesis-based predictions are finally compared against time series of pricing data. Cochrane's goal, following NBER's credo alluded to previously, is to explain and predict the behaviour of the data points and make this knowledge available to the various stakeholders for them to use.

Throughout my thesis, I will often refer to this expression, i.e. "knowledge for use". A brief review thereof is warranted. I let Cartwright (2007) inform my reflections. Cartwright puts claims in the context of our knowledge of social phenomena and how this knowledge can be used in policy making situations. She discusses three aspects of knowledge for use: Which claims are established, which methods "license" them, and how broad is their "scope" – in particular how such claims can be applied to situations outside the circumstances and population from which they were extracted. Let me address these three points one at a time.

Cartwright *first* asks which claims are established. In the context of my discussion, I answer the question with a reference to *section 6* later in this chapter. In that section, I introduce a well known case study showing that investments in stocks, over a period from 1889-1978, have offered a significantly higher return than investments in bonds (Mehra and Prescott 1985). By way of a declarative sentence it is claimed: "Financial market risk taking has been systematically rewarded". In order to explain why this has been the case, a consumption based capital asset pricing model is applied to the data. Although the model fails in explaining the empirical fact, it has led to a better understanding of the phenomena: "This knowledge is now leading to a much more successful set of variations on the consumption-based model." (Cochrane 2005, p. 455).

Cartwright's *second* point is related to how such claims are established, i.e. the warranting method. Later in this chapter, in *section 3* and *5*, I show that Cochrane gives the CCAPM a microeconomic foundation in which a representative investor optimizes his own situation within a financial market setting. In *Chapter Three, section 3.2.3*, I demonstrate that the CCAPM's approach to real situations can be interpreted in the context of John Stuart Mill's deductive a-priori method, and in *Chapter Four, section 4.3*, I reconstruct that approach within a "hypothetico-deductive" framework. I conclude that Cochrane warrants his claims by applying standard methodological tools and processes, i.e. *formulate* a hypothesis, *deduce* a prediction from the hypothesis, *test* the predictions, *evaluate* the hypothesis on the basis of the test results. Now, the question is whether this is defendable in the financial market context. In *Chapter Five*, *section 4* and *5*, I discuss Cochrane's research effort and conclude the deductive a-priori method is indeed defendable - albeit with a twist.

Cartwright's *third* intervention points towards the scope of the claims that have been established. She is mindful about, *first*, their use in situations outside those that are warranted by the specific method, *second*, the data population, and, *third*, the particularities of a situation. For example, Cartwright explains that ideal experiments can "tell us with certainty what the effect of a given cause is" – but only in the circumstances of that particular experiment and not necessarily elsewhere.

With this, Cartwright poses serious challenges for social scientists in general and financial economists in particular. For example, the socio-economic circumstances under which the stock market crashed in the late 1920's and again in 2008, were not the same. Furthermore, unlike experimentation in the natural sciences, financial economists cannot easily set-up controlled experiments to isolate single causes and extract their direct effects. Finally, Cochrane's choice of the deductive a-priori method is, as I mentioned, defendable but not faultless. For these reasons, Cochrane is careful. His claims seldom stretch beyond what is extractable from knowledge about past situations. Obviously, Cochrane is confident that his version of the CCAPM includes relevant and true principles that hold across a given range of circumstances. So anything derived from the CCAPM will also be true across anything in that range of such circumstances.

Portability of claims beyond the data and the particular circumstances under review is therefore a major issue in Finance. Hence, toning down claims about the future state of financial markets is recommended. This, however, does not prevent Cochrane from giving advice such as: "You have to buy stocks that everyone else thinks are dogs. Then you have to sell stocks and long term bonds in good times, when stock prices are high." (Cochrane 1999, p. 54). Nonetheless, such advice should in no circumstances be positioned as distinct, meritorious and scientific "knowledge for use".

Let me now leave this brief review of "knowledge for use" and turn back to the main content of this first section. I am concerned with the first local stops of the CCAPM research effort, i.e. the observable asset pricing data. Then, I explore how the two different groups of asset pricing models use this data.

1.1 Financial market data

It is not always easy to keep up with the ever changing headlines of financial market news or disentangling their complexities. This is particular the case when a casual bystander is exposed to financial markets jargon and their idiosyncrasies. Consider, therefore, the Financial Times (FT), a UK based newspaper which is printed in more than twenty global locations and brands itself the "World Business Newspaper". Almost daily, the reader is presented with short summaries like the four quoted here from 2013. Let us first review the journalistic views on Google – the US internet search company:

> "Google shares soared on Wednesday following the release after Tuesday's close of quarterly earnings that beat expectations, as the search engine's revenues from advertising stabilised. Shares rallied 5.5 per cent to USD 741.50 and have gained more than 26 per cent in the past 12 months. (...) The benchmark S&P 500 dipped in and out of negative territory, however, the index finished the day 0.1 per cent higher at 1.494.78, hitting a fresh five-year high. The Dow Jones Industrial Average closed 0.5 per cent higher at 13,779.17. The technology heavy Nasdaq Composite rose 0.3 per cent to 3,153.67." [9]

Later in the year, the FT reports on Google again:

> "Google, meanwhile, has been riding high on expectations, with mobile and YouTube advertising leading the way. While its shares have pulled back recently, they have been surging since its last results, climbing 12 per cent. Net revenues in the latest quarter are forecast to have jumped to $14.2bn from $8.1bn the year before, thanks in part to its acquisition of Motorola Mobility, with earnings rising 6 per cent to $10.69 a share." [10]

Next, the FT has its focus on Caterpillar – the US earth-moving company:

> "Caterpillar, the machinery maker often seen as a barometer of economic activity, predicted continued global economic weakness in the first half of the year as it lowered its revenues forecast for 2013. (...) Announcing worse fourth-quarter results than expected, the world's largest maker of earthmoving equipment by revenues forecast full-year 2013 revenue of USD 60bn to USD 68bn, (...) Caterpillar, which generates two thirds of its revenue outside of North America, said the

[9] Financial Times, 23 January 2013.
[10] Financial Times, 15 April 2013.

> lowered revenues range "reflects the level of uncertainty we see in the world today". (...) Shares in Caterpillar rallied as the company's full-year earnings forecast beat analyst's estimates, even though quarterly profits were disappointing. (....) The benchmark S&P 500 index, however, was trading 0.1 per cent lower at 1501.34, still at fresh five-year highs reached last week" [11]

Finally, mid 2013, the FT reported on Caterpillar's fortunes:

> "Caterpillar, the earthmoving and mining equipment maker, cut its full-year profit forecast for the second successive quarter as it announced net income for the second quarter down 43 per cent on the same period last year. The company, which in the first quarter suffered from declining demand from the mining industry, cut its forecast for full-year sales 2.6 per cent to a range of $56bn to $58bn. Earnings per share would be $6.50 in the middle of that range, down 7 per cent from the $7 forecast at the previous projected range…. Doug Oberhelman, chief executive, said the company's expectations for overall end-user demand remained around the same as in April, when it previously cut its forecasts. However, equipment dealers had cut inventories more than expected during the quarter. (…) The shares fell 2.43 per cent to $83.44 [12]

Before I comment upon these quotations, let us take a quick look at typical line-graphs of the stocks mentioned by the FT journalists, i.e. Google and Caterpillar, as well as one of the US stock market indices, i.e. S&P 500. [13] The graphs are extracted from Bloomberg, the business and financial market news provider. [14] They show the historical price development of the two stocks and the index value. The lines thus connect all end-of-day stock prices since 19 August 2004, the day Google became a publically traded or listed company, through 31 August 2013. Price graphs like these are published in popular media such as newspapers, magazines, television and on dealing screens at banks. The data are thus public available, abundant and easily accessible. We may call the data "raw" because they are emitted at the ultimate source, i.e. the company level and because the data have not yet been used to derive new time-series of data such as total-returns, variances and co-variances.

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[11] Financial Times, 29 January 2013.
[12] Financial Times, 24 July 2013.
[13] The S&P 500 is a stock market index of the 500 largest US based companies in terms of their market capitalisation, i.e. stock price multiplied by the number of outstanding stocks.
[14] www.bloomberg.com

Graph 1: Google stock price from 19 August 2004 to 31 August 2013

Graph 2: Caterpillar stock price from 19 August 2004 to 31 August 2013

Graph 3: S&P 500 stock index price from 19 August 2004 to 31 August 2013

The four short journalistic FT statements related to these three graphs are similar in several ways. *First*, they describe how individual stocks of two US domiciled companies, i.e. Google (Graph 1) and Caterpillar (Graph 2), as well as three main US stock market indices, i.e. Nasdaq, Dow Jones and, as exemplified in Graph 3, the S&P 500 performed over a particular time period. *Second*, the FT extracts offer the readers explanations by connecting these price levels to specific events. These events are, for example, that Google's "revenues from advertising stabilised", and that Caterpillar's dealers "cut in inventories". The *third* unifying point is that investor's activities and their behaviour are not mentioned. It is as if they are not partaking in the formation of asset prices. *Fourth*, no forecasts are offered. We are, for example, not given any indications related to the possible future path of the values of the stock market indices or the individual prices of their underlying constituents. This disappointment aside, the snippets are certainly informative to FT readers.

For the not so versed financial newspaper reader, however, FTs descriptions and explanations contain financial market "jargon" which may not be comprehensible at first sight. There are references to "stock market indices", "prices", "returns", "profit", "revenue", "uncertainty", "economic activity", etc. Different financial economists think differently about how to make sense of these concepts and how they connect. Earlier, in *Chapter One, section 1.4*, I mentioned that they can be allocated to two main schools of thoughts, i.e. the macro- and the factor-model group. Let me briefly introduce them.

1.2 Two groups of asset pricing models

The four FT extracts presented above aspire to be both descriptive and explanatory. They first describe the price developments of financial market data and then suggest that the price behaviour of individual stocks and their aggregates are influenced by particular financial and economic events. This raises at least two questions; *first*, can a connection between asset prices and individual events be established and, *second*, can such connections be generalized. Are we led to believe, for example, that whenever global economic activity increases, the price of Caterpillar's stock will always go up? Or can we expect Google's stock price to benefit from a higher level of advertising income?

Needless to say, such forward looking projections and advice, if reliable, will be of considerable help for those who invest their wealth in financial market assets. In anticipation of a less uncertain economic environment, investors could simply buy as much of Caterpillar's stock as possible, wait for the price increase to happen and realize a positive return when selling the stock at a higher price than was initially paid.

Financial economists have similar aspirations, i.e. they are looking to describe, explain and predict the behaviour of financial market data in order to give advice to various stakeholders. Earlier, I said that asset pricing models can be grouped in two cohorts under the labels macro-models and factor-models. Cochrane calls them "absolute" and "relative" models respectively:

> "In absolute pricing, we price each asset by reference to its exposure to fundamental sources of macroeconomic risks. The consumption based and general equilibrium models are the purest examples of this approach. The absolute approach is the most common in academic settings, in which we use asset pricing theory positively to give an economic explanation for why prices are as they are, or in order to predict how prices might change if policy or economic structure changed. In relative pricing, we ask a less ambitious question. We ask what we can learn about an asset's value given the price of some other assets. We do not ask where the prices of the other assets come from, and we use as little information about fundamental risk factors as possible." (Cochrane 2005, p. xiv). [15]

Cochrane's classification leads him to place his own consumption based capital asset pricing model (CCAPM) into the absolute, or macro-group. Cochrane also locates Sharpe's Capital Asset Pricing Model (CAPM) in that group. Nonetheless, Cochrane explains that when the CAPM is applied to the financial market data, it loses some of its consumer based ambitions and moves into the camp of the latter school. Cochrane also puts "pure" factor-models as popularized by Fama and French (1992, 1993, 1995, 1996) into the relative- or factor-based group. Option pricing models based upon the seminal work of Black and Scholes (1972) are also found in Cochrane's second group.

[15] In this quote Cochrane makes references to "absolute" and "relative" pricing of financial assets. In my language, I denote the former "macro-models" and the latter "factor-models".

In the next section, I briefly review two prominent factor-based pricing models, i.e. Sharpe's CAPM and Fama and French's factor-model. Thereafter, in *section 2*, I turn to Cochrane's version of the CCAPM, which is the main focus of this thesis.

1.3 Factor-based asset pricing models

While Markowitz's (1952, 1959) main focus was on how to construct investment portfolios from a large number of individual stocks, Sharpe (1963, 1964), and others such as Treynor (1961), Lintner (1965, 1969), Mossin (1966) and Black (1972), changed the perspective towards asset pricing. In this context, Sharpe stands out. He asked what can be said about individual stock prices if all investors followed Markowitz' advise on "portfolio selection". The answers were provided in two steps: In Sharpe (1963), he addresses the practical aspects of Markowitz' portfolio construction. Later, in Sharpe (1964), he extends the research effort to a partial capital market equilibrium situation which considers the relation between expected rates of return on stock investments and their risk.

Observing that the cross sectional variations in single stock returns tend to move together, Sharpe assumed that there is a common factor which could be held responsible for the return on any one stock. He was thus searching for this exogenous factor in the "stock market as a whole", the "GDP" or "any other factor thought to be the most significant influence on the returns from securities". Finally, Sharpe settled on the stock market as a whole and denoted this variable the *market portfolio*.

In order to establish his CAPM, Sharpe first enriched the financial market with more asset classes than Markowitz' stock only universe and confronted the investor with a choice between them. Sharpe found inspiration in Tobin (1958) which, based upon John Maynard Keynes' theory of *liquidity preference* (1936), had already extended the investment opportunity set to include a so-called "riskless asset" class, i.e. cash deposit at the local savings bank. This also enabled Sharpe to use Tobin's *separation theorem* which states that the investor first chooses an optimal combination of risky assets, i.e. follow Markowitz, and, thereafter, decide how much of his/her savings is allocated between that risky portfolio of stocks and the riskless asset. Second, Sharpe also filled the gap that Tobin and Markowitz had left with respect to the pricing of assets and the equilibrium situation. Sharpe found inspiration in von Neumann and Morgenstern (1947) axioms of rational choice with uncertain outcomes, and from Arrow and Debreu (1954) on the conditions for an equilibrium situation.

The combination, i.e. the "two-asset" investment universe and the introduction of a utility maximizing investor enabled Sharpe to offer a theoretical framework that eventually led to a partial equilibrium situation in the financial market. Sharpe assumed that the equilibrium is reached when all investors have optimised their own situation with respect to maximizing end-of period savings, i.e. investments. In this situation, *all* investors have agreed that there is only *one* desirable portfolio combination of the available risky assets, i.e. the market portfolio. This agreement is different from Markowitz who had investors hold different portfolios of risky assets.

But Sharpe did not stop here. He also presented a mathematical equation that subsequently was statistically tested against the real situations. It can be expressed in a single equation, single-factor model, which linearly connects the expected return on an individual stock with that of the market portfolio (Sharpe 1964):

$$E(R_i) = R_f + \beta_i [E (R_m) - R_f]$$

$E(R_i)$ is the expected (E) rate of return (R_i) on a specific stock (i). It is set equal to the risk-free rate of return R_f plus a risk premium $[E(R_m) - R_f]$, where $E(R_m)$ is the expected return on the stock market portfolio minus the risk-free rate of return. The risk premium is then multiplied by the so-called "beta-factor" β_i for that specific stock. The beta-factor has a more detailed description (Sharpe 1964):

$$\beta i = \frac{COV(Ri, Rm)}{VAR\ (Rm)}$$

The beta-factor β_i for any specific stock equals the co-variance (*COV*) between the historical return on the specific stock R_i and the historical return on the stock market portfolio R_m divided by the return variance (*VAR*) of the market portfolio R_m. Empirically, the market portfolio is proxied by a stock market index such as the S&P 500.

CAPM claims that the return on a risky asset, for example, Caterpillar's stock, has two components; the risk-free rate of return plus a compensation for company specific risks. Investors who venture beyond the risk-free assets to hold stocks get exposure to the risk of not getting the invested money back as initially expected. The investor, therefore, requires a compensation for the uncertainty of owning a particular stock. This compensation is given by the equity risk premium, i.e. the market price of risk multiplied by the "beta", defined as the quantity of risk for each specific stock. For this reason, every stock is assigned a unique, empirical "beta". The higher the beta, therefore, the more risk premium the stock should collect. The lower the beta, the more "defensive" the stock is. Intuitively this makes sense. Early empirical regression results confirmed the positive trade-off between risk and return; high beta stocks are riskier than low beta stocks. When the market portfolio performs well, for example, the high beta stock performs better. In a negative market environment, the high beta stocks do worse than the overall market.

In the end, Sharpe's CAPM did not survive more detailed empirical scrutiny, but there were other challenges as well. [16] Merton (1971, 1973), for example, points towards the theoretical possibility that other common factors could be used to statistically demonstrate why returns are differ across stocks. Ross (1976), for example, extended the CAPM to account for other factors in his Arbitrage Pricing Model (APT) framework. Not only did such models perform better than the CAPM in empirical tests, it also highlighted that CAPM's single factor, i.e. the market portfolio could not be correctly identified as Roll (1977) pointed out. These findings seriously challenged Sharpe's research effort,

[16] Well known references are Black, Jensen, Scholes (1972), Miller and Scholes (1972), Blume and Friend (1973), Litzenberger and Ramaswamy (1979), Shanken (1985), Campbell, Lo and MacKinlay (1997), Ang and Chen (2005).

and it also opened up for more elaborate equity pricing models denoted "*multi-factor*" models.

Both macro- and microeconomics provide a rich background for finding factors, which might be used to determine asset prices and returns. Fama (1981) and Chen, Roll, and Ross (1986) are early sources. Chen, Roll and Ross, for example, explore the question whether changes in macroeconomic variables such as inflation, industrial production, or the different interest rates systematically influence the return on investments in risky assets in excess of the risk-free rate.

In the course of developing multi factor pricing models, however, financial economists started paying more attention to variables that could be constructed directly from financial market data sources rather than from micro and macroeconomics. Fama and French, building upon the contributions of Merton (1973), Ross (1976) and Banz (1981), possibly developed the most influential example of a multi-factor model. It still stands out as an eminent reference point for all such factor-based research efforts – also recognized in Cochrane: "Fama and French (1996) is an excellent crystallization of how average returns vary across stocks." (Cochrane 2006, p. 13).

Using a broader universe of stocks, a longer time series and higher frequency data than those drawn upon by Sharpe (1963, 1964), Fama and French (1992, 1993, 1995, 1996) extended Sharpe's regression framework to cover cross-sectional relationship between expected excess return and risk. In empirical tests, they found statistical significance in three such variables, i.e. CAPM's market portfolio, the market capitalisation of a stock, i.e. the size of the company, and the equity price relative to its book value, i.e. a measure of a company's market value. In particular, Fama and French claim that small and undervalued stocks tended to perform better through time and economic states than their large and overvalued counterparts. This means that a carefully assembled portfolio of stocks with such statistically properties would reward investors with a higher excess return than that offered by any other portfolio. They dubbed them "style" portfolios.

These findings were developed in the context of a single-equation, multi-factor model (Fama and French, 1993):

$$R_p - R_f = \alpha_i + \beta_p\ (R_m - R_f) + s_p\, SML + h_p\, HML + \varepsilon_p$$

The return on a portfolio of stocks R_p minus the risk-free rate R_f is a linear function of three factors: the return of the stock market portfolio R_m minus the risk-free rate of return R_f, the difference between the returns on a portfolio of small and large stocks, *SML*, i.e. small S minus M large L stocks, and the difference between the returns on a portfolio of high and low book-to-market stocks, *HML*, i.e. high H minus M low L valuation. The regression parameters are denoted $\alpha_{i,}$, β_p, s_p, and h_p. The error term is ε_p.

This short description does not do justice to the path-breaking work of Sharpe as well as Fama and French in which specific factors were developed and used to describe and give explanations for stock price behaviour. Much more detail can be added, but that will take us away from our main focus which Cochrane claims should be on connecting asset returns directly to the investor, his/her behaviour and choices. Hence, let us keep the single- and multi-factor pricing models for future references and move on to the consumption based capital asset pricing model as advocated by Cochrane.

2. Financial market facts and a story

For more than two decades, Cochrane has published numerous articles and books on asset pricing (1991, 1997, 1999, 1999a, 2000, 2005, 2005a, 2006, 2008, 2011, 2011a). Some are theoretical contributions, but the majority of his work ends up in the empirical arena. It is rooted in observed data from which Cochrane extracts what he refers to as “facts”. Let us first review a few of those fact-based statements before I let Cochrane tell us what he calls a *story* about asset pricing.

2.1 The financial market facts

Cochrane's first fact-based claim tells us:

> "Over the last century, the stock market in the United States has yielded impressive returns to its investors. For example, in the post-war period, stock returns have averaged 8 percentage points above Treasury Bills. Will stocks continue to give such impressive returns in the future? Are long-term average stock-returns a fundamental feature of advanced industrial economies? (...) How does the recent rise in stock markets affect our views of future returns? (...) In this article, I summarize the academic, and if I dare say so, scientific evidence on these issues." (Cochrane 1997, p. 3).

He continues:

> "The last 15 years have seen a revolution in the way financial economists understand the investment world. We once thought that stock and bond returns were essentially unpredictable. Now we know that stock and bond returns have a substantial predictable component at long horizons. (...). In this article I survey these new facts, and I show how they are variations on a common theme. (...). Each case suggests that financial markets offer rewards in the form of average returns for holding risks related to recessions and financial distress, in addition to risk represented by overall market movements." (Cochrane 1999, p. 36).

Cochrane reiterates:

> "Some assets offer higher average returns than other assets, or, equivalently, they attract lower prices. These "risk premiums" should reflect aggregate, macroeconomic risks; they should reflect the tendency of assets to do badly in bad economic times. I survey research on the central question: what is the nature of macroeconomic risks that drive risk premia in asset markets." (Cochrane 2008, p. 239).

Finally, Cochrane tells us:

> "What should investors do? An important current of academic research investigates how portfolio theory should adapt to our new view of the financial world. I summarize this research, and I distil the advice for investors." (Cochrane 1999, p. 59).

The four paragraphs of statements have several commonalities. *First*, Cochrane alludes to many of the same concepts also used by the FT journalists. We hear about risk and return, stocks and bonds, and that their prices vary over time and across asset classes. In particular, we learn that risky stocks, over a long time horizon, have returned more to investors than riskless US Treasury Bills. [17] Cochrane portrays this as an observed statistical fact often referred to as a "risk premium" or "equity risk premium". [18]

Second, we also learn that asset returns have a "substantial predictable component". How does Cochrane know? He relies on statistical analysis:

> "We are not only concerned with the average return on stocks but whether returns are expected to be unusually low at a time of high prices, such as the present. The first and most natural thing one might do to answer this question is to look at a regression forecast." (Cochrane 1997, p. 7).

But not only that: "The most obvious approach to these questions is of course statistical. What is the evidence on past stock and bond returns?". (Cochrane 1997, p. 4). Asset class return, or excess returns such as the equity risk premium, can thus be regressed on selected explanatory factors. Cochrane continues:

> "The central technique is simple forecasting regression: if we find $|b| > 0$ in $\mathbf{R_{t+1}} = a + bx_t + \varepsilon_{t+1}$, then we know that $\mathbf{R_{t+1}}$ varies over time. The forecasting variable $\mathbf{x_t}$ typically has a suggestive business cycle correlation. Expected returns are high in "bad times", when we might well suppose people are less willing to hold risks." (Cochrane 2008, p. 244). [19]

[17] US Treasury Bills, also known as T-Bills, are used by the US Treasury Department as a short-term debt financing instrument to borrow money from the public, ie investors. Their repayment and interest payments are guaranteed by the "full faith and credit" of the US Government.

[18] I will have more to say with respect to this "premium" later in this chapter. Cochrane tells us, for example, that in the period from 1947 to 1996, the annual average return on S&P 500 after inflation, i.e. "real" was 9.5% with a standard deviation of 16.8%. Over the same period, the "risk free" US Treasury Bills returned 0.8% p.a. after inflation with a standard deviation of 2.6% p.a. Subtracting the return on Bills from that of S&P 500 gets us the "equity risk premium" of 8.7%. Another way of viewing the risk premium is through the so-called Sharpe Ratio (SR). It is calculated by deducting the risk free US Treasury Bill return from the return of the asset under review and dividing the result by the standard deviation of the asset return. In this case the SR equals 0.51, i.e. (9.5 – 0.8)/16.8. The higher the ratio, the better the risk adjusted return is.

[19] In this linear function, R_{t+1} is the expected stock market return in the next period $_{t+1}$, x_t is the current forecasting variable, a and b are parameters. or coefficients, while ε_{t+1} is the error, or disturbance term.

As I mentioned earlier in the context of factor-models, much of the asset pricing literature has been focused on how the return can be "explained" or as Cochrane prefers, "summarized" by one or several of these variables.

Third, statistical analysis of time series data is suitable for delivering descriptive facts and analysing patterns in data. Cochrane, however, is of the opinion that such analysis is of limited use when trying to understand asset price movements. Something more fundamental is missing:

> "Statistical analysis of past returns leaves a lot of uncertainty about future returns. Furthermore, it is hard to believe that average excess return are 8 percent without knowing why this is so. Perhaps more important, no statistical analysis can predict if the future will be like the past. Even if the true expected excess return was 8 percent, did that result from fundamental or temporary features of the economy? Thus, we need an economic understanding of stock returns." (Cochrane 1997, p. 12).

This is a crucial statement as it introduces the need for a theoretical and fundamental economic understanding of asset price behaviour. Implicitly, it criticises factor-based asset pricing models which rely exclusively on statistical analysis.

Fourth, Cochrane seeks outlets for these asset return facts. In particular, he would like to use them as a basis for giving advice to the various stakeholders, i.e. investors, public policy makers or fellow academics. This advice could thus qualify as knowledge for use.

2.2 The financial market story

The financial market facts that Cochrane alludes to need explanation, and he has just told us that neither descriptive statistics nor regression analysis suffice. I have already indicated that Cochrane, for these reasons, has chosen a different kind of explanatory path. Specifically, he seeks to connect observable asset price and return data with macroeconomic aggregates such as consumption, production and investment. But these aggregates are sums of individual investors' decision making and, ultimately, choices about consumption and which assets to hold in their portfolio. Hence the importance of a microeconomic foundation firmly rooted in the individual investor becomes evident.

Before I turn to how Cochrane formally intends to improve our economic understanding of stock returns by presenting supporting theories and the model "M" in CCAPM, let us first review how he frames that discussion with the use of what he alludes to as *stories*:

> "Many superficially plausible stories have been put forward to explain historically high return on stocks and the time-variation of returns. Economic models and theories make these stories explicit, (...). Few stories survive this scrutiny." (Cochrane 1997, p. 12).

Here, Cochrane makes references to how *stories* are used to explain and the importance of confronting the model-based claims against the empirical situations. Later in *Chapter Four, sections 1.2.2* and *4.4* as well as in *Chapter Five, section 5.2*, I will discuss the methodological importance of stories in connecting the CCAPM to real situations. Let us now turn to the narrative.

As a starting point, consider two individual stocks, for example those of Google and Caterpillar, and then read Cochrane's very informative *story* about he refers to as "recession proof stocks":

> "One of them does well in recessions while the other does poorly. Clearly, most investors prefer the stock that does well in recessions, since its performance will cushion the blows to their current income. If lots of people feel that way, they bid up the price of that stock, or equivalently, they are willing to hold it at a lower average return. Conversely, the pro-cyclical stock's price will fall or it must offer a higher average return in order to get investors to hold it. In sum, we should expect that pro-cyclical stocks that do well in booms and worse in recessions will have to offer higher average returns than countercyclical stocks that do well in recessions, even if the stocks have the same market beta. We expect that another *dimension of risk* – covariance with recessions – will matter in determining average returns. What kind of additional factors should we look for? Generally, asset pricing theory specifies that assets will have to pay high average returns if they do poorly in "bad times" – times in which investors would like their investments not to perform badly and are willing to sacrifice some expected return in order to ensure that this is so. Consumption (or, more generally, marginal utility) should provide the purest measure of bad times. Investors

> consume less when their income prospects are low or if they think future returns will be bad. Low consumption thus *reveals* that this is indeed a time at which investors especially like portfolios not to do badly, and would be willing to ensure that wish. Alas, efforts to relate asset returns to consumption data are not (yet) a great success. Therefore, empirically, useful asset pricing models examine more direct measures of good times and bad times." (Cochrane 1999, p. 39).

Cochrane refines the important *story*:

> "Good assets pay off well in bad times when investors are hungry. Since investors all want them, those assets get *lower* average returns and command higher prices in equilibrium. High average return assets are *forced* to pay those returns, or equivalently to suffer low prices, because they are so "bad" – *because* they pay badly precisely when investors are most hungry. (...) To make these ideas operational, we need some procedure to measure the growth in marginal value of wealth or "hunger" (...). The traditional theories of finance, CAPM, ICAPM, and APT, measures hunger by the behaviour of large portfolios of assets. (...) Research connecting financial markets to the real economy (...) goes one step deeper. It asks what are the fundamental economic determinants of the marginal value of wealth? (Cochrane 2008, pp. 240).

The *story* about "recession proof" stocks leads directly to the following advice:

> "You have to buy stocks or long-term bonds at the bottom, when stock prices are low after a long and depressing bear-market, in the bottom of a recession or the peak of a financial panic. This is a time when few people have the guts or the wallet to buy risky stocks or risky long term bonds. (...) You have to buy stocks that everyone else thinks are dogs. Then you have to sell stocks and long term bonds in good times when stock prices are high relative to dividends, earnings and other multiples... (...) You have to sell the popular stocks, with good past returns, good sales, and earnings growth. (...) If this feels uncomfortable, what you are feeling is risk. If you're uncomfortable watching the market pass you by, perhaps, you *don't* really only care about long-run mean and variance, you also care about doing well when the market is doing well." (Cochrane 1999, p. 54).

The FT journalists and Cochrane present well-rehearsed narratives. They even make similar analytical comments. They share the ambition to explain asset prices and returns by linking these data points to particular events, or, in the case of Cochrane, general tendencies such as the demand for recession proof stocks. While the journalists, however, tell a fairly straight-forward *story*, Cochrane is willing to dig a bit deeper. Prominently, he introduces an investor. At the same time, the investor is also a consumer. In other words, he/she connects investments with its assumed final purpose, i.e. consumption. Next, this agent is placed in a financial market context in which he/she reflects on his/her current and future consumption, and which portfolio of assets to hold. We are told that this leads to an "equilibrium" situation and "hunger", i.e. marginal utility, takes the investor there with the support of the "recession proof" stocks. Cochrane thus has an internally consistent and almost all encompassing *story*. But we were also told up-front that the: "...consumption based model does not work very well." (Cochrane 2005, p. 43). I discuss why this is the case in *Chapter Three and Four,* but, in particular, in *Chapter Five*.

The above extracts indicate Cochrane's view that asset return data can be described, explained and predicted. Cochrane is mindful of the difficulties involved in identifying the "forecasting variables" that may be used to explain and how to think about them. With this, Cochrane reminds us that not only should the *stories* "work well", i.e. be statistically successful when compared with the available data, but they should also be "convincing". These two aspects of model-based claim evaluation will grow in importance and, finally, be discussed in *Chapter Five, section 5*. But first thing first.

Cochrane has so far extracted what he refers to as empirical facts from the financial market data. He has also told us a *story* about the investor's search for recession proof stock returns that would enable him/her to uphold a steady consumption pattern when economic times are bad. As I continue the discussion on the CCAPM in the next three chapters, and in particular Cochrane's version of it, I will have more to say about how financial economists measure financial market activities and how they use the available time- and cross-sectional data points to extract empirical facts. The *stories* that financial economists tell in order to connect their theories and models to the real situations will remain a main topic in *Chapter Four, sections 1.2.2* and *4.4* as well as in *Chapter Five, section 5.2*.

Let me now turn to the theories that underlie the story-based narrative. In this myriad, I focus on three main elements, i.e. the investor, his/her utility and his/her attitude towards risk. In the next section, I show how these theories are made "explicit" in the equations of the consumption based asset pricing model.

3. Investor, utility and risk

Cochrane's *story* identifies the investor and his/her demand for recession proof stocks. The topic of this section is to review who the investor is, and how his/her behaviour is described. The answers take us into three main topics, i.e. rational decision making, utility and risk preference. This section is thus relevant for my discussion in *Chapter Three* on CCAPM's theoretical foundation.

The review of these topics allows me to introduce basic notations, definitions and concepts that the reader will encounter throughout this thesis. Once they have been established, I can move on to the next *section 4* that puts these concepts into a coherent, equation-based framework of the consumption based capital asset pricing model.

3.1 The Investor

In Cochrane's *story*, and as a consequence, its theories and models, the investor gets a lot of attention:

> "An investor must decide how much to save and how much to consume, and what portfolio of assets to hold. The most basic pricing equation comes from the first order condition for that decision. The marginal utility loss of consuming a little less today and buying a little more of the asset should equal the marginal utility gain of consuming a little more of the asset's payoff in the future. If the price and payoff do not satisfy this relation, the investor should buy more or less of the asset." (Cochrane 2005, p. 3).

Here, we learn two things: investors exist and they make two related decisions. For now, let us assume that the investor is a person just like you and me and focus on his/her decision making behaviour. Cochrane approaches the behavioural topic by focusing on an investor's preferences with respect to deciding between consumption now or later and which portfolio of assets to hold. An essential first step in the derivations to come is that investors are assumed to be "rational" in their decisions. To this end, the preferences of investors should satisfy a number of axioms, which give formal mathematical expressions to fundamental aspects of behaviour and attitudes towards the objects of choice (Jehle and Reny 2011). Let us briefly review the axioms:

The *first* axiom tells us that payoffs on any asset, hence consumption of goods and services, in any two periods can be compared. The investor may, therefore, *strictly prefer* consumption in one period rather than in the other; he/she can be *indifferent* towards either; or he/she can *weakly prefer* one over the other. Such preferences are referred to as being *complete*. The investor is thus always able to express a preference between available alternatives, i.e. consumption now or later. *Second*, it is assumed that if we have two strategies that result in exactly the same consumption pattern, the consumer is indifferent between the two. In these situations, preferences are said to be *reflexive*. *Third*, if one alternative is preferred to a second, and the second is preferred to a third, then the first is also preferred to the third. Economists say that these preferences are internally consistent, i.e. *transitive*. *Fourth*, the agent prefers more of anything rather than less of it. This is captured by the axiom of *strict monotonicity*. *Fifth*, if bundle A is at least as good as B and bundle C is very close to B, then A is also at least as good as C. In other words, preferences and indifference curves are assumed to be *continuous. Sixth*, it is better receiving a little bit of several alternatives rather than lots of only one, i.e. averages are preferred to extremes. This is the *convexity* axiom, which is the most critical assumption of consumer theory because it implies that consumers are willing to trade-off some of one good to get more of another, e.g., substituting current consumption for future consumptions.

The six consumer preference axioms are essential for Cochrane's model building efforts. Predictions of behaviour can be formed based on these axioms using the process of mathematical derivations. The axioms require that consumers only make binary comparisons, i.e. they only examine two consumption plans at a time and make a decision regarding those two. Other aspects to having a rational investor included, is that he/she will act, rationally, according to his/her own, and nobody else's preferences and beliefs, i.e. self-interest, and, finally, that on average, his/her beliefs are correct. These are strong assumptions. I discuss them in a philosophical context in *Chapter Three, sections 1 and 2*.

3.2 Inter-temporal consumption choice and utility

But investor's behaviour and decisions are governed by more than just rationality. There are also practical issues to consider. In particular, his/her pecuniary means are restricted. Robert E. Lucas, an early advocate of consumption based asset pricing, calls this initial budget, an "endowment" (Lucas 1978). Cochrane often uses this term but also, interchangeably, makes references to "income" and "wealth" as limiting restrictions. [20] A second practical issue is the time-frame of deploying the endowment. The investor is asked to decide how the endowment is spent across time, for simplicity two periods, now or later. Economists refer to this decision as *inter-temporal*. Thus, Cochrane formalizes the investor's rational preferences mathematically in such a way that current and future consumption are both considered given the available budget.

Economists use the so-called *utility function* to model the preferences of individuals. The mathematical function has as inputs certain variables, in this case current and future consumption. It gives the total level of utility, i.e. "satisfaction" or "wellbeing" associated with those inputs. For simplicity reasons, Cochrane typically imposes a convenient structure on the total utility from consumption (Cochrane 2005, p.4):

$$U(c_t, c_{t+1}) = u(c_t) + \beta E_t [u(c_{t+1})]$$

CCAPMs standard utility function $U(c_t, c_{t+1})$ tells us that the total utility U of current consumption c_t and the next period's consumption c_{t+1} is a linear combination of the utility of the current consumption $u(c_t)$, and the current expected utility E_t from the next

[20] Similar uses are found in Sharpe but not in Fama and French's single- and multi-factor models.

period's consumption (c_{t+1}) multiplied by a parameter β [21]. Both the value of parameter β and the choice of an appropriate functional form for the current utility $u(c_t)$ are crucial building blocks for the solution of the CCAPM.

Financial economists call the β parameter the *subjective discount factor*. The subjective discount factor tells us how important an individual's utility derived from expected future consumption is, when it is compared to the known utility of current consumption. It makes the expected utility from future consumption comparable with the known utility of consumption today. It thus *discounts* or brings the future utility back to its present value. In a sense, it reveals an investor's subjective impatience in terms of "waiting to consume". For example, if the investor rather consumes all his/her current income today than invest in a portfolio of assets that potentially generates payoffs for a higher level of future consumption, his/her impatience is extraordinary high. By implication, he/she demands a high expected future payoff, or return, for delaying consumption. Or, in other words, the price he/she is willing to pay for this future payoff is low.

The choice of functional form for assessing the investor's utility is also relevant because it models how strongly an investor prefers his/her consumption stream to be stable over time. One specific form for the utility from current consumption $\boldsymbol{u(c_t)}$ is particularly popular because of its technical and empirical properties, i.e. its ability to capture certain stylized facts with respect to financial market returns. This function is called the *power utility* function. It is given by the formula (Cochrane 2005, p. 4):

$$u(c_t) = \frac{1}{1-\gamma} c_t^{\;1-\gamma}$$

The power utility of current consumption $u(c_t)$ operates with the parameter γ which is an estimated value of the investor's subjective risk aversion. This particular function is also often referred to as *constant relative risk aversion* (CRRA). The power utility function is

[21] Not every function can be a valid utility function. Most importantly, the utility function should reflect rational preferences as described above. For example, having $u(c_t) = - c_t$ would violate strict monotonicity as described earlier in axiom four.

an important concept that I will revisit throughout my thesis. In *section 3.5*, I discuss its properties and in *section 6.2* its confrontation with the empirical situations. Before I get there, however, let me first proceed to a few central concepts related to the utility itself.

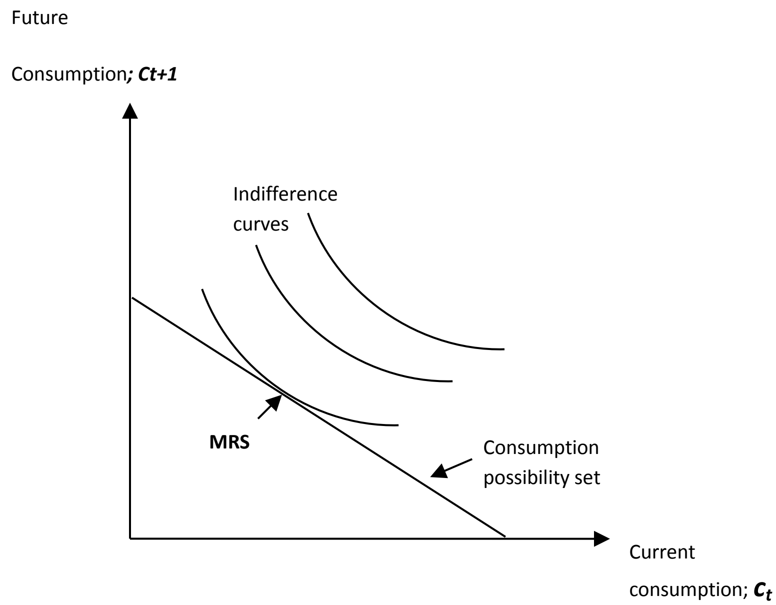
3.3 Marginal rate of substitution

From the utility function and the assumption of a rational preference for distributing consumption over time, economists often derive what they call *indifference curves* in a standard x-y diagram. The x-axis shows the amount of current consumption and the y-axis the amount of future consumption. Several such indifference curves can be drawn, and the further "out" they lie in the diagram, the higher the level of total utility.

Graphically, an indifference curve is negatively sloped and convex to the origin. This indicates that the investor can "trade-off" current and future consumption along any particular curve. Since every point on a single curve gives him/her the same *total* utility, the investor is said to be indifferent towards the split between the timing of consumption. This is noteworthy because it also indicates that the investor's decision is not to consume either now or later, but related to the optimal allocation between the two alternatives. In other words, the investor does not specialize in any of the two outcomes but seeks a balanced consumption pattern over time and in varying economic states.

However, as I mentioned earlier, there is the practical consideration of a limited budget – or endowment. It is represented by a straight line drawn between a vertical intercept at the y- and a horizontal intercept at the y-axis. The budget-line represents a set of current and future consumption bundles that exactly exhaust the available endowment, i.e. the consumption possibility set. The larger the budget, the further "out" the budget line can be drawn. At the point at which the budget line is tangential to an indifference curve, the investor has maximized his/her total utility and the optimal allocation between current and future consumption is given.

Figure 1: Trade-off between current and future consumption



In the tangential point, there is an exact "exchange-rate" between consumption now and later. Economists call this rate the *marginal rate of substitution*, or $MRS = - dc_t \ / dc_{t+1}$. It is the maximum amount of current consumption that the investor is willing to give up now in order to obtain one additional unit of consumption in the next period. In mathematical terms, it is the absolute value of the slope of the line that is tangent to the highest indifference curve the investor can achieve given his/her budget constraint.

Another noteworthy aspect of the convex indifference curves is that MRS decreases as c_{t+1} increases. This is referred to as the *diminishing marginal rate of substitution* which implies that the investor wants to smooth consumption over time. An investor's marginal utility is, therefore, closely related to the MRS concepts. To be precise, the MRS equals the ratio of the marginal utilities of the future and current consumption. It considers how an investor's utility changes as he/she marginally trades out of consumption now for later. As I will discuss later in *Chapter Four* on the model "M" in CCAPM, Cochrane is adamant about the importance of marginal utility, i.e. the "hunger" for the next bite.

3.4 Investment risk and contingent consumption

In *section 3.2* above, I introduced the CRRA class of consumption function, and circled in on the favoured power utility version. One central element is the investor's attitude towards risk. In this section, I continue the discussion on investment risk. I first show how financial economists view this risk in general before I, in the next section, turn to an investor's risk preference and how it is described in the utility function.

Risk and uncertainty are two terms that are often encountered in this context of asset pricing. The terms are, however, not the same. Frank H. Knight reminds us that risk refers to events to which we can assign probabilities while this is not possible in cases of uncertainty (Knight 1921). Consider, for example, a coin toss. If we know that the coin is fair, and not rigged, we are exposed to risk, because we can assign probabilities to the two possible outcomes when the coin is tossed. In these situations, we can list the possible outcomes and know the likelihood of each occurring. If we do not know whether the coin is fair or not and if so to which side, we are exposed to uncertainty because the outcome of the toss is uncertain and we cannot assign probabilities to them. In such situations, we know the possible outcome, but the likelihood of each is unknown.

As uncertainty is challenging to work with because the expectation E_t in equation $U(c_t, c_{t+1}) = u(c_t) + \beta E_t [u(c_{t+1})]$ is not defined, most financial economists, including Cochrane, consider only risk in their asset pricing research effort. Risk is defined as the difference between the expected and the realised payoff from an investment. This difference does not provide a good measure of risk because the outcome is sometimes positive and sometimes negative. Financial economists, therefore, choose to calculate the standard deviations of the actual investment returns. The standard deviation is the square root of the weighted average of the squares of the deviations of the payoffs. The measure makes it possible to compare the return variability of, for example, the stocks of Google and Caterpillar. Since the historical return of Google has a higher standard deviation than that of Caterpillar, investors consider Google to be a "riskier" investment.

The choice to model risk as a probability weighted outcome has implications. And as we shall see, uncertainty as a concept remains a critical element in the modelling of the investor and his/her behaviour. Consider again the investor and his/her dual decision, i.e. consume now and later and the content of his/her investment portfolio. Since he/she is inclined to spread consumption over time, he/she will always allocate savings to an investment portfolio. But how does this portfolio look like? In order to choose between which risky assets to hold, the investor must be clear with respect to three issues. *First*, he/she needs to forecast the future payoff on each individual assets in the available investment universe in each possible future state of nature; *second* compute the individual probabilities of realization for each state of nature; and, *third*, calculate the probability weighted expected payoffs. Thereafter, investors are in a position to rank all outcomes from the highest to the lowest state contingent payoff and rationally choose assets for their investment portfolio from within that set. This is a formidable task, to say the least.

In order to overcome the challenges listed in the previous section, Cochrane makes several assumptions. He assumes that the investor knows the payoffs on all assets in all possible future economic states. The investor is also given perfect foresight with respect to all possible future economic states. Not only that. Also the probabilities of their occurrence are assumed to be known to the investor. The expected payoffs, therefore, connect with the states and create easy to calculate vectors of state-contingent payoffs, i.e. consumption opportunities for investors to consider. Since these states of nature are mutually exclusive, only one will be realized. For example, two different states such as an economic boom and a recession cannot happen at the same time in the same place. The investor, however, does not know which of these states will occur. This is the only element that remains unknown to him/her at the time of the decision to consume and save and what portfolio of assets to hold. Obviously, this lets the future payoff on his/her investment portfolio become uncertain. The final payoff, hence future spending level and ultimately consumption, therefore, is state contingent. His/her given ability to foresee certain aspects of the future helps reduce uncertainty, but not all of it. This conclusion of "near to perfect knowledge and foresight" will be of continued importance as I advance my discussion of the investor (see, for example, *Chapter Three, section 1.3.2* and *section 1.4*).

Given his/her assumed knowledge and foresight, the investor is now in a better position to decide which assets to hold in the portfolio. A quick glance at the available investment universe which now is ranked from the highest to the lowest payoff seems like a good guide. Choosing the "best" assets are, however, not straightforward. The leading stock in terms of probability weighted expected payoff, for example, might be extremely risky in the sense that if a particular state materializes, its payoff disrupts the investors desire to optimally distribute consumption over time and towards different economic states. His/her subjective preferences for risky payoffs must, therefore, be considered. This is the topic for the next section.

3.5 Risk preference

In a situation of state dependent future consumption, the investor is confronted not so much with the choice between particular stock names such as Google and Caterpillar, but more with monetary payoffs and the likelihood of their occurrence. In this respect, the financial economists tell us that the investor does not choose between individual stock names but rather between probability distributions of expected payoffs. These distributions produce *expected* utility.

Expected utility is used by economists in the context of choices with uncertain outcomes. The idea behind the expected utility theorems was first given by John von Neumann and Oskar Morgenstern in their *Theory of Games and Economic Behavior* (1944). Therein, a few basic theorems provide a set of hypothesis under which an investor's preference ranking may be represented as a combination of the expected payoffs, on one hand, and their respective probabilities on the other. In an economic context, the expected utility of any asset is then the weighted mean of ex-post utilities with the state probabilities as weights. A real number is thus attached to each asset and ranked in a so-called "utility-index". Von Neumann and Morgenstern formulated this theory-based on the concept of simple “lotteries”. Such a lottery can be denoted (x,y,Þ). It offers the participant a payoff x with a probability Þ or a payoff y with probability 1- Þ.

We see that this is also what Cochrane uses when portraying the uncertain payoff of risky assets. Von Neumann and Morgenstern’s theorems ensure that a utility function for the investor can be entertained on the lottery space, or in Cochrane’s context, over uncertain payoffs.

Investors do not have the same tolerance towards uncertain payoffs. Some want stable and reoccurring investment income in order to support future levels of consumption. Others seek the excitement from the highest level of possible payoff and are willing to bet on that happening. And yet a third is indifferent to either. These different attitudes have to do with investors individual or subjective preferences towards risk. Cochrane, as we have seen, assumes that the investors, in general, belong to the first group. He/she extracts wellbeing from a smooth consumption path across time and different economic states. Economists refer to him/her as being *risk averse*. The assumption of risk-aversion is also motivated by experimental psychology research and patterns observed in financial markets. Representatives from the second and third groups of investors portray a preference for *risk taking* and *risk neutrality*.

Cochrane choice to let the investor be of the risk averse type is important and this concept will also stay with us as we proceed through my thesis (see *Chapter Three, section 1.1.2*). From a technical perspective, Cochrane now needs to incorporate risk aversion into the chosen power utility function. Earlier I mentioned several notable properties of such utility functions. It tells us, for example, that the investor wants more consumption or wealth rather than less (as dictated by axiom four about strict monotonicity). As total consumption or wealth increases, his/her expected utility increases as well. Technically, this means that the first derivative of the utility function over, for example, wealth W must be positive $U'(W)>0$. It also tells us that marginal utility decreases with greater W because the second derivative of Cochrane’s favoured form of the utility function is negative, i.e. $U''(W)<0$. This concept is called concavity, i.e. a measure of a function’s curvature. This is at the same time a necessary and sufficient condition for risk aversion. Again, consumption smoothing and risk aversion go hand in hand and can be expressed by utility functions that exhibit decreasing marginal utilities. It is also possible to measure the degree of an investor's risk aversion by looking at the curvature of the power utility function.

To be more concrete, let us consider which restrictions power utility puts on the risk averse investor, or posed another way, since the investor receives utility from the uncertain payoff from investing in risky assets and wants to uphold his/her consumption pattern, how much will he/she be willing to pay for that investment opportunity?

For illustrative purposes consider, once more, the utility function – here in terms of investor's wealth:

$$u_t(W_t) = \frac{1}{1-\gamma} W_t^{\;1-\gamma}$$

Assume that the investor has an initial endowment of USD 100 and is looking at an investment opportunity. The payoff is dependent upon the realization of one of two future economic states. Depending upon which of two states will materialize, he/she will be paid either USD 50 or USD 100. There is a 50/50 chance that one of them will be realized, i.e. Þ equals 0.5. Economists often refer to this as a "fair bet". In this case, the investor knows that the expected, average, future payoff is going to be USD 75 (0.5*50+0.5*100). A risk neutral investor would be willing to pay exactly USD 75 while a risk taker would even pay more since he sees an upside in gaining USD 100, ending up with maximum USD 200 (initial endowment plus the maximum return). A risk averse investor, however, will never pay more than USD 75. He/she rather pays less. But how much less? The answer is dependent upon both the value of γ , i.e. the risk aversion parameter and the level of his/her wealth W. We can now define various levels thereof and calculate the price:

$$\frac{W+CE^{1-\gamma}}{1-\gamma} = 0.5* \frac{(W+50)^{1-\gamma}}{1-\gamma} + 0.5* \frac{(W+100)^{1-\gamma}}{1-\gamma}$$

In this equation, CE is the price, or the certainty equivalent, as many economists call it. The CE is the maximum amount the investor wants to pay to enter this lottery. Letting both γ and W be zero, an investor will pay exactly 75 for entering into this bet. His/her behaviour is thus governed by risk neutrality. If we increase his/her risk aversion by setting γ to 5 and keeping W at zero, the price will be 58. If the wealth is also increased, for example, to USD 100, the price he/she is willing to pay for the uncertain payoff is USD 66.

We make the following observations: *First*, there exists an optimal numerical value at which any investor will enter into a risky transaction. A risk averse investor will always pay less than the probability weighted expected payoff. *Second*, keeping the risk aversion parameter constant, the higher his/her wealth, the higher the price he/she is willing to pay for the risky payoff. This is because power utility implies that the absolute risk aversion is declining in wealth. Consequently, a wealthy investor is willing to pay a higher price because he/she demands a lower risk premium. *Third*, the functional form of power utility embedded in the von Neumann and Morgenstern's expected utility function synchronizes the knowledge regarding the distribution of future payoffs with that of the subjectively held view by the risk averse agent. *Fourth*, the difference between the probability weighted expected payoff, i.e. USD 75 and the price the investor is willing to pay can be regarded as a safety margin, i.e. a risk premium that is meant to compensate the risk averse investor for entering into a fair bet.

In sum, the investor is rational, risk averse and utility maximizing individual. He/she applies these characteristics across two decisions. *First*, since the investor prefers spreading consumption over time, he/she decides to postpone some consumption for later. The unspent endowment budget thus reduces current level of spending. The difference is saved for future consumption. Since savings equals investment, the investor faces his/her *second* decision, i.e. what portfolio of assets to hold. This portfolio may be investments in financial assets ranging from a risk-free interest bearing savings account in a nearby commercial bank or in more risky assets such as stocks. The payoff at the end of the investment period for the risk-free deposit is known in advance. This is not the case for risky assets such as stocks. These assets have uncertain payoffs in the sense that neither their end-of period price nor their, possible, dividends can be foreseen. This payoff uncertainty worries Cochrane's rational agent. He/she is, therefore, said to be risk averse. When making decisions, Cochrane assumes that the rational, risk averse investor maximizes his/her utility across time and economic states subject to the available budget, i.e. endowment. We shall later see in *section 5* that this set-up together with some additional assumptions leads to an equilibrium situation in the financial markets. Finally, the concept of the rational, risk averse and optimizing investor is important for my discussion in the upcoming chapters (see, in particular, *Chapter Three, section 1.1.2*, and *1.3*, as well as, *Chapter Four, section 3*).

It is now time to put these concepts, definitions and notations into context. In the next section, I show how Cochrane uses them to develop his consumption based asset pricing model. This will continue lay the foundation for my discussions in *Chapter Four.*

4. The consumption based asset pricing model

Earlier, in *section 2.2*, I quoted Cochrane's *story* about the representative investor's demand for financial market assets in the form of recession proof stocks. It is the role of the consumption based asset pricing theory and model, Cochrane tells us, to make this *story* "explicit". Below I summarize how Cochrane does just that. I describe and derive the consumption based model (CCAPM) using the definitions and notations I introduced above in *section 3*. The framework introduced here gives us a structure that is valid for any investor. In other words, Cochrane does not make any assumptions besides rationality and the form of the utility function of an investor. In the following *section 5*, I explore the consequences thereof when I make some simplifications to aggregate outcomes across all investors. *Sections four* and *five*, as a result, are the two pillars I need to initiate a philosophical discussion the model "M" in CCAPM in *Chapter Four*.

4.1 The basic pricing equation

The CCAPM is formulated in a mathematical structure. Although much is requested from the readers with respect to technical dexterity, Cochrane seeks to reduce the *story* about the demand for risky assets and their pricing to something "simple": "Asset pricing theory stems from one simple concept: price equals expected discounted payoff." (Cochrane 2005, p. xiii). The portrayed simplicity is expressed in what he denotes the *basic pricing equation* (Cochrane 2005, p.6):

$$p_t = E_t\,(m_{t+1}\ \ x_{t+1})$$

In the equation, p_t is the current price an investor observes in the market place for a risky asset such as Google or Caterpillar – or even a stock market index such as the S&P 500. This price is the product of the current expected value E_t of a future discount factor m_{t+1} and a future payoff x_{t+1}. Both x and m are stochastic. Cochrane tells us that the payoff x_{t+1} is the sum of the uncertain value of a financial asset at the end of the next period plus a dividend (if any) paid out in the meantime. The other term m_{t+1} is called the "*stochastic discount factor*" (SDF). Since the basic pricing equation indicates the current value of a payoff in a specific realized future scenario, the SDF is different for different future economic scenarios. In *section 4.3*, I return to the SDF.

Next Cochrane asks what this future random payoff is worth to a rational, risk averse and utility maximizing investor. To this end, he uses the power utility function and form as introduced above in *section 3.2*:

$$U(c\ ,\ c_{t+1}) = u\,(c_t) + \beta\, E_t\ [\,u\,(c_{t+1})\,]$$

$$u\,(c_t) = \frac{1}{1-\gamma}\, c_t^{\ 1-\gamma}$$

Previously, I mentioned that the investor is concerned with the optimal distribution of consumption over time. How does he/she find this optimum? I discuss the answer to this question in the next section.

4.2 The first order condition and the central asset pricing formula

Cochrane assumes that the investor who is asked to value the random future payoff x_{t+1} can buy and sell as much of this payoff as he/she desires at the current price p_t given the available budget. How does his/her optimal solution look like?

If the investor does not desire to consume at a future point in time, which of course does not make sense for practical reasons, he/she would neither save nor buy any financial assets and would maintain the original consumption level equal to the available budget. On the other hand, reducing current consumption will allow him/her to buy *ð* units of financial market assets, such that he/she can consume the uncertain value of these *ð* units of financial market assets in the next period after selling of course. Under this constraint, Cochrane tells us that the investor would arrange his/her consumption plans in the following way (Cochrane 2005, p. 5):

$$\max_{ð} u(c_t) + E_t [\beta u(c_{t+1})] \text{ s.t.}$$

$$c_t = e_t - p_t ð$$
$$c_{t+1} = e_{t+1} + x_{t+1} ð$$

Cochrane next substitutes the given constraints into the objective and sets the derivative with respect to *ð* equal to zero. This gives the *first order condition* for the optimal consumption and portfolio choice – the two central choices the investor has to make in the consumption based asset pricing framework. We thus get (Cochrane 2005, p. 5):

$$p_t u'(c_t) = E_t [\beta u'(c_{t+1}) x_{t+1}] \text{ or}$$

$$p_t = E_t \left[\beta \frac{u'(c_{t+1})}{u'(c_t)} x_{t+1}\right]$$

Cochrane denotes this important final equation the *central asset pricing formula*. I will make several references to it throughout my discussions in the upcoming chapters. For example, it will dominate my review of the importance of assumptions in *Chapter Three, section 2.1*, throughout *Chapter Four* on the asset pricing model itself, and, finally, in

Chapter Five, section 3 and 4 on model justification and the use of unrealistic assumptions. [22]

The equation tells us that the investor sacrifices current consumption for an uncertain future consumption. As long as the expected value-gain from the discounted future utility exceeds that of the current utility sacrifice in terms of foregone consumption, his/her total utility increases. Given the concave and increasing form of the utility function, sacrificing current consumption now becomes increasingly painful for the investor and the discounted future utility gain adds less and less. At some point, the gain and the sacrifice exactly offset each other. At that point, no further improvements can be made. This is exactly when the central asset pricing formula holds. It is referred to as the investor's first order condition.

4.3 The stochastic discount factor

In the *basic pricing equation* referred to above in the previous *section 4.1*:

$$p_t = E_t\,(m_{t+1}\;\; x_{t+1})$$

we find m_{t+1}, or, as Cochrane denotes it, the *stochastic discount factor* (SDF). It is the most important consumption based capital asset pricing concept, and it can be found again in Cochrane's *central pricing formula* (Cochrane 2005, p. 6):

$$m_{t+1} = \beta \frac{u'(c_{t+1})}{u'(c_t)}$$

The SDF is the subjective present value of the expected *marginal* utility coming from the future and current consumption. [23] For known periodic payoffs, such as the one an

[22] The *central asset pricing formula* shows utility as a derivative, i.e. [$\boldsymbol{u'(c_t)}$] and [$\boldsymbol{u'(c_{t+1})}$]. The apostrophe indicates that it is not the absolute level of utility, i.e. wellbeing but the next "bite", i.e. the marginal improvement in wellbeing which is subject to optimisation – or as quoted from Cochrane in *section 3.1*. earlier: "…from the first order condition for that decision. The marginal utility loss of consuming a little less today and buying a little more of the asset should equal the marginal utility gain of consuming a little more of the asset's payoff in the future."

investor could earn by placing money in an interest bearing savings account at a safe bank, i.e. the risk free rate R^f , the calculation is rather trivial. We have:

$$p_t = \frac{1}{R^f} x_{t+1}$$

The price of a riskless asset is thus the present value of its future known payoff. [24] The value of m_{t+1} is then the inverse of the risk free interest rate.

It stays mechanistic, but the complexity increases when neither the future stochastic discount rate nor the future payoff can be determined with certainty – as is the case in the context of Cochrane's CCAPM. The beauty of Cochrane's mathematical structure becomes visible. The SDF is generic and is defined by the *subjective discount factor* β and the *risk aversion* parameter γ and does *not* depend on the asset in question. The SDF is thus a characteristic of the investor. Yet, we can use it to obtain *asset specific* expected returns or discount rates for any asset traded in the financial market. If we have

$$p_t = E_t [m_{t+1} \; x_{t+1}]$$

then we can find a number R^i, that is different for every asset i such that (Cochrane 2005, p.7) :

$$p_t^i = \frac{1}{R^i} E_t (x^i_{t+1})$$

If, for example, Google's payoff is more uncertain than that of Caterpillar, the investor would assign a higher discount rate to Google, making the price he/she is willing to pay for the stock lower than that of Caterpillar. In this case, the Google stock's payoff is

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[23] See footnote 22 above.

[24] If for example the risk-free interest rate is 2% and the payoff of a government bond USD 100, then the price of the bond today is approximately USD 98 (100*1/1.02). In other words, if the investor invests USD 98 today he will receive USD 100 in the next period. Financial economists often refer to this as the "time value of money".

indeed considered riskier than that of Caterpillar. This risk adjusted stochastic discount factor, therefore, is the sum of the risk free interest rate and the particular risk premium associated with a specific stock.

The SDF adds flexibility to the asset pricing modelling task. Cochrane sees two main advantages. *First*, the SDF can incorporate any type of utility function. Modelling work is, therefore, not restricted to the power utility form. *Second*, the SDF can price any asset or asset class. The riskier a payoff is considered to be, the more risk premium is added to the risk free rate. In fact, assets with different degrees of riskiness should attract different asset specific discount factors. But there is still only one SDF and each asset specific discount factor is determined by it.

The SDF has been given different names. It is also often referred to as the "marginal rate of substitution". This term is often encountered in standard textbooks on microeconomic analysis.[25] In such textbooks, it measures the "price" at which a consumer trades, for example, apples against oranges. In Cochrane's context, this rate is also a price but the "goods" are current and future consumption. Other names for the SDF are "*state-price density*" and "*pricing kernel*".

So far, we have seen how Cochrane incorporates uncertainty into the model and how this risk can be priced. Nonetheless, there are other ways to incorporate this uncertainty. Below, I present this second variation, which, in the end, is more central for his research. Keep in mind that this is the other side of the same medal. The approach presented below works out the theoretical concept of the SDF better and shows explicitly why *one* SDF can generate *many* different asset specific discount factors.

[25] See for example: Varian (2009), Baily (2005), Copland, Weston, Shastri (2005), Danthine and Donaldson (2005), Cuthbertson and Nitzsche (2004), Lengwiler (2004), Mas-Collel, Whinston and Green (1995), Krebs (1990).

4.4 The covariance

With the intuition of the *story* concerning investor's demand for recession proof stocks and the utility gained from holding such stocks, Cochrane implicitly focuses on the covariance between the payoff from such an asset and the marginal utility of consumption. This topic will be addressed again when I evaluate Cochrane's contribution in a historical context in *Chapter Three, section 2.2.1*. In order to explain this crucial piece of the theoretical framework better, let us first explore a similar link in Sharpe's CAPM. [26]

Sharpe told us that the prices, hence returns, on any individual stock are given by the particular stock's covariance, or "beta", with the return on the stock market as a whole, i.e. the market portfolio. In good economic times, for example, the payoff from the market portfolio is expected to be positive. In such situations, some stocks will return more than the market portfolio while others will not. The outperformers have "betas" higher than one and are thus riskier than the lower than one beta stocks that underperform in a positive market environment.

Cochrane does not share Sharpe's view that investors focus on the relationship between risk and return in a portfolio context. Cochrane's investor focuses on preserving a stable consumption pattern over time and through different economic states. Therefore, the only reason why investors care about the performance of stocks or portfolios thereof is because their payoff uncertainty influences the level and volatility of future consumption. Hence, Cochrane takes the view that investors seek to smooth their consumption through time and across economic states. They dislike disruptions in spending pattern – both positive and negative.

Sharpe's high-beta stocks typically do not ensure this. They pay out in good economic times when their returns are not necessarily required because the investor's income from other sources is abundant. The marginal utility from that extra investment income is thus lower in good times when affluence reigns than in bad times. Cochrane's advice to

[26] For reference, see *section 1.3.*

investors is thus anti-cyclical. He urges investors to identify, buy and hold "recession-proof" stocks in their investment portfolios. It is thus expected from such stocks that they pay out exactly when this payoff is needed the most, i.e. during bad economic times. This payoff stabilizes income and contributes to maximizing utility from the trade-off between current and future consumption.

The relationship Cochrane establishes between investment payoffs on one hand, and consumption growth on the other, is thus a simple one. This can also be shown mathematically. Cochrane first asks us to consider the definition of covariance:

$$cov(m,x) = E_t\,(m_t\, x_t) - E_t\,(m_t)E_t\,(x_t)$$

From *section 4.3* above, we know that the risk free interest rate is used in connection with the payoff to give the present value of an asset. Using the covariance definition we thus get (Cochrane 2005, p.13):

$$p_t = \frac{E(x_t)}{R^f} + cov\,(m,x)$$

Cochrane explains that the price of any stock p_t equals the discounted present value of a known payoff $\frac{E(x_t)}{R^f}$ using a known risk-free rate, for example, a bank deposit interest rate R^f plus a mark-up or discount that is stock specific. The second term, i.e. cov, is called a *risk adjustment*: "An assets who's payoffs covaries positively with the stochastic discount factor has its priced raised and vice versa." (Cochrane 2005, p. 13). Cochrane offers the following equation to give his readers a better understanding of this risk adjustment by substituting m_t, i.e. the SDF in terms of consumption and gets the following (Cochrane 2005, p.13):

$$p_t = \frac{E(x_t)}{R^f} + \frac{cov\left[\beta\, u'(c_{t+1})\ (x_{t+1})\right]}{u'(c_t)}$$

Cochrane explains:

> "Marginal utility ***u´(c)*** declines as **c** increases because of the concavity embedded in the utility function. Thus, an asset's price is lowered if its payoff covaries positively with consumption. Conversely, an asset's price is raised if it covaries negatively with consumption." (Cochrane 2005, p. 13).

What does this analytical application of the basic pricing model imply? Cochrane again:

> "If you buy an asset whose payoff covaries positively with consumption, one that pays off well when you are already feeling wealthy, and pays off badly when you are already feeling poor, the asset will make your consumption stream more volatile. You will require a low price to induce you to buy such an asset. If you buy an asset whose payoff covaries negatively with consumption, it helps to smooth consumption and is more valuable than its expected payoff might indicate." (Cochrane 2005, p. 13).

Cochrane argues against Sharpe's conclusion in an intuitive way. In his view, an investor's utility stems from being able to upkeep his/her consumption-demand in dire economic times. The spending pattern, i.e. future consumption is, therefore, dependent upon the payoff from the stocks held in the investment portfolio. Stock prices should, in other words, reflect their ability to provide a pay-out during bad times. If the investor thinks this ability is high, he/she will pay-up to acquire those stocks. In the opposite case, prices will fall until the higher expected payoff compensates the investor for holding less than "recession-proof" stocks.

5. From individual optimization to equilibrium asset pricing

When developing and specifying the consumption based capital asset pricing model, Cochrane draws on a range of different concepts sourced to cover three broad categories, i.e. investor, financial market and financial assets. In combination, the three categories resemble a standard micro-economic setting in which economists place a rational agent in a choice situation with uncertain outcomes and ask him/her to maximize his/her wellbeing, i.e. utility. [27] Beyond this traditional economic setting, however, other concepts are particular to finance. They are primarily related to tradable assets and the way markets are set to operate.

In this section, I continue reviewing the theories underlying the CCAPM, but here I focus on Cochrane's effort to generate an equilibrium situation which incorporates the investor, the utility maximizing behaviour, and risk aversion. In particular, I am interested in how Cochrane aggregates an individual first order condition towards an equilibrium situation in financial markets. In order to get there, Cochrane makes several simplifying assumptions with respect to the investor, the financial market and the financial assets. This strategy of simplifying complex real situations is a main topic that I will discuss in *Chapter Three, section 1.3*.

Let me start with a short extract from Cochrane. It helps frame the following discussion.

Cochrane claims:

> "Writing ***p = E(mx)*** we do *not* assume: (1) Markets are complete, or there is a representative investor; (2) Asset returns or payoffs are normally distributed (no options), or independent over time; (3) Two-period investors, quadratic utility, or separable utility; (4) Investors have no human capital or labour income; (5) The market has reached equilibrium, or individuals have bought all the securities they want to. All of these assumptions come later, in various special cases, but we *have not* made them yet." (Cochrane 2005, p. 35).

This extract has two separate parts. *First*, there is the basic pricing equation which we encountered in the previous *section 4.1*. We recall it states that the price an investor is willing to pay for an asset is equal to its expected discounted payoff. It goes without saying that in this form, the equation does not yet contain any of the assumptions that Cochrane, later, allocates to it. Cochrane, however, points out that this basic structure is in some ways sufficient for what he has in mind: "...for many purposes one can stop short

[27] See for example: Varian (2009), Baily (2005), Copland, Weston, Shastri (2005), Danthine and Donaldson (2005), Cuthbertson and Nitzsche (2004), Lengwiler (2004), Mas-Collel, Whinston and Green (1995), Krebs (1990).

of specifying (possibly wrongly) all this extra structure, and obtain very useful predictions about asset prices from (1.2), even though consumption is an endogenous variable." (Cochrane 2005, p. 6).[28] *Second*, Cochrane refers to the "extra structure". The structure holds assumptions that can be added to the basic pricing formula. If this is done, the structure gains in explanatory stature but loses in robustness if the assumptions are violated.

A review of these extra assumptions that harness the structure of the *central asset pricing formula* as shown in *section 4.2*, reveals that they can be clustered in the three different categories, i.e. investors, financial markets, and financial assets. Some of the assumptions are listed by Cochrane in the above extract. Nonetheless, as we shall see, other assumptions are also drawn upon. They have in common that they are simplifications of what happens in real situations. Let us start with the investor.

5.1 Simplifications with respect to the investor

Earlier I said that the investor can be just like you and I. But, we know that there are many financial market participants. They also have different reasons for their inter-temporal consumptions choices, and they hold as Markowitz alluded to but Sharpe rejected in *section 1.3* above, decidedly different investment portfolios. Aggregating preferences of all investors in an economy would make the CCAPM overly complicated and difficult to handle. Therefore, financial economists often make simplifying assumptions regarding the investors.

One of the most popular simplifications is that of the *representative investor*: "Complete markets/representative agent assumptions are used if one wants to use aggregate consumption data (...) or other specializations and simplifications of the model." (Cochrane 2005, p. 35). [29] The idea here is that instead of solving the portfolio problem

[28] The (1.2) equation that Cochrane refers to is the *central asset pricing formula* shown in section 4.2 earlier.

[29] Yet, one of the most elegant features of Cochrane's framework is that many of the derivations in the model hold for any investor and asset class: "These equations [*from the asset pricing model*] apply to each individual investor, for each asset to which he has access, independently of preference or absence of other

for all investors and then aggregating, it is assumed that there is one investor who is representative of the whole spectrum of investors. In this case, it is sufficient to solve his/her portfolio problem. The solution with respect to asset prices, or expected returns, follows naturally and justifies Cochrane's interest in analysing "aggregate" macroeconomic data. Another notable simplifying aspect of the representative investor is that he/she is self-interested. This follows from the fact that he/she operates on his/her own, for his/her own account and in the absence of involvement in any social activities beyond the decision on current and future consumption, and which portfolio of assets to hold.

Furthermore, financial economists often want to make additional assumptions about how additional income beyond the endowment is obtained. For example, most people work and receive an income. Yet, the level of human capital and its income generating capacity may be hard to measure. Therefore, financial economists often for simplicity reasons assume that income from financial market investments is sufficient to measure overall consumption growth.

Cochran's consumption based capital asset pricing research effort, therefore, has at its core the rational, self-interested and risk averse representative investors who maximize his/her total expected utility across time and different economic states – given his/her endowment. This is a powerful setting that will stay with us throughout this thesis – in particular *Chapter Three, sections 1.1* and *1.3.*

5.2 Simplifications with respect to financial markets

In reality, there is not one single financial market. In fact, there are several. In some stocks are traded, and in others, fixed income securities. Some exist in the US and others in Asia. And while some markets are open others are closed due to different rime-zones. How does Cochrane make them look homogenous? The answer lies in what economists call "completeness" of markets.

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assets or investors." (Cochrane 2005, p.35). In other words, the assumption that the representative investor exists is not necessary for many of his results.

Earlier Cochrane told us that the theory assumes a payoff being dependent on which state of nature materializes in the future. Caterpillar's stock, for example, might be expected to fetch a higher price and dividend in good economic times than in bad. These states of nature, we recall, can be exhaustively described. No matter what the future holds, therefore, there is an exact Caterpillar pay-out attached to every possible state of affairs. Since the investor does not know which state of nature prevails in the future, his/her preferences for holding a particular stock are not constant as he/she reviews the infinite number of possible outcomes. Caterpillar will, therefore, be priced according to the individual investor's risk appetite, i.e. below the fair bet price in the case of risk aversion.

Defined this way, the price for every possible pay-out can also be viewed as a distinct market for a particular stock. Cochrane assumes that markets are complete, which means that if there are ***n*** possible states and ***n*** linearly independent assets, any future consumption plan can be obtained as a portfolio payoff. All other asset prices can then be expressed as a function of these ***n*** basic securities. It is thus assumed that there exists a market in which a price is found for every possible future pay-out on all stocks:

> "Financial markets are said to be *complete* if, for each state of nature Ø, there exists a market for contingent claim or Arrow-Debreu security – in other words, for a claim promising delivery of one unit of consumption good (...) if state Ø is realized, and nothing otherwise." (Danthine and Donaldson 2005, p. 196).

Theoretically, therefore, there are an infinite number of Caterpillar stocks - one for moderately good economic times, another for better times, a third for the best of times, etc. In this way, stocks and their pay-outs are not only something for today or tomorrow, but also something for every possible future scenario.

Market completeness ensures the existence of a unique competitive equilibrium in the financial market. If markets are incomplete, the equilibrium consumption allocations and prices are not unique because there are infinitely many portfolio allocations that generate the equilibrium consumption allocation. I have more to say about this financial market structure in *Chapter Three, section 1.2.1*.

5.3 Simplifications with respect to financial assets

There are also many types of financial market assets. Previously, I have pointed towards stocks, bonds, options, etc. They come in all forms and shapes with different maturities and payoff patterns. Stocks pay dividend, bonds pay coupons, and option payoffs are contingent upon the realization of particular events.

Financial economists simplify this multitude by referring to asset classes such as stocks and bonds, or they just speak about a representative security. Cochrane is not an exception. He uses this theoretical security with attractive properties. It can be bought and sold everywhere, anytime and at no transaction cost. It was first introduced in Arrow (1976). It is, therefore, called the Arrow-Security, or a “pure security” or a “state-contingent” claim. It is defined as delivering a unit of purchasing power (consumption) conditional on a specific event, which is the occurrence of a particular state. If that event materializes, a predefined sum is paid out to the holder of such assets. If the event does not occur, its payoff is zero. An event can be, for example, a “good” economic state or a “bad” economic state. Should the former materialize, a pay-out of, say, 2 units is made. In the latter case, 0.5 units, for example, is earned. Both events, however, cannot happen at the same time. They are mutually exclusive.

The Arrow-Security has several beneficial properties. If markets are complete and the *law of one price* holds, then the payoff pricing functional assigns a unique price to each state claim. In Cochrane’s language, a unique asset specific discount factor exists that equals the contingent claim price divided by the economic state’s probability. In *Chapter Three, section 1.2.2*, I continue this analysis. [30]

[30] For a more detailed analysis of market structure and financial assets see Danthine and Donaldson (2005), Lengwiler (2004), LeRoy and Werner (2001).

5.4 Equilibrium

In the previous section, we discussed how investors maximize their expected utility. Here, I am interested in one prominent consequence of this behaviour, i.e. an equilibrium situation. When all investors maximize their total expected utility, the loss in marginal utility incurred by foregoing current consumption, and instead buying assets, must be equal to the expected gain in marginal utility, contingent upon the anticipated increase in consumption given the uncertain return provided by the asset in the future. This trade-off will be settled when each investor cannot improve on his/her situation anymore, given the behaviour of the others. If this equilibrium situation is violated, i.e. the first order condition (see *section 4.6*), there exists at least one investor who would rather forgo consumption now and invest more for a higher level of future expected consumption. Furthermore, expected future utilities must be discounted back to their current values. I addressed this topic in the model discussion in *section 3.2* above, on the subjective discount factor β.

Thus, in this situation, on aggregate, investors have reached agreement on their optimal holdings of all possible financial markets assets, at particular prices at a point where no-one can benefit from additional transactions. Wellbeing is thus maximized at a societal level, and the financial economy will have reached an equilibrium situation. Many conditions support the existence of such an equilibrium situation and much can be said about the outcomes thereof, i.e. *first and second welfare theorems*. This is, however, not the place to explore any of them in detail. [31]

So far, I have reviewed Cochrane's *story* concerning investor's demand for risky assets, the first order condition for the optimal consumption and portfolio choice – the two central choices the investor has to make in the consumption based asset pricing framework, and how a simplified view of the investors, markets and assets leads to an equilibrium situation in the financial markets. This is theory. Cochrane's next step is to check whether it corresponds with real situations. The next section reviews Cochrane's suggestions.

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[31] For further insights see Arrow and Debreu (1954), Hirshleifer (1964, 1965, 1966), and in particular Radner (1972).

6. *Assessing the CCAPM*

Earlier, in *section 3 and 4*, I established that Cochrane is providing more than a good *story* when it comes to explaining stock price behaviour. Economic models, but also theories, should investigate:

> "...whether they [*the stories*] are internally consistent, see if they can quantitatively explain stock returns, and check that they do not make widely counterfactual predictions in other dimensions, for example, requiring wild variations in risk-free rates or strong persistent movements in consumption growth. Few stories survive such scrutiny." (Cochrane 1997, p. 12).

Here, Cochrane makes two main points; asset pricing research is empirical in nature and, as a consequence, the model-based claims shall be compared with respect to their accuracy relative to the real situation data.

Fellow academics agree:

> "Financial economics is a highly empirical discipline, perhaps the most empirical among the branches of economics and even among the social sciences in general. This should come as no surprise, for financial markets are not mere figment of theoretical abstraction, they thrive in practice and play a crucial role in the stability and growth of the global economy. (...) The close connection between theory and empirical analysis is unparalleled in the social sciences, although it has been the hallmark of the natural sciences for some time." (Campbell, Lo and MacKinlay 1997, p.3).

In the previous sections of this chapter, I have reviewed the first three main elements of the asset pricing research effort, i.e. data, theories and models, In this final section, I turn towards the fourth, i.e. model assessment, and show how Cochrane's CCAPM, empirically, has not been able to deliver on its promises. In support of my general statement, I chose a topic that has occupied a generation of financial economists. It is referred to as "equity risk premium puzzle" (Mehra and Prescott 1985). The equity risk premium, we recall, refers to the historical fact that investments in US equities have returned more than investments in short-dated US Treasury Bills. The size of the positive return difference is a puzzle because consumption based asset pricing models have, so far, been unable to explain it. I will use this topic as a case-study for my philosophically related discussions on pricing model application and assessment in *Chapter Five*.

6.1 The research question

In their research paper, Mehra and Prescott notes:

> "Historically, the average return on stocks has far exceeded the average return on short-term default free debt. Over the ninety-year period 1889-1978, the average real annual yield on Standard and Poor 500 Index was seven percent while the average yield on short term debt was less than one percent. The question addressed in this paper is whether this large differential in the average yields can be accounted for by models that abstract from transactions costs, liquidity constraints and other frictions in the Arrow-Debreu set-up." (Mehra and Prescott 1985, p. 145).

Here, the authors tell us that in the United States, over a long period of time through differing economic and political states, the average annual real yield, i.e. inflation adjusted return on "risky" stocks was much higher than that of short term, "risk-less" assets. The proxy for the former is the S&P stock index of stocks comprised of the 500 largest US domiciled companies, and for the latter, primarily, US Treasury Bills with a maturity of three months. The average annual return difference between these two different asset classes is more than six percentage points. It means that an investor who put his/her savings into the 500 stocks would have earned six percent more per year than other investors who preferred to hold and roll over their short term investment in the safe asset class. We are told that the difference is the equity risk premium, i.e. the premium or compensation that investors earn because equities are riskier than US Treasury Bills. [32]

Financial economists, as we know by now, frequently measure risk as the standard deviation of realized stock returns. When applying this statistical calculation to the long return time series used by Mehra and Prescott, we find that the result indicates a number close to seventeen percent for equities and near six percent for the riskless assets.

[32] See footnote 17 earlier in this chapter on US Treasury Bills.

Subtracting and adding the standard deviation number from, respectively, to the realized stock investment return of seven percent gives the average annual payoff corridor. For equities, the annual return range, i.e. one standard-deviation was between minus ten percent and plus twenty four per cent. This is a large number. It means that the investor does not know whether he/she receives a positive or a negative return next year from holding stocks in his/her portfolio. The US T-Bill investor experienced a tighter confidence interval of returns. His/her was between minus five and plus seven per cent return per annum. This difference in realized return volatilities also tells us that the stocks were three times “riskier” than US T-Bills. Hence, some financial economists often tell us that the stock investors were compensated by a higher realized return for bearing a higher realized risk.

Financial economists consider the equity risk premium to be both of theoretical and practical relevance. The theoreticians are focused on understanding it while practitioners are afraid of missing out on it, i.e. reaping an excess return if the premium continues to exist in the future. The question that Mehra and Prescott ask themselves is how the equity risk premium connects with their type of consumption based model and its underlying theories. Below, in the next section, I continue this discussion.

6.2 The complication

After defining the research question directed at understanding the size of the equity premium, Mehra and Prescott decided to pursue its answer within a theoretical framework of the consumption based capital asset pricing model that I discussed earlier in this chapter. We know from previous discussions that the CCAPM connects financial asset’s payoff with consumption. In their version of the model economy, Mehra and Prescott suggest that the stock price expresses a claim on a stochastic production opportunity. This also defines future consumption because what is produced is also consumed. The stock payoff is thus defined to co-vary with the marginal utility of consumption. The question then raised is whether the magnitude of this covariance is large enough to justify the observed risk premium. Following some mathematical dexterity, Mehra tells us:

> "...the equity premium is then the product of the coefficient of the risk aversion, γ, and the variance of the growth rate of consumption. As we shall see later this variance (...), is 0.00125, so unless γ is large, a high equity premium is impossible. The growth rate of consumption just does not vary enough." (Mehra 2003, p. 58). [33]

We are told that the growth rate in consumption is fairly stable over time. In fact, this stability should only give a small equity premium – unless the risk aversion is unusually high. Alternatively, it is possible that better measures of consumption should be used that are more volatile and correlated with equity returns, a topic we return to at the end of this chapter in *section 6.5*. For now, I focus on investors' preferences and how they are captured in the utility function.

To examine the investor's risk aversion γ, Mehra and Prescott assume the same power utility function that is used by Cochrane. They first calculate the mean and standard deviation of per capita consumption growth in the US economy. It averages close to two percent per annum and has a standard deviation of around four percent. Compared with the standard deviation of the stock return of 17 percent, we intuitively discover a disconnect between these volatilities. From there, and with the help of several mathematical assumptions covering the growth rates in consumption, production and returns, as well as their statistical properties and correlations, the two authors solve for the parameter values of the risk aversion and the subjective discount factor for which: "...the model's average risk-free rate and equity premium match those for the U.S. economy over this ninety-year period." (Mehra and Prescott 1985, p. 154). The risk aversion parameter γ impacts the expected return on the stock market whereas the subjective discount factor β impacts the risk-free rate since it measures the impatience of investors with respect to consumption now or later. This distinction is crucial because it is possible that the equity premium is too high because either stock market returns are too high or the risk-free rate is too low relative to the predictions of the CCAPM.

[33] A similar argument and conclusion can be found in Shiller (1981, 1982, 1989).

When specifying the model, Mehra and Prescott made the assumption that the risk aversion parameter γ should not exceed the value "10". They say that this number is already on the high side and refer to several academic studies based on investment choice experiments, i.e. behavioural economics. These experiments mostly seek parameter values between zero and two. They also restrict the subjective discount factor to be between zero and one. This range is reasonable, but a number closer to one is even more so because it tells us that the utility from tomorrow's consumption is not significantly different from that gained by consumption today.

When put through this calibration of risk aversion of "10", however, the CCAPM fails to account for the size of the return difference between risky and riskless assets. In fact, the model says that the risk premium is unusually low:

> "We find that for such economies, the average real annual yield on stocks is a maximum of four-tenths of a percent higher than that on short-term debt, in shape contrast to the six percent premium observed." (Mehra and Prescott 1985, p. 146).

The stock market has, in other words, performed too well relative to the risk free asset – or the return on the risk-free asset has been too low.

This result stunned the proponents of the consumption based asset pricing model. How can it be that the model predicts a very similar return on risk-free and risky assets, i.e. the difference being only 0.4 percentage-points per annum instead of the 6 percentage-points difference observed? Mehra and Prescott dubbed this anomaly the "equity premium puzzle". It refers to the inability of consumption based asset pricing models adequately to explain the magnitude of the equity risk premium.

In its defence, the consumption based asset pricing model used by Mehra and Prescott indicates a small positive risk premium. This is intuitively right because the representative investors equipped with von Neumann and Morgenstern capabilities would demand a premium for holding equities with a historical standard deviation of returns approximately three times larger than that of a risk free asset. The puzzle, therefore, seems to be one of magnitude – albeit large. How can a more reasonable result be found? There are two paths for answering this question. *First*, Mehra and Prescott can alter the dispersion of the consumption growth. This solution, however, is not pursued because they want to anchor the parameterization in the realized numbers from the historical sample. *Second*, the authors can adjust the parameters attached to the risk aversion and the subjective discount factor.

Which numbers, in other words, do Mehra and Prescott need to calibrate and "plug-in" to make the model output compatible with the observed growth process in the consumption data? "If we set the risk aversion coefficient γ to be 10 and β to be 0.99, what are the expected rates of return and the risk premium using the parameterization just described?" (Mehra 2003, p. 59). From this, a risk-free rate of return of 12.5% per annum and a stock return of 14.1 % p.a. is calculated. This implies an equity risk premium of 1.6 percentage points. The result is still decidedly different from what the historical experience has been. It turns out that the risk aversion and the subjective discount factor require unrealistic high numerical levels to match the equity premium. In fact, Black (1972) finds that γ must be as high as 55 and the β as low as 0.55 to "back-out" the historical realized premium from the consumption data.

The risk aversion number is as most financial economists point out, too high. It implies, for example, that an investor would spend all his/her available income, or endowment, now, as quickly as possible, rather than invest to collect an uncertain future payoff that can be used for a higher level of consumption later. This, in particular, goes against the empirical fact that people indeed are observed to save, and the assumption that they target a smooth consumption pattern over economic states and time. Financial economists are thus challenged because their model readings do not correspond with the data from the real situations. How they respond, is the topic of the next *section 6.3*.

6.3 Three avenues of rectification

Psychologically, reactions to negative and unexpected surprises and events come in waves, i.e. disbelief, denial, resignation, etc. In the case of model builders, reactions are not markedly different. Nonetheless, financial economists are of the resilient type. Up until now, "JStore", the electronic storage platform for academic research papers, reports several thousand published references with "equity risk premium" in their titles. [34] This search result indicates interest, but also the fact that a generally accepted "explanation" of the phenomena has yet to be delivered. A brief review of the most recent papers reveals a trend in financial economists' approach towards solving the equity premium puzzle. In fact, they seek answers to the puzzle in three areas: the theory and its collection of assumptions, the mathematical structure of the model, and, finally, the data. Let us start with the data.

6.3.1 Data

Earlier we saw that asset pricing data come as raw prices or derivatives thereof such as returns, variances or risk premia. We established that their existence and availability were unproblematic. In the search for puzzle-resolution, however, financial economists took a renewed interest in the data. Could it be, they asked, that "biases" in the data were responsible for the high risk premium? What do they have in mind?

Students of the times-series sample used by Mehra and Prescott make the point that the time series is too long. They ask whether the risk premium is visible also over shorter or moving data time-windows. The answer is "yes". Post World-War Two, in 20 year increments up to 2004, the risk premium in the US has always been positive (Siegel 2005). However, the numbers differ from a low of 1.46% to a high of 11.21%. For others, the time period under review is too short. Siegel (1992), for example, reconstructs financial market data to go as far back as to 1802. Again, the risk premium was inherent in that data sample. Siegel points out, however, that real rates on short-term fixed income instruments have fallen over the time period under review. This might have put an upward bias on the return difference between riskless and risky assets. Other students asked if the premium observed in the US also is visible in other countries. The answer is,

[34] www.jstore.org

once again, "yes". A comprehensive study in 16 different countries for the period between 1900 and 2000 established that the premium is apparent (Dimson, Marsh and Staunton 2002). In historical and cross-market comparisons, therefore, no particular biases could be detected and the equity risk premium upheld its puzzle-status.

Besides the length of the time-series, the data focus has also been directed towards the particular economic and political states or circumstances during the years of data collection. Do the data reflect socio-economic biases? A lot has happened in the US economy since the measurement started – to say the least. The country, for example, went through several wars, the Great Depression and other more recent significant economic and political events. While only the few "fittest" of companies survived through these times, the rest went bankrupt and other emerged. This so-called "survivorship" bias might, therefore, have underestimated the riskiness of stock markets and overstated the return on stock investments (Brown, Goetzmann and Ross 1995). But studies of stock returns in other countries, including Switzerland and Sweden, which experienced less economic and political disruptions, confirm the existence of the premium.

In sum, the equity risk premium has often been dismissed on the notion of data-biases. Empirical studies refute this.

6.3.2 Theoretical understanding

Since data-biases so far have been excluded as a source for artificially introducing a high equity risk premium, proponents of the consumption based research effort turned towards the theoretical foundation upon which their models are built. The foundation has two pillars; the *first* pillar comprises assumptions that support the development of the representative investor, i.e. his/her rationality, self-interest and risk aversion. These elements are captured mathematically in the two arguments of the stochastic discount factor (SDF), i.e. the subjective discount factor and the utility function. The *second* pillar comprises particular conditions that define the circumstances under which the SDF is applied, i.e. the financial market conditions and the financial assets. Let us review how financial economists approach the second pillar in order to explain the puzzle.

Mehra and Prescott (2003) draw our attention towards several possible theoretical "adjustments". They mention: "...modified probability distributions to admit rare but disastrous events, survival bias, incomplete markets, market imperfections. (...) limited participation of investors in the stock market,.." (Mehra and Prescott 2003, p. 31). From the suggestions made by Mehra and Prescott in this bundle, much attention has gone towards the completeness of markets. As assumed by the standard CCAPM, a complete market has an asset for every possible trade that the investor can think of for every possible realization of a future economic state.

The existence of an infinite number of assets is a certainly a strong theoretical assumption. Constantinides and Duffie (1996), therefore, in their own variation of the CCAPM, suggest incorporating markets that are "in-complete". In a recessionary economic environment, for example, the "recession-proof" assets that are expected to generate positive returns during an economic slowdown might not be available, i.e. they do not exist. Investors, therefore, facing dire economic states and possible unemployment, have no means to effectively "hedge" against the uncertainties. In a model with market incompleteness, investors might require a large equity premium, i.e. the expectation of being paid a high return in order to be incentivized to buy and hold assets with uncertain payoffs. When this proposition is compared with respect to its accuracy relative to the real situation data, however, the equity premium does not yield and continues to persist.

In sum, several assumptions have been reviewed and developed to adjust the market structures incorporated in the original CCAPM framework to account for the observed equity premium. These efforts, however, have not extinguished the puzzle. Another path has, therefore, been taken; review the more fundamental assumptions supporting the representative investor. This led to changes in the mathematical structure of the stochastic discount factor. Cochrane alludes to the incorporation of non-standard preferences as "reverse-engineering".

6.3.3 Incorporating Non-Standard Preferences

Richard H. Thaler points out:

> "Many explanations for the equity risk premium puzzle have been offered, and all the theoretical explanations so far proposed are behavioural – in the sense that they build on the Mehra-Prescott model and make some inferences about investors' preferences. In most of these models, the investor makes rational choices but their preferences are slightly different from ones traditionally considered normal." (Thaler 2002, p. 3).

I am interested in exploring what these non-traditional preferences are, and whether the suggestions offered have helped solve the puzzle. The answer lays the foundation for my discussion in *Chapter Three, section 2*.

Recall that the stochastic discount factor used by Lucas, Mehra and Prescott, as well as Cochrane, builds on a power utility function. It is the traditional "working-horse" of micro-economic theory. Economists say that this function is "well-behaved" (Varian 2006, p. 44). Furthermore, as we discussed in the previous section, the assumption of power utility is not only convenient because it leads to tractable asset pricing models. It also explains the stability of financial variables in the face of secular economic growth because it implies that the absolute risk aversion decreases with wealth while relative risk aversion is constant.

Nonetheless, the application of the power utility form has been criticized because it connects the coefficient of the relative risk aversion and the elasticity of the inter-temporal consumption substitution. In fact, the CCAPM considers these decisions to be made simultaneously as one is the inverse of the other: "The implication is that if an individual is averse to variation of consumption in different states at a particular point in time, then she or he will be averse to consumption over time." (Mehra 2003, p. 60).

Hall (1988) claims that the investors' attitude towards the risk- and the time-dimension should be pulled apart and analysed independently of each other; the former is related to the investor's preference for consumption from across unknown states of the world at a particular time in the future while the latter is the timing issue, i.e. now or later. Epstein and Zin (1989, 1991), for example, introduce a utility function that parameterize these two preferences independently. The advantage is that a high risk aversion does not necessarily mean that consumption needs to be smooth over time. Nonetheless, Weil (1989) demonstrates that despite this innovation, the equity risk premium puzzle remains what it is said to be, a puzzle.

Given the disappointing performance of consumption based models, the most radical of the suggested avenues of improving our understanding of asset prices is to dispose of this set-up completely, i.e. wave the project that seeks to link asset prices to macroeconomics, and retreat to the factor-based, statistical explanations. A revival of the statistical times series analysis, however, is, in Cochrane's view, a step in the wrong direction. Nevertheless, he can see some burden-sharing: "With this insight, we can achieve a satisfying division of labour, rather than a fruitless alpha-fishing contest." (Cochrane 2006, p. 6). Cochrane here alludes to factor models doing the relative pricing of assets based in Sharpe's market- and Fama and French's style- portfolios while macro-models explains why these factors are relevant as discussed above in *section 1.3*.

Cochrane, however, has another idea that keeps the consumption based framework alive. He adjusts the assumptions that define the specific conditions at work inside the central asset pricing formula. This, once again, raises the question whether the manipulations of the model are founded in innovation in theories and their derivations, or if whether the manipulations merely contribute to the extensions of known mathematical techniques. It seems that the latter is the case. Cochrane's belief in the ability to adjust the model to new empirical results supports my view. Neither theory nor data can then be viewed as the "primitive". More relevant is the mathematical adjustment of existing arguments at a deep level inside the pricing formula.

Let us now explore how Cochrane uses reverse-engineering techniques to manoeuvre his/her research effort towards better empirical results than those already achieved. It takes us into a topic labelled *habit-persistence*. I again discuss this important notion of habit-persistence in the context of the asset pricing application and assessment in *Chapter Five, section 2.2.*

6.3.4 Habit-persistency

Cochrane's starting point is his version of the standard CCAPM. We recall it contains the idea that investors prefer smooth consumption across different economic states and through time. This preference is modelled by the power utility function so that asset prices, in equilibrium, reflect the discounted first order condition times the expected future payoff from the investment portfolio. Since this model does not "explain well" and the data themselves are not open for renegotiation, Cochrane's focus goes elsewhere, i.e. the utility function. We recall that the investor's wellbeing comes from his discounted total expected utility. Cochrane and his financial economist colleague Campbell ask whether the current and expected marginal utility is influenced also by other variables. The answers to this question are given in the so-called Campbell-Cochrane habit-persistence version of the standard CCAPM (Campbell and Cochrane 1999). The extension of the standard CCAPM to a new habit-persistent model is a main topic that will gain importance as I proceed into the next three chapters (see, in particular, *Chapter Five, section 2.2*).

Campbell and Cochrane's model is based on related work of modelling non-standard preferences by Abel (1990) and Gali (1994). They suggest the representative investor is interested in how other people are doing. It might be the case, the two authors explain, that the marginal utility of the representative investor increases if he/she does better relative to his/her peers. Abel referred to this "external" benchmarking as "keeping up with the Joneses". The Joneses' wellbeing is captured in the per capita consumption data, and this time variable is thus added as a new argument into the utility function of the representative investor. His/her marginal utility would then be influenced by fluctuations in the consumption of others. If the fluctuation is large, the representative investor would avoid stocks, i.e. become risk averse. This would explain a high and time-varying risk aversion number. Nonetheless, as we have seen above in *section 6.2*, per capita consumption is fairly stable and does not induce the investors to shun stock investments. Empirically, the equity premium remains.

In the second related example of reverse-engineering as an answer to the puzzle, investors are said to compare their expected utility relative to their own realized consumption over past periods. Constantinides (1990) and later Heaton (1995), refer to this as an “internal” benchmark. For this type of situation, the investor’s own past consumption is introduced as the new argument in the utility function. The investor is, therefore, concerned with avoiding a possible negative deviation from his/her earlier historical consumption path. If he/she succeeds to uphold his/her consumption level, the current marginal utility increases. Given the return volatility of equities, there is a real danger that this might not happen because the next period’s payoff could be negative. His/her consumption would possibly fall below past levels. This would explain why the risk aversion and the equity premium are both high. Constantinides claims that: “...habit persistence can, in principle, reconcile the high mean equity premium with the low variance of consumption growth and with the low co-variance of consumption growth with equity returns.” (Constantinides 2002, p. 1580). In the end, however, Constantinides points out to us: “Habit persistence may well gain in empirical relevance in explaining assets returns, once we correctly measure the consumption of the unconstrained marginal investor in the capital markets.” (Constantinides 2002, p. 1581). This remark, as we shall see in the following section, and has not passed unnoticed.

Campbell and Cochrane, in their habitual extension of the standard CCAPM suggest that the utility function might be expanded to reflect on: “...extra goods like leisure, nonseparability over time in the form of habit-persistence, nonseparability over states of nature so that consumption if it rains affects marginal utility if it shines.” (Cochrane 2005, p. 466). Incorporating such innovative arguments into the traditional utility function thus expands the efforts made earlier by Hall (1988), Weil (1989), Epstein and Zin (1989), and Abel (1990). It calls for a re-engineering of preferences in the sense that utility might flow from a variety of different sources other than exclusively from consumption now and later. For example, the utility of having an umbrella when it rains is different from the utility it yields when the sun is out. In the standard model, this question is easily settled - either you have an umbrella or you don't. The choice situation to save and invest, however, gets more complex when incorporating event-contingent utility – or, we can say, more realistic.

In the end, Campbell and Cochrane settle for an external habit-persistence benchmark akin to that discussed in Abel (1990): "An individual's habit level depends on the history of aggregate consumption rather than on the individual's own past consumption." (Campbell and Cochrane, 1999, p. 208). A relationship between per capita, aggregated societal consumption and the investor's consumption choice is thus, theoretically, established. The two authors tell us that it can be captured by what they call the *surplus consumption ratio* or S_t (Campbell and Cochrane 1998, p. 3):

$$S_t = \frac{C_t - X_t}{C_t}$$

C_t equals consumption and Xt habit consumption – both at current time t. The ratio is thus the fraction of consumption that exceeds the habitual level. This surplus is said to influence the utility of the investor. In bad economic times, a surplus in excess of the habitual consumption level might shrink towards nil. But it can also, in good times, rise towards one. Whenever the surplus decreases and falls towards the per capita consumption trend-line, risk aversion increases. The perspective of a possible drop below this habitual level is thought of as being very bad indeed. In situations in which the economic activity typically falls, the marginal utility of consumption is high. Mathematically, however, this scenario of a negative surplus is excluded by modelling the log surplus consumption ratio in a particular way, which implies complex nonlinear dynamics for habit. In the Campbell-Cochrane model, habit adjusts slowly to consumption in order to explain long, cyclical paths in stock prices.

Campbell explains:

> "If habit Xt is held fixed as consumption Ct varies, the local coefficient of relative risk aversion is:
>
> $-Cu_{cc}/u_c = \gamma / S_t$
>
> where u_c and u_{cc} are the first and second derivative of utility with respect to consumption. Risk aversion rises as the surplus consumption ratio declines, that is, as consumption approaches the habit level." (Campbell 2001, p. 67).

Cochrane tells us that:

> "This specification means that a habit can act as a "trend-line" for consumption; as consumption declines relative to the "trend" in a recession, people will become more risk averse, stock prices will fall, expected returns will rise, and so on." (Cochrane 2006, p. 34).

Investor preferences are thus modelled with the standard power utility function, and let the investor extract utility from the difference between the expected consumption and the exogenously given trend line of habitual consumption. The investor's total utility from the decision to consume or save is thus also influenced by Abel's (1990) neighbours, i.e. the Jones' family. The outcome of the mathematical dexterity tells that when the surplus consumption ratio is low, the marginal utility of consumption is high.

Furthermore, the Campbell-Cochrane model points towards two theoretical forces that counteract. On one hand, in bad times, as current and expected future consumption expectations fall towards the habitual level, individuals save more to prevent consumption from falling further, and as a consequence, interest rates fall. On the other hand, investors might also want to upkeep past period's consumption levels. This choice is related to the time dimension of inter-temporal consumption. It implies that the investor turns to the credit market to take out a loan in order to upkeep the level of consumption. Hence, interest rates will increase.

The precautionary savings motive of investors in the former situation, therefore, contrasts with the inter-temporal substitution effect through borrowing in the latter. But this serves Campbell and Cochrane well. They make the assumption that these two tendencies entirely offset each other. The implication is that their model work with the hypothesis that the interest rate used to discount future cash-flows to present values is stable, i.e. constant at their observed historical average. This example illustrates that the model proposed by Campbell and Cochrane is based on reverse engineering of the standard CCAPM in the face of empirical challenges.

6.4 Evaluation of the suggested solutions

Cochrane explains that reverse engineering works backwards from the observed data towards the desired model specification. This involves choosing and setting the parameters of the model accordingly. Let us review a few other examples from the work of Campbell and Cochrane (1999):

> "We choose the free parameters of the model to match certain movements of the post-war data. (...) We take the mean and the standard deviation of log consumption growth,..., to match the consumption data. We take the serial correlation parameter,..., to match the serial correlation of log dividend ratios. (...) Since the ratio of unconditional mean to unconditional standard deviation of excess returns, i.e. the Sharpe ratio, is the heart of the equity premium puzzle, we search for the value of (*the model parameters*) so that the returns on the consumption claim match the ratio in the data." (Campbell and Cochrane 1999, p. 218).

The reason for this extensive cascade of quotations is twofold. *First*, it demonstrates how technical the debate has turned. None of these suggested changes is derived consequences of the fundamental assumptions of rationality and equilibrium. In other word, they are imported from outside of the original model. *Second*, they are all good examples of how Campbell and Cochrane use the data and let them define functional forms and how parameters can be calibrated for the purpose at hand, i.e. replicate the real situations. Cochrane has more to say regarding the method of "reverse engineering":

> "Rather than dream up models, test them, and reject them, financial economists since the work of Mehra and Prescott (1985) and Hansen and Jagannathan (1991) have been able to work backwards to some extent, characterizing the properties the discount factors must have in order to explain asset return data." (Cochrane 2005, p. 455).

In this sense, the financial economists know the desired result, i.e. the closure of the equity risk premium gap, and then get to work towards getting there. How can this research approach be evaluated?

First, by reviewing the empirical results. And, indeed, the empirical results modestly improve in comparison to the standard CCAPM without habit-persistency. During some time periods, the habit-persistency model does fairly well. In others, however, it fails to do away with the risk premium. On a longer term historical average basis, it gets the risk aversion parameter γ down to "6" – which is still far off the lower number needed to explain away the equity risk premium, i.e. 0.4% in the traditional CCAPM. Nonetheless, a parameter value of "6" is more plausible than "55" as Black suggested in *section 6.2* above. Out-of-sample forecasting is crucial for addressing concerns that the Campbell-Cochrane model over-fits the data because it is calibrated to match historical stylized facts with respect to equity returns.

Second, since the models' ability to forecast has modestly improved, it might be that Cochrane has added in the right "causes". Nonetheless, new functional forms, innovative arguments in the utility function or changes to the numerical values of parameters are not *causes*. They all, numerically, express what causes can take. By letting them take the desired forms, Cochrane, admittedly, seeks to tease out the "drivers" of the results. However, I am looking for the scientific basis of this approach. Cochrane seems to agree: "In general, empirical success varies inversely with theoretical purity." (Cochrane 1999, p. 40).

Third, it might well be that people's consumption is influenced by both "internal" and "external" benchmarks. Purchasing patterns are hard to break as it involves a change in "life-style" and, possible, standard of living. This also questions whether the habit-persistency enhanced CCAPM has identified the right cause.

Habit formation, both from an "internal and "external" perspective, therefore, seems to have been a reasonable effort to reverse engineer the standard CCAPM. The choice to let an investor derive utility from the difference between his/her current consumption and a time-varying and externally given habit level, and capture this within the utility function, enable the high and time varying risk aversion go hand in hand with the high and volatile excess return on equities. In *Chapter Three* and *Five*, this topic gains importance when I review it in a philosophical context.

6.5 Recent work on consumption based models

In a recent paper, Cochrane shows that during the financial crisis in 2008, consumption and stock prices did fall together. Based on this, he concludes: "…the basic logic of consumption based models that assets must pay higher returns if their values fall more when consumption falls, is not drastically wrong." (Cochrane 2011, p. 1072). In addition, he shows that the price-dividend ratio is a nearly log-linear function of the surplus consumption ratio as predicted by the habit formation model. It is essential to note that Cochrane does not argue that the decline in consumption caused the stock market crash. Instead, he argues that habit-persistency acts as an amplification mechanism for the effect of consumption volatility on stock prices. This evidence leads Cochrane to conclude that he still thinks the macro-finance approach is promising: "…research and further elaboration of these kinds of models, as well as using their basic intuition as an important guide to events, is not a hopeless endeavor." (Cochrane 2011, p. 1075).

The most notable extension of the basic CCAPM in the last decade is the long-run risks model proposed by Bansal and Yaron (2004). Building on the Epstein-Zin-Weil recursive utility function, they add a term to the CCAPM that reflects the covariance of an asset return with long-run consumption growth expectations. They argue that investors are averse to shocks to long-run consumption even if those are uncorrelated with shocks to current consumption. Bansal and Yaron show that when the coefficient of relative risk aversion is larger than the inverse of the elasticity of intertemporal substitution, a small predictable component in consumption growth can explain the equity premium puzzle with modest risk aversion. Furthermore, they claim that the persistent, long-run fluctuations in the mean and volatility of aggregate consumption growth also explain the high variation in stock prices relative to consumption growth volatility.

Essential in all consumption based asset pricing models is that variation in asset prices is due to shocks to the process driving aggregate consumption. These models differ in the exact type of consumption shock as well as with respect to how such a shock works through (which is determined by the specification of the utility function). The habit formation model of Campbell and Cochrane (1999) stresses the importance of shocks to the current level of consumption relative to a moving average of its past values. In the long-run risks model of Bansal and Yaron (2004), the main shocks that lead to fluctuations in aggregate stock prices are changing expectations of long-run consumption growth and its volatility. In the rare-disaster model of Barro (2006), Gabaix (2011) and Wachter (2012), the main drivers are changes in the probability or severity of a large fall in consumption. These changes produce time-varying discount rates and, therefore, generate predictability of the equity premium and excess volatility of the stock market.

Beeler and Campbell (2012) point out two difficulties with the long-run risks model. *First*, empirical evidence suggests that in contrast to the calibrations of Bansal and Yaron, in which the elasticity of intertemporal substitution (EIS) is greater than one, the EIS is smaller than one if we look at aggregate consumption data, and even close to one in micro data for stock market participants. *Second*, there is little evidence that the consumption-wealth ratio predicts long-run consumption growth in the way implied by the long-run risk model. *Third*, the data do not show as much persistence in consumption growth as implied by the model, which casts doubt on the existence of the predictable variations in long-run consumption growth that drive the long-run risks model.

Bansal, Kiku and Yaron (2012) respond to this critique by noting that there can be a downward bias in the estimates of the EIS when variables exhibit stochastic volatility, as is the case in the long-run risks model. Furthermore, they argue that when the model is tested on subsamples of the data as done by Beeler and Campbell (2012), it should be recalibrated because the macroeconomic dynamics in the sub-sample can be decidedly different from those in the full sample. They also note that the habit formation model of

Campbell and Cochrane (1999) implies that consumption growth predicts the future price-dividend ratio, which is inconsistent with the data.

This heated debate shows that interest in consumption based asset pricing has reinvigorated in recent years by the development of a new generation of consumption based theories. The next step in the assessment of these theories requires moving beyond calibration to formal econometric estimation and model comparison. Apart from the aforementioned extensions of the CCAPM, recent work has also focused on the quality of the consumption data used to empirically test these models. In particular, Savov (2011) shows that a new derived measure of consumption, garbage is more volatile and more correlated with stock returns than the standard consumption measures used in the literature that are too smooth. A garbage-based CCAPM can explain the equity premium with relatively modest risk aversion in the U.S. and in Europe. A detailed overview of the recent developments in consumption based asset pricing and the empirical methods used to test these new models can be found in Ludvigson (2012).

Chapter 3:
CCAPM Theory, Assumptions and Research Programme

Introduction

In the previous chapter, I gave a focused description of main elements of John H. Cochrane's research effort related to the so-called consumption based capital asset pricing model. The primer thus covered three main topics: asset pricing theories, asset pricing models, and empirical tests of model claims. In the following, I critically assess these three topics. I am motivated by Cochrane's intuitive *story* related to investors' demand for risky financial assets and how this *story* is made explicit in theories and mathematical equations. However, only modest theoretical and empirical progress give us justified reasons to doubt the research effort's abilities to provide useful knowledge for use to the various stakeholders, i.e. investors, public policy makers and fellow academics (see *Chapter Two, section 1*).

I believe the explanations for CCAPMs lack of success can be found, primarily, in the well-rehearsed representative agent-based equilibrium theories, the elaborate mathematical structures, and the assessment of the model-based claims. In this chapter, I address the first of these three potential sources of discomfort, i.e. the consumption based capital asset pricing theory. I postpone the two latter topics, i.e. asset pricing model and its assessment to *Chapter Four* and *Five* respectively

My starting point is Cochrane's suggestion: "The theory of asset pricing contains lots of assumptions..." (Cochrane 2005, p. 35). This claim is reasonable. In *Chapter Two*, *section 2.2*, I initially pointed towards Cochrane's *story* concerning investors and their demand for recession proof stocks. I also showed how this *story* can be seen as a casual interpretation of CCAPMs theoretical foundation. I reviewed this theoretical foundation, primarily, in *section 3* and *5*, where I described the agent's utility, risk-preference, and optimizing behaviour which leads to a financial market equilibrium situation. It is crucial not to underestimate the importance of this narrative and its relationship with the CCAPM theoretical foundation, and, as we shall discover in the next chapter, its modelling effort. In fact, I will show how the *story* has a dual role; it interprets the theory and it represents the real situations (see *Chapter Four, section 4.4.1*). The story thus makes use of simplifications, isolations, idealizations, and fictional constructions. These elements not only simplify a complex reality. They also deliberately distort them. Distortions, I will argue, help explain why the CCAPM, so far, has been statistically unsuccessful.

The *purpose* of this chapter is to analyse the theoretical foundation of the consumption based asset pricing research effort. The *goal* is to identify possible problem areas that can be held responsible for its only modest ability to generate knowledge for use. I *conclude* that the consumption based capital asset pricing effort is a distinct "research programme" that uses stable fundamental, "hard core", principles, and a flexible set of auxiliary, "protective belt", assumptions, methodological decision rules in the form of "positive and negative heuristic", and an established form of assessing whether the research programme is "progressive" or "degenerating". Nonetheless, regardless of my findings on the programme's methodological insistence, its success is overshadowed by its content. The three main elements of the theory, i.e. the representative investors, his/her character and behaviour, the financial market structure, and the financial market assets take on idealized and fictional forms in isolation from a much richer empirical world. It points towards the challenges of reconciling the theoretical cases with the real situations. I refer to this as the "fallacy of simplification" (see *Chapter Three, section 1.4*).

I argue for my conclusions in three sections: The *first* section takes Cochrane's statement at face value, i.e. the asset pricing theory is a collection of assumptions. I put these assumptions into three compartment; *first*, the investor, his/her character and behaviour, *second*, the financial market structure, and *third*, the financial market assets. Of these three, my main focus has so far been and will continue to be on the investor because of CCAPMs important micro-economic foundation. Initially, I let Mary S. Morgan inform me with respect to the changing character of this individual through time. It emerges that

CCAPM's *central asset pricing formula* builds on an agent referred to as the "representative investor". Then, I progress my analysis in four steps. I, *first*, let James S. Buchanan and David F. Hendry remind us that a strategy that suppresses irrelevant details to achieve "simplifications" is generally accepted and makes the research effort "scientifically manageable". *Next*, drawing mainly on Uskali Maeki, I show how simplification can be achieved in a process of isolation. *Thereafter*, I turn to Mary S. Morgan who informs us that the outcome of isolation, i.e. idealization, can be taken to the extreme through the construction of fictions. *Finally*, Daniel M. Hausman tells us that simplification, isolation, idealization take place in a "separate realm". I argue that this strategy presents internally consistent and rigorous solutions on one hand, but it makes less useful representations of their real situation targets on the other because it relies on distortions. The epistemological value of idealizations and fictions is thus questioned, and I refer to this as the fallacy of simplification.

In the *second* section, I continue my discussion of the many assumptions that characterize the asset pricing theory. Now, I turn to Cochrane's assertion that some of the CCAPM assumptions are more "fundamental" than others. I explore this suggestion, and find that the assumptions can be classified as either "fundamental" or "auxiliary". With this, I identify a clear hierarchy of assumptions. In the *third* section, I ponder over how to characterize Cochrane's CCAPM framework from a philosophical of science point of view and establish a connection to Imre Lakatos' concept of a "research programme". This programme is seen as a collection of interlinked theories with a common set of "hard core" assumptions surrounded by a number of flexible, adjustable and replaceable assumptions in a "protective belt". This view gives support to my claim that CCAPM's fundamental and auxiliary assumptions can be characterized as "core" and "belt" respectively. Furthermore, Cochrane's version of the CCAPM makes use of both the "negative" and "positive" heuristic that Lakatos promotes when facing "anomalies". Finally, I identify a defined way of assessing the programmes' theoretical and empirical progress, i.e. whether it is "progressive" or degenerating". I, therefore, conclude that the consumption based capital asset pricing effort can indeed be characterized as a Lakatosian research programme.

1. The microeconomic foundation

In *Chapter Two*, *section 1.2*, I pointed out the differences between what Cochrane calls the "relative" and "absolute" approach to asset pricing. [35] While the former can be considered theory "light", the latter is often regarded as theory "heavy". Cochrane motivates his choice of a theory heavy approach first by discrediting it: "Why bother, given that "reduced form" or portfolio-based models like the CAPM are guaranteed to perform better?" (Cochrane 2008, p. 242). Following this rhetorical question, he gives a thoughtful answer:

> "The centrepiece of dynamic macroeconomics is the equation of savings to investment, the equation of marginal rates of substitution to marginal rates of transformation, and the allocation of consumption and investment across time and states of nature. Asset prices are the mechanism that does all this equating. If we can learn the marginal value of wealth from asset markets, we have a powerful measurement of the key ingredient of all modern, dynamic, intertemporal macroeconomics." (Cochrane 2008, p. 242).

From these quotes, I extract two main observations: *First*, Cochrane tells us that macroeconomics is considered to be the "endgame". To get there, the analysis has to pass through the formation of asset prices. Asset price are the outcome of investor's behaviour in a financial market context: "An investor must decide how much to save and how much to consume, and which portfolio of assets to hold." (Cochrane 2005, p. 3). At the bottom of this hierarchy, therefore, Cochrane builds a microeconomic foundation centred on the investor. From there, preference-based choices feed into asset prices and connect micro-events to the macro-level. *Second*, I note that Cochrane regards the predictive power of a theory and its model as selection criteria when choosing between various propositions. However, we also hear that this requirement can be overridden. Cochrane deliberately chooses to ignore Sharpe's (1963, 1964) factor-based, relative capital asset pricing model, i.e. the CAPM, despite its "guarantee to perform better." This is a crucial

[35] I also use the terms "factor" approach for the relative and "macro" approach for the absolute types of models. I will use these terms interchangeably with those that Cochrane here advocates.

observation that I leave for now but return to and discuss in some length in *Chapter Five* on asset pricing application and assessment.

Cochrane is thus of the opinion that a better understanding of the macroeconomic dynamics requires a microeconomic foundation. Hence, the assumptions that describe the individual investor take centre stage. The first section on the microeconomic foundation of Cochrane's asset pricing theory has four sub-sections. I *start* with a review of where in the world Cochrane found this individual, who he/she represents, and how his/her character is best described. As a first step, therefore, we need to gain a better understanding of the individual's origin. I let Morgan (1997, 2006) guide us in that effort. It takes us to brief reviews of classic references made by authorities such as Adam Smith, John Stuart Mill, W. Stanley Jevons, and Paul A. Samuelson. It becomes clear that Cochrane's view of the investor's rational, self-interested and risk averse character brings out essential aspects of human nature. Nonetheless, these simplified and idealized versions are not accurate descriptions of the "real man" he supposedly represents. I *next* show that same can be said in relation to Cochrane's description of the financial market structure and the financial assets.

Thereafter, I review how Buchanan (1958) and Hendry (1987) justify a strategy of deliberate simplifications and how Cochrane's utilizes it with respect to developing the representative investor, the financial market structure, and the financial assets. The strategy comes at a cost. In order to frame that discussion, I draw mainly on contributions from Maeki (1992, 1994), Morgan (1997, 2001, 2001a, 2004, 2006, 2008, 2008a) and Hausman (1992). I use Maeki to show that Cochrane applies the "method of isolation". This method ring-fences several entities and isolates them from a more complex reality, i.e. target situation. In their final forms, they become "idealized" versions of the real investor, market and assets. Having shown how Maeki isolates, I turn to Morgan. She tells us that economists not only isolate and idealize. They also create fictional constructs of an "artificial man in a mathematical laboratory".

Next in line is Hausman. He demonstrates how these isolated elements can be structured in a way that creates a "coherent vision" of the economic project. In particular Hausman tells us that this "structure" establishes the "separate realm" of economics. I show that Cochrane follows a similar strategy when defining the financial market structure in which the investor is active.

Finally, I turn to a critical assessment of the process of isolation that Cochrane applies when developing the investor, financial market structure and financial assets. The analysis shows that the generally accepted scientific method of isolation and the resulting simplified, idealized and fictional cases do not bring Cochrane's asset pricing theory close enough to our image of the real situations that it seeks to portray. In fact, the ideal and the real may be regarded as opposites. This is problematic because the ideal versions of investors, markets and assets do not represent their targets very well. I, therefore, argue that our expectations of what these cases can achieve should be toned down. The fact that this strategy, so far, does not explain and predict well, supports my assertion. Despite the somewhat pessimistic conclusion, I present an argument that should encourage financial economists to continue their endeavours to explain and predict asset prices. But that has to wait until I discuss Cochrane's "analytical cases" in *Chapter Four, section 3*. Here, it suffices to say that the simplified, idealized and fictional cases can be de-idealized and de-fictionalized.

Before I turn towards the origins of the investor in Cochran's theory, let me first pick up a few main themes from the previous *Chapter Two* in which Cochrane uses mathematical equations to "make the "facts", the "story" around the investor's demand for recession proof stocks "explicit".

1.1 The investor

We recall from *Chapter Two, section 2.2* that Cochrane first presents us with a *story* about investors' choices related to consumption and investment as well as their preferences for "recession proof" stocks that, hopefully, will render a stream of payoffs in bad economic times. These choices and preferences have implications for stock prices.

Given the demand for these particular stocks, investors will bid up their prices and, as a consequence, their expected returns will fall. Following this plausible sounding narrative, Cochrane makes the *stories* "explicit" in a theoretical framework. This framework "contains lots of assumptions" that are formalized with the help of mathematical equations (see *Chapter Two, sections* 4.2 and *4.3*). Finally, Cochrane confronts the model's stock price predictions with the actual prices observable in financial markets.

I showed that this starting point claimed that a current stock price $\boldsymbol{p_t}$ "equals expected discounted payoff" which is expressed in the *basic pricing equation*:

$$p_t = E_t\,(m_{t+1}\ \ x_{t+1})$$

In the equation, x_{t+1} is the expected future payoff from a stock investment while m_{t+1} is the factor used to discount this future payoff. Cochrane also call the m_{t+1} the stochastic discount factor (SDF):

$$m_{t+1}= \beta\frac{u'(c_{t+1})}{u'(c_t)}$$

In this equation, Cochrane makes reference to an investor who applies his/her subjective discount factor β to the ratio of his/her future marginal utility over his/her current marginal utility $\frac{u'(c_{t+1})}{u'(c_t)}$). The SDF is thus the numerical value of the subjective present value from the expected marginal utility originating from current and future consumption. The SDF and the *basic pricing equation* give the *central asset pricing formula*:

$$p_t\ = E_t\left[\beta\frac{u'(c_{t+1})}{u'(c_t)}\,x_{t+1}\right]$$

It's implied *first order condition* satisfies an equilibrium situation in the financial market. It claims that the investor's loss in marginal utility incurred by foregoing current consumption and buying assets at a particular price must be equal to the expected gain in marginal utility, contingent upon the anticipated increase in consumption from the uncertain return provided by the asset in the future.

Accordingly, Cochrane claims that the observable asset price fluctuations, as expressed, for example, in the *graphs 1, 2,* and *3* in *Chapter Two*, are the outcome of investor's adjustments towards the unobservable first order condition. Cochrane tells us "*the* central pricing formula" applies to "each individual investor", but most of all they are used with respect to the so-called "representative investor". This particular agent is thus a derivative of the original. He/she plays the dominant role in Cochrane's consumption based capital asset pricing theory. Let us now review both the original and the derivative.

1.1.1 From the "whole man" to the representative investor

Consider Morgan's findings:

> "Economic man was initially an explicit simplification of whole man, taken to represent real man in economic respects. Later, economic man became an artificial character, idealized to the extent required to explore the full outcomes of neoclassical economic theorizing. These simplifications and exaggerations, carried out always for reasons of good scientific method, created the modern caricature of "rational economic man"." (Morgan 1997, p. 77).

In this extract, Morgan confronts the reader with more than 300 years of economic theory development. I identify three main trends in her narrative: narrowing of man's character, narrowing of the domain in which he is active, and the importance of "good" scientific method. In the following sections, I will discuss each of these topics one at a time. "Economic man" first.

The short historical overview of man's narrowing character is relevant for two reasons. *First*, its end-point forms the basis for CCAPMs representative investor, and, *second*, it lays the foundation for my analysis of Cochrane's strategy with respect to justifying his use of this character. Morgan's first reference goes to Adam Smith's *Wealth of Nations* (1776). Therein, Smith paints a picture of a passionate man of generous sympathy, who avoids risk, loves the country-side over town-life, prefers his homeland to overseas, exploits his talents and has a natural tendency to exchange goods with others. This man is a broad-minded individual active in many fields of society. According to Morgan, he can be characterized as a "whole man".

In the course of the centuries, Morgan tells us, Smith's "whole man" developed into a more and more single-minded and specialized individual. John Stuart Mill was the first to start this transformation from a broad human character in a complex societal context towards a narrower image. In a frequently referred quote, Mill makes the following statement related to the objectives of economics:

> "It [*economic science*] does not treat the whole of man's nature as modified by the social state, nor of the whole conduct of man in society. It is concerned with him solely as a being who desires to possess wealth... (...) It makes entire abstraction of every other human passion or motive; except those which may be regarded as perpetually antagonizing principles to the desire of wealth, namely, aversion to labour, and the desire of present enjoyment of costly indulgences." (Mill, 1874, E5,V.38).

This is a rich and insightful statement. It has often been used by scholars to justify why economics should be considered a science. I will not enter into this debate here. Nonetheless, the quotation warrants three points. *First*, Mill clearly narrows Smith's broad-brush ambition to a more "manageable" scope. We see this in terms of both the economic area and the psychological outfit of the individual. *Second*, Mill identifies "desire of wealth" as the main causal driver for engaging in economic activity. *Third*, this main desire is counteracted by two other causal influences, i.e. "aversion to labour", and "enjoyment of indulgences". Morgan refers to Mill's individual as the prototype of the "homo oeconomicus".

Interestingly enough, Morgan points out, of the two "perpetually antagonizing principles" that Mill refers to, the second "vice", i.e. "enjoyment of indulgences" was later picked up and reformulated into a "virtue" by Jevons who claims "...it is surely obvious that economics does rest upon laws of human enjoyment." (Jevons 1871, Ch 3, 111.5). Mill's agent's pursuit of wealth as a motivational factor was thus exchanged with a strive for "enjoyment" or "happiness". This happiness comes through consumption and not from a higher level of wealth. This established a micro-economic view of the economic man and his behaviour and developed, in a bottom-up process, the economic consequences of the agent's actions. In the quest for individual "happiness" through consumption, Jevons suggests that the society as a whole stands to benefit. The "laws" of economics were thus thought of as operating at a micro-economic level in a specific domain, and from there the, positive, aggregated societal consequences were derived. Subsequent writers portrayed this as "selfish" pleasure seeking and even "hedonism" void of any "higher" responsibilities or duties. Morgan has a name for Jevon's individual, as well. She calls him the "calculating man".

Finally, with Samuelson (1938, 1947), the discussion shifted the focus away from possible behavioural motives, to rationality in both preference ordering and in the choice. In fact, rational choices came to dominate the theoretical development because of the intricacies related to expressing man's subjective preferences. It was thought that other academic fields than economics were better suited to explore man's preferences - for example psychology. Economists, therefore, conveniently assumed that a rational choice satisfied or even maximized these preferences – whatever they were related to. Any economic motives are, as a consequence, acceptable as long as rationality orders them and governs their choices. Morgan names Samuelson's theoretical construct the "rational economic man". He is as Morgan mentioned earlier, a less complete individual than Smith's "whole man". Nonetheless, this is mainly where micro-economic stands today with respect to rational decision making theory at the level of the individual. Cochrane, as we shall see, uses the concept in his theory of asset prices. The individual is at the same time an investor and a consumer.

There is one further development of the individual we need to high-light before I turn to Cochrane's investor. It is the introduction of the representative investor. Hartley (1996) reminds us that the representative agent concept was brought to life around the turn to the 20^{th} century through the writings of Alfred Marshall. Marshall's agent, however, was not introduced to represent a human being but an industrial firm: “Let us call to mind the “representative firm” whose economies of production...” (Marshall 1920, p.158). Marshall, Hartley claims, also toyed with the idea of introducing a similar construct to cover an individual’s role in the production process but decided against it because his focus was more on explaining wages as a dominant contributor to the costs of production.

Since Marshall, modern microeconomic literature has shifted away from wages to how consumers spend and save their income. The first few chapters of such textbooks often introduce the “Robinson Crusoe” economy with only one consumer, one type of income, two goods to choose between and at prices that cannot be influenced. From this limited foundation, the literature usually expands towards a more mature economy encompassing many consumer and many goods. At this stage of the standard analysis, the aggregated demand for consumer goods is formulated as the sum of individual demands. Different tastes and inequalities in income distribution are neglected. The representative consumer is thus regarded as an “average” of a heterogeneous population.

Financial economists also work with an individual, but he/she is quite different from the eremite on the island. Cochrane first tells us “*the* central pricing formula” can be applied to “each individual investor”. But then, as he expands his theoretical framework, Cochrane switches to a preference for the “representative agent”, or investor-based concept in relation to the analysis of aggregate macroeconomic data. This is not unusual for financial economists. It, however, requires aggregation.

Aggregation across populations of individuals may have two possible starting points; either the underlying population is heterogeneous or it is homogeneous. In a heterogeneous population, the representative agent’s behaviour is considered to be the weighted sum of the societal behaviour. This is the case as we saw, in most microeconomic models. In the homogeneous case, all individuals are the same - anyone can represent the full population. Cochrane claims no particular preference. In a competitive equilibrium with complete asset markets, it is mathematically possible to aggregate the single investors’ utility function as a weighted average of the utility functions of the various investors in the economy (see *Chapter Two, section 5.2*). In a theoretical context, therefore, it is often considered a matter of convenience whether the first or the second approach is taken. I revert to this point below.

Let me now return to the transformation of Smith's "whole man" to Samuelson's "rational economic man". In this process, moral sentiments are lost. The "rational economic man" is thus narrower than the "whole man". However, the core of his character is kept unchanged throughout this transformation. Cochrane thus uses Samuelson's description but also adds to it. This is the topic of the next section.

1.1.2 The triad of temperaments

As alluded to in the previous *section 1.1*, the investor or rather his/her derivative, the representative investor, is essential in Cochrane's CCAPM context because he provides a micro-foundation for aggregated behaviour and allows for an analysis of observable macroeconomic data and asset prices: [36]

> "The central and unfinished task of absolute asset pricing is to understand and measure the sources of aggregate or macroeconomic risk that drive asset prices. (...) For example, expected returns vary across time and across assets in ways that are linked to macroeconomic variables that also forecast macroeconomic events; a wide class of models suggest that a "recession" or "financial distress" factor lies behind asset prices". (Cochrane 2005, p. xiv).

In this quote, we receive a confirmation of CCAPMs micro-economic foundation. This anchors Cochrane in a theoretical paradigm which Morgan referred to as neoclassical. It is also often referred to as "methodological individualism". [37] But not only that. The quote also confirms Cochrane's aspirations to go beyond the simple one period pricing model such as Sharpe's (1964) CAPM, which I discussed in *Chapter Two, section 1.3*, and embed asset pricing into a dynamic, more realistic setting in which production, consumption, investments, stock prices, etc. develop over time. Cochrane thus entertains

[36] I will use the two terms, "investor" and "representative investor" interchangeably.
[37] This view is radically different from the relative or factor-based asset pricing theory as advocated by Eugene F. Fama and Kenneth R. French. The two authors, we recall from *Chapter Two, section 1.3*, do not need an investor in order to explain and forecast stock price returns.

an asset pricing model that seeks to understand how real investor's behaviour impact real asset prices.

Above, I introduced Samuelson's "rational economic man". Cochrane sees him as an "ideal" starting point for his own asset pricing theory. This choice reflects today's mainstream economic theory and practice. [38] In fact, as I pointed out in *Chapter Two, section 3*, Cochrane engraves three standard but specific features into the core of the representative investor character; rationality, self-interest and risk aversion. Going forward, I will refer to them as the "triad of temperaments". Let us take a closer look at these three characteristics one at a time.

The *first* character attribute is rationality. Rationality, we recall from *Chapter Two, section 3.1*, is defined in relation to preferences, on one hand, and choices on the other. Rationality with respect to preferences is related to knowing all available options and then rank them according to their relative attractiveness. Economists say that preferences ordered this way are "complete", "reflexive" and "transitive". Rationality related to choices builds upon such preferences and concerns itself with always choosing the highest ranked option from the available opportunity set. It is also rational we are told, to want more rather than less and prefer averages over extremes. In a nut-shell, therefore, rationality concerns itself with doing what one believes is likely to render the best subjective overall outcome.

The *second* feature of Cochrane's representative investors' character is self-interest (see *Chapter Two, section 3.1*). Self-interest directs the individual investor to choose on his/her own and, exclusively, for him/herself. He/she is self-centred. For example, when pondering over the current and future consumption decision, the agent is not concerned with the material or emotional well-being of others, such as family or friends. The agent's choice is thus directed towards maximizing his/her own well-being, or his/her total expected utility. In Cochrane's context, this maximization takes place at the margin. It

[38] See, for example, current literature on the Dynamic Stochastic General Equilibrium models (DSGE) in Dotsey (2013)

means that the marginal utility loss of consuming a little less today and buying a little more of the financial market asset should equal the marginal utility gain of consuming a little more of the asset's expected payoff in the future. In addition, as we know by now, the future expected marginal utilities need to be discounted back to a present value and compared with the utility of consuming now.

The *third* characteristic is risk aversion (see *Chapter Two, section 3.5*). By adding risk aversion to the first two sentiments, Cochrane widens the character of Samuelson's "cut to the bone" individual. Furthermore, the CCAPM based individual is also given eternal existence. During his/her infinite life, he/she has a preference for stable consumption. This leads him/her to save part of his/her current income for future expenditures. The representative investor is namely allergic towards fluctuations in his/her consumption level. This is partly mitigated by rendering him/her insights into the probability distribution with respect to future states of the economy and the expected returns on the assets that he/she can hold in his/her portfolio. As a result, he/she is given close to perfect foresight (see *Chapter Two, section 3.4*). Nonetheless, the remaining uncertainty about which economic state will materialize makes the representative investor cautious when making decisions involving uncertain outcomes. This uncertainty is related to the expected future level of consumption which will be supported by the payoff on his/her investment portfolio. He/she will, therefore, not spend all his/her endowment or wealth on consumption today, but save some of it for future spending. This inter-temporal substitution of purchasing power is facilitated by financial market assets. Hence, the investor purchases stocks and hold them in his/her investment portfolio in the expectation of future payoffs in bad economic times. Uncertainty thus requires risk aversion. While his/her desires are subjective, his/her beliefs corresponds with the available facts, i.e. they are considered to be objective

This triad of temperaments has consequences for CCAPM's investor's behaviour and the way this behaviour is modelled inside the CCAPM's structure. The rational, self-interested and risk averse individual acts to maximize the expected present value of discounted utility from consumption over his/her entire lifetime. This embedded drive directs him/her towards an equilibrium situation in the financial markets in which all supplied and demanded assets are held at the desired price. The centrality of the utility function inside the stochastic discount factor now becomes clear (see *Chapter Two, section 3*). The chosen power utility function has two attractive analytical properties: *First*, it shows that more consumption is preferred to less. Utility is, therefore, an increasing function in consumption. *Second*, the marginal utility of consuming more is decreasing at higher levels of consumption. The power utility function is also used to capture the first order condition for a maximum, i.e. the equilibrium situation (see *Chapter Two, section 5*).

It is interesting to note that two of the triad of temperaments are "expansionary" and one is "contractionary". To see this point clearer, let us first recall that Mill introduced one dominant and two counteracting causes with respect to the individual active in the "Political Economy". They were the desire for wealth on one hand and "aversion to labour" and "enjoyment of costly indulgences" on the other. Next, consider Cochrane. His asset pricing theory takes a similar approach. Also, here there is an interplay between "counteracting forces". Rationality and self-interest might pull decisions in one direction while risk aversion pull in the other. In addition, Cochrane's investor is also held back by budgetary constraints – he/she has a limited endowment - or money to spend. The endowment can be characterized as a technical restriction rather than one associated with the investors intrinsic motivational drives.

In sum, this short historical review of the narrowing of man's character towards a triad of temperaments reveals a process towards a simplified representative investor who is developed to perform very specific tasks inside a narrow area of economic life. In this, Cochrane's version of the CCAPM follows a fairly standard micro-economic methodology. Nonetheless, the definition of the representative investor raises very specific topics that I discuss at some length in *section 1.4* under the heading fallacy of simplification, i.e. reconciling the theoretical cases with the real situations. Before I get there, however, I first move on from the representative investor's character and behaviour, to, briefly, review how Cochrane's version of the CCAPM establishes the economic area, i.e. the financial market structure in which the agent unfolds his/her investing activities and the means he/she is given to balance current and future consumption, i.e. financial assets. Thereafter, in *section 1.3*, I return to the more philosophical question of why CCAPM needs simplifications in all those areas, for the purpose of, as Morgan tells us, “good scientific method”.

1.2 The domain of the financial market activity

From Morgan’s (1997, 2006) historical analysis, I identified three main trends along which economics as we know it today developed; the character of the individual, the domain of activities, and the good scientific method. I introduced the first trend in the previous section. Let us turn to the second here, i.e. the domain of activity and its content, i.e. financial market assets.

1.2.1 The financial market domain

Smith’s *Wealth of Nations* was concerned with nations as aggregates and asked what it is that makes them prosperous. His answer shifted the focus away from previous thinking that wealth is associated with the accumulation of gold and silver, to a competitive exchange-based economy, its main structure and processes. Smith highlights the productive energy of individuals but sees this more in the context of the resulting aggregations towards macro-variables such as investment and trade. His reference to the “invisible hand” as the coordinating mechanism is in this respect legendary.

Subsequent writers, however, were quick to separate the economy from the rest of societal activities. John Stuart Mill, for example, states: “A department of science may thus be constructed, which has received the name of Political Economy.” (Mill 1882, 6.9.3). Mill’s discussions are, therefore, more focused and narrower in scope than what Smith offered. Since then, this tradition has subsequently been upheld by most modern day financial market economists.

Financial economist's theorizing is a good example of how Mill's "Political Economy" was made even more specific. Cochrane often alludes to the modern economy as a complicated entity. In each sector of the economy, a multitude of companies engage in the production of numerous different goods and services that are brought to the "market" for distribution and sale. Individual consumers make decisions regarding what to purchase, where and when, given their preferences and limited budgets. Because it is so challenging to describe the features of the producers and buyers in any detail, economists, such as Samuelson, whom I introduced earlier, reduce real world complexities to something that is perceived to be "manageable". Simpler and more comprehensible structures are thus developed.

Cochrane's domain for asset pricing is no exception, and I gave a short introduction in *Chapter Two, section 5.2*. It is a reduced view of the overall and more complex real financial market situation. He argues: "The central and unfinished task of absolute asset pricing is to understand and measure sources of aggregate or macroeconomic risk that drive asset prices." (Cochrane 2005, p. xiv). By now we know that this theoretical ambition takes place at different levels. At the bottom, we have the representative investor, his/her character and behaviour, in the middle the financial markets and their asset prices, and on the top we find macroeconomic aggregates such as savings, investments, and consumption. This three layer structure is a simplified version of the economic domain. Cochrane thus assembles an accurate theory of asset pricing that combines the three layers to generate an unambiguous equilibrium situation.

In the context of financial markets, therefore, the representative investor is given the choice to consume now and later, and the means, i.e. financial assets to transport purchasing power into the future. But the representative investor is alone. There is in other words no one with whom he/she can exchange assets. Hence, market forces such as competition do not exist. What kind of equilibrium has Cochrane then derived and what can then be said about it? At least it is a peculiar one. Economists characterize the situation as a "non-trade" equilibrium. On this point, Cochrane's version of the CCAPM deviates massively from what is thought of as the "essence" of financial market activities.

In the absences of transactions, the supply of stocks equals its demands, and all stocks are held by the sole investor at equilibrium prices. But this also means that there are no non-equilibrium situations that incentivise the lone investor to reconsider his/her decisions to consume and invest. To simplify the market situations even more, Cochrane assumes that the representative investor does not work and is not independent wealthy. But this does not seem to bother the model-based investor. At birth, he/she is entitled to an endowment, i.e. a starting level of cash. This cash is used for consumption and for the part not immediately consumed, for investments.

Much is excluded from Cochrane's financial market structure. And these elements, structures and relationships go unmentioned. No references, for example, are made to other pending decisions or trade-off's that the investor might be confronted with – for example that of work and leisure. No references are made to other individuals or any societal activities. There are no companies that produce, distribute and sell products and services - or issue stocks for that matter. [39] Neither is there any institutions that provide infrastructure for these activities. There are no banks and no stock exchanges. Nor are there any institutional governance structures or intervention minded public sector servants.

We note, however, that some financial market details are pointed out but neglected. There are, for example, zero taxes on stock dividend and capital gains, and stock transactions are carried out at zero costs. Information gathering, i.e. research is also costless. Markets without taxes and costs are, as financial economists like to call them, "frictionless".

Cochrane, therefore, does with the financial market structure as he did for the representative investor. He lists the minimum requirements that are necessary for the development of a theory of asset prices. Cochrane's simplified version of the representative investor and the financial market structure is certainly a long way from

[39] Lucas (1978), who I introduced in *Chapter 2, section 3.2* as an early advocate of the CCAPM, assumes such production structures.

what can be considered to be real situations. Is this the case for the financial market assets as well?

1.2.2 The financial market assets

Morgan did not cover financial market assets when she reviewed the historical development from the "whole man" to the "economic man". Her intervention had a different focus. In the context of my discussion here, these financial assets are of utmost importance. Without them, the investor could not be called an investor. In fact, of the two decisions he/she was confronted with, the last, i.e. which portfolio of assets to hold, could be disregarded. As a consequence, there would be no theory of asset pricing. It is, therefore, the financial market assets that make an asset pricing theory possible. They are the intertemporal mediator or transporter of purchasing power between current and future expected consumption. But the investable assets in Cochrane's asset pricing theory do not resemble those issued by companies in the real situations.

Consider, for example, the stocks of Google and Caterpillar. Cochrane is interested in their prices, and how they behave through time and in various economic states - for example through economic contractions and expansions. One of the concerns is concerned with the equity risk premium, i.e. the historical return difference between the higher yielding stocks and the lower yielding bonds. This return difference is readily available. It can be extracted by applying basic time series analysis. But, as we know, this is not enough to satisfy Cochrane. He wants to connect the premium to the behaviour of investors. He thus assumes that investors save parts of their endowment, and that stocks are used as a collection vehicle for consumer's surplus cash balances.

But it is not the individual stocks of Google and Caterpillar that the representative investor is confronted with when in a theoretical context. Here, the investor encounters nameless entities. In Cochrane's *story* (see *Chapter Two*, *section 2.2*), we hear about "recession" proof - but not prone - stocks. In the same context, they are referred to as "Arrow Securities". We recall that some financial economists also call these assets "contingent claims" because their payoff is dependent upon the outcome of the future realisation of a particular "state of the world" (see *Chapter Two, section 5.3*). Furthermore, they are assumed to be infinitely divisible – a standard assumption often found in micro-economic text-books.

These terms abstract from the given names of Google and Caterpillar – their empirical counterparts. Arrow Securities do not exist in real situations. They can only be acquired and disposed of in the context of the asset pricing theory. They are thus simplified entities that cannot be expected to be "discovered" in the same sense as the Higgs-Boson particle was at the CERN in July 2012, i.e. as an instantiation of a theoretical prediction. [40]

As he did for the representative investor and the financial market structure, Cochrane does for the financial assets. He lists the minimum requirements that are necessary for the development of a theory of asset prices. In the context of standard economic modelling, this methodological strategy is well established and represents what Morgan, probably, would denote "good scientific method".

With respect to the representative investor, who I introduced in *section 1.1.2*, I pointed towards the upcoming discussion of a topic I denoted the fallacy of simplification, i.e. reconciling the theoretical cases with the real situations. Since Cochrane follows the same blueprint of simplification with respect to financial markets and their assets, this fallacy is also at work in those two other main areas of his theorizing. As already mentioned, I revisit these issues in *section 1.4* where I assess CCAPMs micro-economic foundation. Before I get there, however, I need to introduce and review Cochrane's strategy with respect to his simplifications.

1.3 Simplifications, idealizations, fictions and separateness

In his version of the CCAPM, Cochrane introduces a rational, self-interested and risk averse investor who makes a limited number of decisions within a financial market structure in which financial assets are employed to transport savings into an uncertain

[40] www.cern.ch

future. The dynamics of this situation is wrapped in an eloquent *story* and made explicit in a mathematical structure. I suggest that this set-up can be viewed as simplifications of the real situations they seek to describe, explain and forecast. Simplification is practised, as Morgan probably would agree, for good scientific reasons. As I pointed out in the previous *sections 1.1.2* and *1.2.2*, my concerns are not directed towards this practice but rather towards what I denote the fallacy of simplification.

In this section, therefore, I continue to focus on the microeconomic assumptions that form CCAPM's theoretical basis. At the outset, I show why simplifications of complex real situations have been a time-honoured practice among economists, and let James M. Buchanan and David F. Hendry inform us. Next, I show how such simplifications can be addressed in a more formal context. I will, therefore, let the discussion be informed by Uskali Maeki's views on how simplification can be achieved through a process of "isolation" and how the results can be viewed as "idealizations". I find that Maeki's views are in general shared by Frigg and Hartmann (2006, 2006a).

Following the simplification and the process of isolation and the topic of idealizations, I turn to Morgan (1997, 2001, 2001a, 2004, 2006, 2008, 2008a), who explains that economists often let their analytical starting point take-on fictional traits. I find this is true not only with respect to the representative investor, but also for CCAPM's financial markets structure and its assets. Idealized elements thus carry constructed, fictional, parts. Thereafter, I review Daniel M. Hausman's claim that economics is a science conducted within a "separate realm". As I proceed through my thesis, the concepts of "isolation", "idealization", "fiction" and "separate realm" will gain in importance.

With respect to Cochrane's version of the CCAPM, I draw two conclusions. *First*, Cochrane's methodological strategy is indeed directed towards simplifying investors, markets and assets and isolating them in a separate realm. *Second*, this strategy goes beyond simplification to the development of idealized, and even constructed, i.e. fictional pictures thereof.

1.3.1 Buchanan's and Hendry's simplification

The strategy of suppressing "irrelevant" details from a rich and complex reality is certainly not idiosyncratic to Cochrane. Buchanan explains:

> "At the heart of any analytical process lies simplification or abstraction, the whole purpose of which is that of making problems scientifically manageable. In the economic system, we recognize, of course, that "everything depends on everything else", and also that "everything is always changing". (…) Real problems require the construction of models,…(...). We simplify reality to construct those models but the fundamental truth of interdependence must not be forgotten." (Buchanan 1958, p. 259).

According to Buchanan, "simplification" makes a research project "scientifically manageable" but, he warns we should not be lulled into believing that the dynamics of the real situations, its "interdependencies" and reflexivity are sufficiently accounted for. Hendry takes a similar view and reminds us:

> "I take it as self-evident that economic behaviour is sufficiently complex and evolutionary that it is not helpful to talk about economic theories being "true" or of inferences yielding "correct" results. (...) By their very nature, [*theory*] models are inherently simplifications and inevitable false." (Hendry 1987, pp. 31).

There is thus an agreement between the two authors; theories and models are "simplifications" of a "complex" economy and warning flags need to be raised with respect to finding "truth" when simplified theories and models are applied to real situations.

Cochrane's road from the complexities of the financial economy, its interconnections, dependencies and evolutions to their simplified forms – be it with respect to the investor, the market or assets – is similar to what Buchanan and Hendry allude to. For the sake of "good scientific method", this strategy takes Cochrane from observation of financial market activities, to *stories* that are made "explicit" in theories and models before the model's predictions are compared against the collected data – a veritable 360 degree tour. This sequel has two main components. In the *first* process step, Cochrane seemingly starts in a complex world and ends up in simplified descriptions. In the *second* process step, Cochrane compares the findings emanating from this reduced picture with what is observable on a full scale in the real world. It is about application and assessment. Let us here focus on Cochrane's simplification strategy.

Making things simpler, as Buchanan and Hendry suggest, implies a starting point from which to begin reducing. By definition, the starting point must be fuller or even more accurate and complete than what Cochrane ends up with. This more complete starting point is, therefore, something like Adam Smith's "whole man", i.e. the "real man", real financial markets and real stocks. In principle, Cochrane should find it less problematic to capture the full reality of the individual components of financial markets and their assets than identifying the essence of man. After all, the first elements are of a technical nature and have been designed purposefully by humans. But even in these cases there are hurdles for Cochrane to overcome.

Consider, first, the financial market assets. Can one say that Cochrane sets out with a full description of a real stock, for example that of Google or Caterpillar, and then subtracts properties to get to a simplified structure to be used in his version of the CCAPMs theoretical structure? In some ways yes, but in others not. Both in the CCAPM theory and in real situations, stocks have a price and the potential to pay a dividend to its owners. In real financial markets, however, they are issued by public stock companies and have a specific nominal value, i.e. issue price and they are traded on stock exchanges and electronically moved from one bank account to another. This is not the case in Cochrane's version of the CCAPM. These and many other properties have been removed from the defining characteristics of stocks because they are deemed irrelevant. Their inclusion, it is assumed, will add little to improve the understanding of asset pricing.

Next, can we draw parallels from the example of financial assets to Cochrane's representative investor? Absolutely. Consider first the starting point. The target of Cochrane's theorizing is the "real man". Having first told us *stories* regarding the real investor and his/her demand for recession-proof stocks, Cochrane's individual comes fast and furious. His character is already cut to the bone upon arrival. This simplified individual, also referred to as the representative investor, is straight forward rational, self-interested and risk averse. To quote Amartya Sen: “The *purely* economic man is indeed close to being a social moron.” (Sen 1977, p.336).

However, being a “social moron” might not be troublesome for Cochrane’s asset pricing theory. Neither Smith, nor Mill nor Samuelson gave a complete and accurate description of their starting point, i.e. the “real man”. Smith’s “whole man” was probably as close as it came by letting him be informed by, for example, generosity, sympathy, public spirit and a sense of duty. However, little of this was left after Mill’s and Samuelson’s had put their degenerating knives to his/her character. In fact, we only hear about the most general and, possibly, positive features of the “real man’s” dispositions. Little or no attention at all goes towards, for example, vices and the more darkening temperaments of man. The definitions of human characters, as suggested by Smith, Mill, Samuelson, and Cochrane are, as a consequence, incomplete. Incompleteness in description, however, is a justifiable outcome of simplification. Cochrane’s theoretical effort, therefore, is more concerned with presenting a specific individual convenient for CCAPM’s case than packing all human traits, financial market structures and idiosyncratic stock details into his theory of asset prices and their behaviour. Cochrane thus works with a theoretical construct. But he still wants this construct to give relevant and accurate insights to real situation activities.

Finally, not only have the financial market assets properties and the representative investor’s character been simplified. Also, the complexities of the real market structure in which the agent is active have to a large extent been lost in the process of simplification. Smith’s competition amongst men and trade with foreign nations, for example, were replaced with Cochrane’s narrowly defined financial market structure. It contains just one individual and one asset. Stock prices adjust instantaneous, and an equilibrium situation is reached given the first order condition of the representative investor. In the name of simplification, all other market participants have been removed. There is no other investor to exchange stocks, no private sector companies that issue stocks, no banks, no stock exchanges, no political parties, no regulators, etc. Again, we see that simplification removes what is perceived to be unnecessary details.

It is clear that Cochrane's strategy of simplification is a dogmatic choice as it rests in a neoclassical microeconomic starting point. I will not raise any particular issues with respect to this choice as it reflects Morgan's "reasons for good scientific method". My interest and concerns are more directed towards the consequences of this starting point and the use of their implications. Before I explore them, and in particular the alluded to fallacy of simplification, I need to take the analysis of Cochrane's research strategy to a different level. For that, I will rely, mainly, on Maeki and Hausman. While Maeki informs regarding a more sophisticated strategy of isolation for theory development than Buchanan and Hendry offer, Hausman gives good reasons for accepting Cochrane's narrowly defined financial markets structure and activities.

1.3.2 Maeki's idealizations and Morgan's fictions

While Buchanan and Hendry speak about "simplifications", Maeki takes this methodological strategy one step further. He uses the term "modify" and "deform" in order to make the real situations "manageable": "Faced with the essential complexities of the world, every science is compelled to employ methods of modifying or deforming it so as to make it or the image of it theoretically manageable and comprehensible." (Maeki 1992, p. 317). Maeki's quote thus expands on Buchanan's and Hendry's suggestions. Will this help us to understand better Cochrane's strategy for developing his consumption based asset pricing theory? My answer is yes, and below I explain why.

In order to "modify" and "deform" the complex reality, Maeki introduces the "method of isolation". The method is so called because: "...a set of elements is theoretically removed from the influence of other elements in a given situation". (Maeki 1992, p. 318). Maeki refers to the long historical tradition this method has seen and, in particular, mentions John Stuart Mill and Alfred Marshall as early advocates. He continues:

> “In an *isolation*, something, a set of X of entities, is “sealed off” from the involvement or influence of everything else, a set of Y of entities; together X and Y comprise the universe. (...) Let us call X the isolated field and Y the excluded field.” (Maeki 1992, p. 321).

Let us first review Maeki’s “method of isolation”. I claim that this method, which I will refer to as a process, is at work in Cochrane’s deliberations. Cochrane, namely, introduces the investor and gives him/her particular characteristics. He/she is rational, self-interested and risk averse. Using Maeki’s terminology, this triad of temperaments is “sealed off” from the rest of the individuals’ dispositions and thus included in the X set. Cochrane then tells us how the three elements within the X set interact and that this interaction leads to an optimizing behaviour under the technical term that I introduced in *Chapter Two, section 4.2*, i.e. the first order condition of intertemporal utility transfer. The triad of temperaments, therefore, defines the representative investor’s character. But not only that. In cooperation, and undeterred by other influences, and in particular whatever is stored away in the Y set, the triad produces expected and very specific model outcomes.

Defining the X set automatically establishes the Y set in the sense that Y = 1 – X. The Y set must, therefore, comprise all the rest there is to human character. The X and Y set, taken together, could, therefore, be understood as constituting the “real man’s” full character – if such “summing-up” would be possible. Cochrane does not name what is included in the Y set, but we can imagine that we might find sentiments such as envy, lust, and pride, sense of duty, loyalty, irrationality, behavioural biases, or public sector regulators and competition amongst a multitude of investors for stocks issued by public sector companies. None of these items are discussed in Cochrane’s asset pricing theory. They are as Maeki points out, “excluded by omission” and carry no causal weight. We know, however, that the Y elements do not constitute assumptions used in the asset pricing theory and, more importantly, they do not influence the inner workings of the CCAPM.

In the context of describing various types of models, Roman Frigg and Stephan Hartmann also take interest in the method of isolation. They see the process as: “...stripping away all properties from a concrete object that are believed not to be relevant for the problem at hand. This allows one to focus on a limited set of properties in isolation". (Frigg and Hartmann, 2006, p. 741). While Maeki says "elements are theoretically removed", Frigg and Hartman portray the isolation process as "stripping away" properties from a concrete object. Despite different use of language, I think the three authors would agree on the main issues present in the process of isolation. It is *not* about, as Smith suggested, creating a starting point in the definition of the "whole man" which would include a full X + Y set of human traits. No, it is more about Samuelson's "rational economic man" who arrives, stripped of complexities and perceived irrelevant details, containing only relevant and manageable character traits that are useful and appropriate for the modelling effort.

In Cochrane's context, there is little, if any, evidence that his starting point is a definition of the complete X + Y set of human traits, and, from there, develops his version of the CCAPM that, eventually, comes to rest in the triad of temperaments. Cochrane's starting point, as I alluded to above, is a dogmatic one, well nested within a neoclassical economic setting. This does, however, not imply that Cochrane is unaware of other influences that could affect the decision-making process of his representative investor. In *Chapter Two, section 6.3.4* on the topic of consumer's "habit-persistency", for example, I show how Cochrane goes beyond the triad when faced with empirically inadequacies of the claims emitted from the CCAPM. Nonetheless, I do not want to pre-empt my discussion on this topic which is at the core of *Chapter Five*. I would here argue that Cochrane's strategy is one of simplification through the process of isolation as described by Maeki, Frigg and Hartmann. This strategy is considered to be, I want to emphasise, a common methodological practice within the field of finance, and, more generally, economics. It does, however, raise several serious questions that I will discuss at length later in *section 1.4*.

But Maeki has more to say with respect to isolation:

> "Isolation in the present sense is sometimes also called *idealizations*. In this usage, idealization is understood widely, so to encompass almost anything that theoretically deforms reality. For the purpose of the present essay, I will be using "idealization" in a narrow sense in which idealizations are formulated in terms of limiting concepts designated or designatable with variables 0 or $|\infty|$ (...). Idealizations are unrealistic in the straight forward sense that they are false statements (...)." (Maeki 1992, pp. 324).

We here, *first*, learn that the process of isolation leads to *idealizations*, *second*, that *idealizations* are "limiting concepts" and, third, that such *idealizations* "deform reality", are "unrealistic" and even "false". Let me accept the first suggestion and spend the next few paragraphs on the remaining two statements on possible *idealization* in Cochrane's version of the CCAPM.

Maeki explains that *idealizations* are easily spotted in economics: "Examples are assumptions of full employment, zero transaction costs, zero cross-elasticities, perfectly divisible goods, and infinitely elastic demand curves." (Maeki 1992, p. 324). In these examples, Maeki tells us that economists use the variables 0 and $|\infty|$ to idealize concepts used in their theories. We immediately see how Cochrane's own research effort follows this strategy when formulating the asset pricing theory. He names, for example, taxes and transaction costs that are "nullified", that stock prices adjust instantaneously, i.e. infinitely fast, and stocks' infinite divisibility. These technical variables are thus explicitly mentioned and included in the theory of asset pricing but idealized through their extreme values. These statements can be considered, as Maeki suggests, to be "deliberate falsehood" when compared with real situations. Real financial markets do not exhibit such properties. And Cochrane is certainly aware of this.

I do not take issue with the *idealizations* just mentioned. They can be regarded as "technical" and overcome easily by de-idealizing them in the theoretical framework and, thereafter, mathematically modify them in the context of CCAPM's equations. De-idealization would then correct the idealized situation and take the theory a step closer to the real situations it sets out to describe, explain and predict. Transaction costs, for example, may be introduced in the theory and given a numerical value in the model. I am, however, more sceptical towards *idealizations* related to the representative investor.

Maeki told us in the above quote that theorizing with respect to complex situations involves “deformation” that might lead to “unrealistic” situations and “false” statements. With respect to the economic agent, Maeki sees him/her as a: “...strongly idealized and isolated version of ordinary humans”. (Maeki 1992, p. 334). Such conclusions are not uncommon amongst philosophers of science. Frigg and Hartmann, for example, claim that an *idealization* “involves deliberate distortions” and tell us that: “Physicists build models consisting of point masses moving on frictionless planes; economists assume that agents are perfectly rational; biologists study isolated populations,...” (Frigg and Hartmann 2006, p. 742). The perfectly rational agent is also according to Frigg and Hartmann “distorted”.

As we know by now, in the context of CCAPM, the representative investor is characterized by the triad of temperaments. It defines his/her behaviour in situations where choices with uncertain outcomes are forced upon him/her. The triad, since they were the outcome of a process of isolation, is, according to Maeki, Frigg and Hartmann *idealizations*. But not only that. They “deform” real situations, are “deliberate distortions”, “unrealistic” and, even, “false”. I believe these rather strong statements can be applied to the outcome of Cochrane’s theory development as well. It goes without saying that the triad of temperaments exemplifies such deformations of real situations.

Nonetheless, Mary S. Morgan expands on the topics of simplification, isolation and idealization. I believe she thinks they take theorizing beyond idealizations into the field of *constructed fictions*. (Morgan 2006, 2011). She claims:

> “In neoclassical economics of the mid-twentieth century, economic man held an idealized character, one no longer taken to represent real man, but to be an artificial character created by economists. No longer one whose behavior could be imagined, and so understood partly at least through introspection, but a construction of artifice that took economics into the mathematics laboratory.“ (Morgan 2006, p. 22).

Constructions, Morgan claims, can be traced back historically to the way generations of economists, since Smith, have "constructed a highly idealized portrait of economic man ... according to the theory of the day". More importantly in the context discussed here, however, is how simplifications of the real man have introduced elements that this individual has yet to portray. Morgan, in particular, refers to Knight who endowed this agent with "full information" and "perfect foresight" (Knight 1921).

It is not difficult to find such constructed elements in Cochrane's theorizing. In the financial markets, for example, Arrow-Securities are defined, but no transactions take place. Additionally, the representative investor's triad of temperaments is complemented with the almost perfect foresight and an infinite live-span. (see *Chapter Two, section 3.4* and above *sections 1.1* and *1.2*). Focusing on the representative investor, his/her two additional properties have yet to be identified as part of man's dispositions. No one has almost perfect foresight and live eternal, earthly, lives. In this sense, these properties cannot originate from simplifications during a process of isolation that "theoretically remove" or "strips away" elements from a real situation starting point. Close to perfect foresight and infinite lifespan are, therefore, the results of theorizing. They do not originate from observation or introspection. I, therefore, argue that these additions to the idealized version of man willingly "distort" the real counterpart for the sake of "good scientific method" in a different way than those achieved through the process of isolation. Hence, *constructions* such as these extend the concept of idealizations. They create *fictional characters*. In *section 1.4*, I show that this view has implications for how the asset pricing theory is used to portray real situations. In particular, I am concerned that Cochrane, without further ado, lets this fictional "model man" represent the "real man".

At this point, therefore, I conclude that the process of isolation helps explain how Cochrane reduces complexities in the real financial world to make it more manageable in a theory building context. The theory of asset prices deliberately distorts real situations by the process of isolation, and idealizes its inhabitant, the financial market area as well as the financial assets. But the theory of asset pricing extends beyond mere idealizations.

In all these three areas of assumptions, Cochrane adds constructed elements. It turns them into fictions. The theoretical development of fictional elements, however, does not necessarily imply that the outcome is useful for those awaiting advice. It certainly questions whether such insights are helpful in understanding the real situations.

1.3.3 Hausman's separate realm

In the previous sections of this chapter, I mainly emphasized the representative investor and his/her character. He/she is a simplified, distorted, idealized and, in many aspects, fictional representations of his/her real situation counterpart. Here, I want to take a closer look at the domain of activity, i.e. the financial market within which the investor makes his/her dual decision – how much to consume now and later and which portfolio of assets to hold – based in his/her triad of temperaments. As was the case with the representative agent, Cochrane is not overly explicit on how he arrives at this financial market structure, its boundaries and inter-linkages to other fields of economic and social life. With insights drawn from Daniel M. Hausman, I hope to bring more clarity to Cochrane's strategy.

In his book *The inexact and separate science of economics*, (1992), Hausman argues that: "Economics is governed by a coherent vision of its overall theoretical mission". (Hausman 1992, p. 90). In an effort to explain the "vision" of theoretical economics, Hausman argues that there is an underlying structure to the "project". This structure is "generally accepted" but "rarely explicitly stated". Hausman seeks clarity in four claims that supposedly establish "the vision of economics as a separate science":

> "1. Economics is defined in terms of the causal factors with which it is concerned, not in terms of a domain.
> 2. Economics has a distinct domain, in which its causal factors predominate.
> 3. The "laws" of the predominating causal factors are already reasonably well known.
> 4. Thus, economic theory, which employs these laws, provide a unified, complete, but inexact account of its domain." (Hausman 1992, p. 90).

Let me here focus on the two first claims that Hausman presents as they are most relevant for my discussion of Cochrane's asset pricing theory. My question is straightforward.

Does Cochrane subscribe to Hausman's formulations, and can I argue that Cochrane seeks to establish a separate area of asset pricing? The answer to this question has importance for the discussions on "research projects" that I commence in *section 2* of this chapter.

In the *first* claim, Hausman refers to causal factors. He identifies them from within the standard paradigm of the basic neo-classical "equilibrium theory". In particular he mentions the "theory of consumer choice" which comprises "rationality", "consumerism", and "diminishing marginal rates of substitution". (Hausman 1992, p. 30). These three elements define what Hausman refers to as "rational greed". He next asserts that economics is defined by these causal factors. A reference is also made to the domain. In this respect, we are told that economics is defined, primarily, by causal factors and not in terms of ring-fencing a specific "domain".

Consider, first, John Stuart Mill who also started his inquiry into economics from the point of view of main causes. He went on to explore the consequences of these causes when acting together in agents' "pursuit of wealth". This method could be used in the separate area that Mill denoted "Political Economy". Causes, therefore, come first and are followed by the domain of activity. In view of Cochrane's research effort, I think he could subscribe to Hausman's first claim, and also side with Mill's views. Cochrane's representative investor, with his/her triad of temperaments, is the starting point for subsequent theorizing. Nonetheless, I do not think there is anything distinct in CCAPMs triad that would earmark it for the exclusive use in asset pricing theories. In fact, these temperaments are active in many strands of life and found in various social science contexts. I have mentioned, for example, that von Neumann and Morgenstern used them in the context of any decision making situation in which the final outcome is uncertain. In particular, the two authors referred to gambling. As a consequence, turning the triad on in a theoretical context does not define in which area they have been activated. The definition of that area is a separate decision.

This takes me to Hausman's *second* claim. I think Hausman's "rational greed" can be viewed as a necessary but not a sufficient condition for establishing the separate realm that he advocates. Sufficiency, in Hausman's view, is reached when economists use the rational greed credo in the context of a "partial equilibrium theory". Cochrane, again, can be seen as a follower of Hausman's analysis. The presence of the triad of temperaments leads the investor to the first order condition. The dynamic of this set-up plays itself out within a financial market structure that provides financial assets as intertemporal transporters of purchasing power. A stable equilibrium situation is thus reached when all assets are held at "market clearing' prices. Both Hausman's rational greed and Cochrane's triad of temperaments, therefore, need a backdrop against which the consequences of their causal factors can play themselves out.

Without these additional elements of optimizing behaviour, financial markets and assets, the triad of temperaments, or Hausman's rational greed for that matter, does not on its own define the consumption based asset pricing theory. It is only in their combination and interaction that they make the theory, *first*, complete, and, *second*, internally consistent. This mutual interdependence thus reinforces and ring-fences Cochrane's asset pricing project. If one of its main elements is removed, the consumption based asset pricing theory crumbles. Cochrane, therefore, establishes what Hausman calls "a coherent vision of its overall theoretical mission". Cochrane so establishes asset pricing as a separate science by "explicitly stating" all elements of its theoretical structure.

It is now a good time to assess Cochrane's microeconomic structure upon which he develops his asset pricing theory. This is the content of the following section.

1.4 The fallacy of simplification

It is certainly challenging to give an assessment of the important role which the microeconomic structure of Cochrane's asset pricing theory plays. Earlier, I established that Cochrane takes the much travelled road of economic methodology that aims at simplifying the complexities of the real investor, financial markets and their assets through a process of isolation. At the end of this process, I found a separate economic realm that was defined by idealized and constructed, even fictional, versions of the real situations. I critically alluded to the challenges of reconciling the theoretical cases with the real situations as the fallacy of simplification. Below, I discuss how this fallacy reveals itself in Cochrane's version of the CCAPM.

1.4.1 The dogmatic starting point

Let me start with the neoclassical economic paradigm in which Cochrane's asset pricing framework originates, i.e. the dogmatic starting point which forms the basis for the fallacy of simplification.

This well-rehearsed economic framework, as I have argued throughout this *section 1*, establishes three main simplified, idealized and constructed elements, i.e. the investor and his/her triad of sentiments, the financial market structure, and the financial market assets. But how does Cochrane justify a starting point in narrow and rather thin characterizations? In fact, he does not. How can we then know that, for example, the triad of temperament, i.e. rationality, self-interest, and risk aversion are primitives? That they are coherently isolated from the rest of the investors' sentiments? That the triad form a closed system? That the triad of sentiments, and nothing else, matters in economic choice situations with uncertain outcomes? The closest Cochrane gets to answering these questions is as follows: "Something like the consumption based model – investors' first order conditions for savings and portfolio choice – has to be the starting point." (Cochrane 2005, p. 455).

Cochrane's justification for the basis of his asset pricing theory is certainly meagre. *First*, it seems that we are just presented with what Cochrane seems to take as a "known fact", i.e. it is so obvious that it meets only a few, or if at all, any objections. Above, in *section 1.3.2*, I referred to Morgan's analysis of the "economic man" and his "artificial character created by economists". In Cochrane's context, this means that the representative investor is not established as initially discussed by Smith, Mill and Jevons, thorough observation and introspection. No, as I have indicated at several occasions, and this is my *second* point, Cochrane's representative investor is the outcome of theorizing. And this is where in particular Cochrane's construction of this individual is of importance. As I described in *Chapter Two, section 3.4*, for example, the investor knows the payoffs on all assets in all possible future economic states. The investor is also given perfect foresight with respect to all possible future economic states. Not only that. Also the probabilities of their occurrence are assumed to be known to the investor. The investor, however, does not know which of these states will occur. This gives him/her close to perfect knowledge and foresight. In addition, he/she lives an infinite life. Certainly, these add-ins do not reflect circumstances found in real situations.

I am most sceptical towards Cochrane's methodological strategy of enriching the simplifications that indeed have real situation counterparts, such as rationality, self-interest and risk aversion, by adding in properties that do not exist in real situations. Perfect foresight across time and economic states and immortality are not only fictional but simply false. I raise similar concerns related to Cochrane's theorizing with respect to the financial market structure and the financial assets (see previous *sections 1.2.1* and *1.2.2*).

By not having a justifiable starting point, Cochrane might be on thin methodological ice. He could certainly claim that it is perfectly fine to start out with simplified, idealized and fictional characteristics of real situations. In this sense, he has coherently isolated the right characteristics that are set to interact with each other in a closed system undeterred by any outside influences. If this is a possible answer, I would make the following remarks.

First, as I explained in the previous *Chapter Two*, i.e. the asset pricing "primer", Cochrane has developed an internally consistent theory of asset pricing. He sealed off the main causes of character and behaviour and showed how they interact towards creating an equilibrium situation in the financial market. More important than technical dexterity, however, is whether he has sealed off the right characteristics. Above, I discussed my scepticism towards his justifications. *Second*, even if the right characteristics have been identified, how can they be successfully demarcated from "the rest"? Would we not want to know what happens when the representative investor is embedded in a societal context, i.e. when savings and investment choices are made based upon other influences as well? In other words, how sensitive are Cochrane's theoretical and empirical results to the introduction of a more complex set of such "moral" and "altruistic" considerations? I am thus concerned with the representative investor's life in a vacuum outside of professional and social interactions. I claim that Cochrane cannot justify that the listed properties within the asset pricing theory are independent of "the rest". *Third*, Cochrane's methodological strategy obviously sets the asset pricing theory a long distance away from it targets. My question in this respect concerns the consequences of this strategy. Does the starting point limit our ability to explain and predict the real situations? Are such explanations and predictions harmful to the user of such asset pricing advice? Let me analyse possible answers in the next section in which I review the second part of the fallacy of simplification claim, i.e. the consequences of the dogmatic starting point.

1.4.2 The consequences of the dogmatic starting point

In this section, I address the consequences of Cochrane's dogmatic starting point with respect to his asset pricing theory. Ultimately, the derived consequences must go beyond the consistency of theoretical work and its technical dexterity. I believe it must be shown that the results are tangible and useful for its stakeholders, i.e. investors, public sector, and fellow academics. Stakeholders would, therefore, need independent and objective advice on the consequences of using the CCAPM derived results that, as we know from the previous section, works from a dogmatic starting point of simplified, idealized and constructed characteristics of the investor, the financial markets and its assets.

Since Cochrane is conflicted in this process and, in addition, does not do a lot to justify the starting point, the stakeholders could consult the outcome of empirical tests. However, as I indicated in *Chapter Two, section 6*, the CCAPM's statistical success is limited. Since this is a stated fact, the main justification for Cochrane's dogmatic starting points is not well supported. This is certainly a problematic situation for Cochrane and his version of the CCAPM. Without pre-empting my discussion on CCAPMs empirical assessment in *Chapter Five*, I will, below, point to a few areas in which the fallacy of simplification is evident. My first point is related to the gap between the theory's starting point and the real situations. My second point addresses the role of Cochrane's *stories*.

My *first* point is directed towards the fact that most observers of financial market activities agree that there are more to real situations than what the CCAPM portrays in its theories. So why should we care about such a simplified, idealized and constructed version of man, markets and assets when we know that we will not encounter them in real situation? We would care, I believe, if we knew that the gap between the real and the theory's output is insignificant, and claims of statistical success is warranted. Since we are told that the latter is, so far, not the case, let me focus on the former.

At the "heart of this story", as Hausman (1992) calls it, we find the investor in a financial market setting involved in consumption and investments. When describing these elements, Cochrane distances himself from the complexities that are visible in the real situations. He simplifies and thus aspires to extract what he believes are the essentials for the consumption based asset pricing theory. These essentials are all idealized and constructed fictional entities which exist in a mathematical framework. As a consequence, they give us pictures of processes and results that with absolute certainty obtain in the theoretical analysis. This information is of value in that specific context. Given the context dependency, however, CCAPM cannot be expected to inform about other situations, i.e. those observable in the real financial markets. Idealized and constructed, and even fictional, theories that claim to capture the essentials of financial market activities are thus distances away from the real situations. This is unproblematic, some would argue, if statistical success is achieved. This is, however, not the case for the CCAPM.

The difference between the theory and the real situations is also visible in the use of language. Characterizations of financial markets are part of our day to day vocabulary. In contrast to these every-day characterizations, we are also being exposed to the various technical concepts such as utility, stochastic discount function, subjective risk aversion, first order condition, contingent claims, and frictionless markets. Cochrane is certainly aware of these differences, but he does not go to any length addressing them. In fact, he leaves me with the impression that one may exchange everyday vocabulary with technical characterizations. Stakeholders might, therefore, be (mis)led to believe that there are no significant differences between the real counterparts and CCAPM's technical characterizations.

There are several examples that demonstrate the difference between the real situations and their idealized, constructed, and even fictional counterparts in the asset pricing theory. The most prominent, again, is the difference between the real and CCAPM's representative investor. On one hand, the representative investor is given extraordinary capabilities, but on the other hand, the investor is deprived of crucial human traits. Included sentiments make the representative investor an idealized and constructed version of his/her real situation counterpart. We thus know what is isolated and included in Cochrane's asset pricing theory, but we are not told how and why other aspects of the investor were omitted. The same hold for real and theoretical financial markets and assets. For example, it is costly to execute stock-transactions in the financial market. And investor, mostly, pay taxes on payoffs, i.e. dividend and capital gains. Market frictions, therefore, influence real investors' behaviour. These frictions are removed in the CCAPM context.

Where do these comparisons take us? My concern is that the consequences of the fallacy of simplification, i.e. the theory-based results, are carried over, with no further ado, to the real situations thoughtlessly by not distinguishing clearly between every-day "speak" and the technical counterparts. Such misinterpretation might have real effects as stakeholders are prone to make wrong decisions regarding their consumption and investment because they mistook theory-based knowledge with reality. This is certainly a problematic issue. But Cochrane does not seem to address it effectively. In his defence, he points towards the strategy's limited statistical success, but he does not "hammer" this point home enough. Cochrane also fails to say convincingly that the idealized and constructed versions of investors, markets and assets do not provide sufficiently good approximations of the real situations. Where Cochrane is good, however, is how he tells *stories* with respect to his perception of financial market activities. And this is where my *second* point comes in.

We already encountered Cochrane's *story* regarding financial market activities in *Chapter Two, section 2.2*. I will not repeat it here. The point to make, however, is that his good and plausible *stories* might obfuscate real investor's view of financial markets. They might feel confident that what they hear is accurate. In this respect, they are prone to overlook the lack of statistical success and focus on this intuitive narrative. This again might lead investors into making decisions that turn out to be built on a weak foundation. In *Chapter Four, sections 1.2.2* and *4.4* as well as in *Chapter Five, section 5.2*, I will discuss the methodological importance of *stories* in Cochrane's version of the CCAPM.

2. CCAPM and its assumptions

Cochrane's asset pricing research effort finds inspiration from a well-rehearsed neo-classical economic back-drop. He uses this back-drop to establish a theoretical framework around the three main areas which define his asset pricing project, i.e. the investor, his/her character and behaviour, the financial market structure, and their assets. The purpose of this section is to continue analysing these old and more newly developed assumptions. My issue is not so much the fact that there are, as Cochrane claims, "lots" thereof. My goal here is more to assess their methodological development and their relative importance and cooperation in developing a theory of asset pricing that, possibly, is progressive.

My review shows that Cochrane is not overly explicit with respect to creating and maintaining a list of both stable and fleeing assumptions. Nonetheless, throughout his writings, Cochrane seems to want such an order. It should, therefore, be possible to bundle these assumptions into clusters and combine them to create this overarching unity. I argue that a hierarchy of assumptions is of value. It helps distinguish the central assumptions from the less so. Not only does such a hierarchy facilitate ex-ante theoretical development but it also help guiding ex-post theoretical adjustments and changes should empirical tests fail to support the claims emitted from the theory. I find support for this view in the publications of two contemporary philosophers of science, Nancy Cartwright and Ronald N. Giere.

2.1 Some assumptions are more fundamental than other

From *Chapter 2*, *sections 4.1 and 4.2*, we know that Cochrane assembles the *central asset pricing formula* based upon *the* theory of asset pricing. Before Cochrane gets to the central formula, he presents the *basic pricing equation*:

$$p_t = E_t\,(m_{t+1}\; x_{t+1}).$$

Cochrane elaborates: "Given the payoff [x_{t+1}] (...) and given the investor's consumption choice [m_{t+1}] (...), it tells you what market price (...) to expect. Its economic content is simply the first order condition for optimal consumption and portfolio formation." (Cochrane 2005, p. 6). Cochrane's statement makes references to the "economic content" which is the first order condition of consumption and portfolio choice, or mathematically in the *central asset pricing formula*: (see *Chapter Two, section 4.2, footnote 22*):

$$p_t \;= E_t \left[\beta \, \frac{u'(c_{t+1})}{u'(c_t)}\, x_{t+1}\right]$$

At the centre of this formula, we find the stochastic discount factor (SDF) consisting of the subjective discount factor β and the, marginal, utility function $u'\,(c_{t+1}\,)\,/\,u'\,(c_{t,})$.

The topic I want to pursue here is not the number of assumptions supporting the central asset pricing formula or the consequences of applying them, but whether they are of equal importance. I will make the point that Cochrane is not overly explicit with respect to their relative importance but that establishing such a hierarchy of assumptions is of benefit to his research project.

In the next sections, I again focus on the representative investor and arrange the assumptions that support his/her character and behaviour into two clusters that I denote “fundamental principles” and “auxiliary assumption”.

2.1.1 Fundamental principles

My task ahead is to disentangle the central pricing formula with its mathematical symbols and assumption-filled content and extract those elements that are more essential than others. Importance, however, is a relative concept. Let us, therefore, inform ourselves. Philosophers of science, for example, are also interested in assumptions and the role they play when developing theories. Two prominent references are Cartwright (1983, 1989, 1999, 2004, 2007, 2008) and Giere (2004, 2008). Cartwright, for instance, asserts that theories rely on both principles and assumptions. Her focus is on the principles. Drawing also on Hempel’s insights (1965, 1966), Cartwright tells us that there are two groups of principles. The *first* group has many names; theoretical-, internal-, first- or *fundamental-principles*. The *second* is called bridge-laws or *bridge-principles*. Here, I am interested in the fundamental principles. Later, in *Chapter Four, section 4.4.3*, I briefly discuss the use of bridge principles.

Natural sciences, and in particular physics, we are told, are rich in *fundamental principles*. Newton’s Principles of Mechanics, Maxwell’s Principles of Electrodynamics, Principles of Quantum Mechanics, and Principles of Thermodynamics are often used as references. Consider, for example, Newton’s Second Law. It is mathematically expressed as $\mathbf{F} = \mathbf{m}\ \mathbf{a}$. Cartwright and Giere agree that this statement is neither an empirical nor a modal generalization over existing entities. Their properties do not occur universally or by necessity. The two authors also agree that Newton’s second law does not present a “vehicle for making empirical claims”:

> “Newton’s three laws of motion, for example, refer to quantities called force and mass, and relate these to quantities previously well understood: position, velocity, and acceleration. But they do not themselves tell us in more specific terms what might count as a force or mass. So we do not know where in the world to look to see whether or not the laws apply.” (Giere 2004, p. 745).

Cartwright goes on to assert that the social sciences, such as economics and finance, have only a limited number of fundamental principles. She explicitly refers to two; the principle of "utility maximization" by agents' rational behaviour, and the principle of "market equilibrium". Giere is supportive. He also refers to "various equilibrium principles", in particular to the economic Principle of Nash Equilibrium. Both Cartwright and Giere, therefore, share the opinion that fundamental principles are present in both natural and social sciences. If this is the case, these principles share common characteristics. Above I pointed towards three: they are neither universal nor modal generalization and they cannot be compared directly with the real situations.

How does this resonate with Cochrane's asset pricing approach? I think quite well. Here is why. From the aggregate consumption data, information related to the state contingent payoff and the prices of the financial market assets under consideration, Cochrane estimates the representative investors utility function. At the heart of this function, we find the agent and his/her given three sentiments. This particular character is exposed towards a choice situation with the uncertain consequence that he/she might not receive the expected pay-off to upkeep his/her desired level of consumption. By carefully considering which assets to hold in his/her portfolio, he/she, unknowingly, brings the financial markets forward towards the equilibrium situation. This narrative, I think, gives a rich foundation for exploring the existence of fundamental principles. Let us start with the investor.

I suggest that Cochrane, indeed, works with a few crucial assumptions when establishing "*the* theory of asset pricing" and assembling "*the* central asset pricing formula". They are found with respect to the investor. How fundamental are they? I think we can find assumptions that deserve to be characterized as fundamental principles. The investor's behaviour is determined by his/her character, i.e. he/she maximizes utility. And his/her character and behaviour, as we know, rests on the triad of temperaments. I believe Cochrane's commitment to these concepts is near to dogmatic. Without the investor, his/her character and forced behaviour, there is no consumption based capital asset pricing theory.

Is the triad of temperaments universal or modal, and can they be compared with the real situations? It seems difficult to imagine a social setting in which the triad is entirely absent. These sentiments, as I have pointed out earlier, may be operating in a reduced mode or they are possibly counteracted by other, unnamed, "disturbing" causes. Are they universals? We do not know the final answer to this question. When confronted with decisions with uncertain outcomes, have all people at all times in all places been guided by the triad? Will their behaviour in all future situations be a consequence of these temperaments only? Will we know that the triad has been completely absent in some choice situations with uncertain outcomes? Is the triad modal? Does it hold by necessity? I think these are difficult questions to answer in the context of my discussion on asset pricing. Nonetheless, I believe Cochrane wants them to be testable. In sum, therefore, I suggest referring to rationality, self-interest and risk aversion as the three fundamental principles of consumption based asset pricing theory.

What about the financial markets and their assets? Are there principles therein that we can characterize as fundamental? My answer is "no". Consider, for example, the absence of taxes and transaction costs, one agent universe, infinite lifespan, no labour income, the statistical distribution of return, etc. They are not fundamental to the theory nor can they be characterized as universal or modal. Comparing such statements with respect to their accuracy relative to the real situations is meaningless because we know they are absent in real market situations. These concepts belong to a different category of assumptions that I describe in the next section.

2.1.2 Auxiliary assumptions

Having written down the *central pricing formula*, Cochrane insists: "Most of the theory of asset pricing just consists of specialisations and manipulations of the formula." (Cochrane 2005, p. 6). He continues: "The rest is elaboration, special cases, and a closet full of tricks that make the central equation useful for one or another application."

(Cochrane 2005, p. xiii). Cochrane here refers to the way the formula can be given more structure by adding in assumptions. Which are they? Cochrane answers, as I indicated in *Chapter Two, section 5*:

> "Writing ***p = E(m x)*** we do *not* assume: (1) Markets are complete, or there is a representative investor; (2) Asset returns or payoffs are normally distributed (no options), or independent over time; (3) Two-period investors, quadratic utility, or separable utility; (4) Investors have no human capital or labour income; (5) The market has reached equilibrium, or individuals have bought all the securities they want to. All of these assumptions come later, in various special cases, but we *have not* made them yet. We *do* assume that investors can consider a small marginal investment or disinvestment." (Cochrane 2005, p. 35).

I would like to divide Cochrane's statement into two parts. The first part captures the assumptions he makes when spelling out the *central pricing formula* and the other part contains the assumptions "not yet made". The first is straight forward. Cochrane says that the investor "can consider a small marginal investment or disinvestment". This takes us back to the triad of temperaments, i.e. the rational, self-interested and risk averse representative investor who contemplates the effect of marginal changes in his /her consumption and investment plan. I have already sorted these assumptions to the group of "fundamental principles". Point (5) in the above quote is also notable. Here, Cochrane claims that the equilibrium condition is not contained in the basic pricing equation. I agree. It is more the consequence of what the representative investors are told to do.

The second part of Cochrane's statement is more complicated to grasp. It lists several, seemingly, unrelated assumptions. They have in common, however that they can all be attached to the *central pricing formula*. When that occurs, they create what Cochrane alludes to as elaborated, distinct analytical cases that are "useful for one or another application". In *Chapter Two, section 5* and in the previous *section 1.3* of this chapter, I have already pointed towards the assumptions that develop idealized and fictional investor, financial markets and assets. From a theoretical point of view are extremely helpful in developing the consumption based asset pricing research effort.

I, therefore, think that the items listed in (1) to (4) represent a second category of assumptions that provide Cochrane with flexibility in the choices of where to take the basic pricing equation. They help refine the investor and his/her behaviour, the financial market structure and the financial assets. But such assumptions are neither dependent upon the fundamental principles, nor are they necessarily the derived consequences thereof. For example, a complete market is not dependent upon investors being rational; an infinite life-span is not the derived consequence of the investor's risk aversion; the investor's self-interest does not need a contingent claim market.

I refer to assumptions in this category as *auxiliary assumptions*. A combination of fundamental principles and auxiliary assumptions, therefore, creates flexibility with respect to Cochrane's research effort in deriving their combined consequences, i.e. various analytical cases that I expose in more detail in *Chapter Four*. The principles and assumptions also combine to make the theory of asset pricing internally consistent.

2.2 A hierarchy of assumptions

The philosophers of science, Cartwright and Giere, informed us regarding the fundamental principles, bridge principles and auxiliary assumptions. When combined, new and more granular cases can be derived, and several of these cases can be assessed against the empirical situations. Cochrane, however, does not use these terms. He prefers calling all of them "assumptions". Yet he seems to endorse the notion that some of the assumptions are more important than others. In the past few sections, I confirmed this. I followed Cartwright and Giere's advice and labelled them "fundamental principles". Furthermore, assumptions that are not a fundamental principle are, as a consequence, "auxiliary assumptions".

Specifically, I argued that the representative investor's three characteristics, i.e. rationality, self-interest and risk aversion can be considered to be fundamental principles. In contrast to Cartwright and Giere, however, I do not consider the equilibrium condition to be a fundamental principle. It is rather the outcome of the investors' considerations of incremental investments in order to maximize his/her discounted total expected utility. These considerations take financial markets towards the equilibrium situations in which "markets clear" at a specific asset price. The representative investor does not even know that this is the outcome of his/her activity. Additionally, a large set of assumptions supports the fundamental principles along the way. Good examples are the eternal life span of the investor, contingent claim securities, and almost perfect foresight, zero taxes and transaction costs. These assumptions give the *central pricing formula* more structure.

If this categorization of assumptions into two compartments is acceptable, we also have a hierarchy of assumptions, i.e. the first set is more important than the second. However, we need to take the analysis one step further. Cochrane frequently refers to *the* theory of asset pricing. It gives the impression that there is a single theory spanning his entire asset pricing research project. It certainly is Cochrane's goal to give such a holistic view, i.e. to provide: "...a complete answer to all the questions of the theory of valuation." (Cochrane 2005, p. 42). Scientific ambition might aim that high but few involved in his or other finance related research projects would claim to have reached such a general level yet. How can Cochrane's theoretical ambitions best be characterized?

3. Imre Lakatos' research programme and the CCAPM

In the previous two sections of this chapter, I showed how Cochrane's hierarchy of assumptions can be interpreted as establishing simplified, idealized and fictional versions of the investor, markets and assets. This model-based outcome is certainly a long distance away from the real situations they are seeking to represent. In *section 1.4*, I referred to this gap as the fallacy of simplification. In this section, I ask how Cochrane's research effort can be characterized from a philosophy of science point of view. To answer this question, I turn to Mark Blaug's classic *Methodology of Economics* (1992). Therein, he asks what the nature of economic explanation is: "Insofar as these explanations consist of definite theories, what is the structure of these theories, and, in particular, what is the relationship between the assumptions and the predictive implications of economic theories?" (Blaug 1992, p. xxv). He reviews several possible answers given by various economists and philosophers of science. In the context of my discussion, Blaug's coverage of Imre Lakatos' response stands out as a promising back-drop for my own endeavour to characterize Cochrane's CCAPM research effort. But there are also other alternatives that might be used. I am, like Blaug for example, thinking of Kuhn's (1970) and McCloskey's (1988) elaborations on, respectively, the progress of science and, more specifically, economic rhetoric. Instead of characterizing these alternatives, I rather point towards the similarities between Lakatos' description of what a research programme is and how Cochrane complies with it without falling prey to Lakatos' many critics.

The section has six parts. I *first* review the four main pillars that characterize what Lakatos refers to as a "research programme" and show the parallels to Cochrane's version of the CCAPM research effort. *Second*, I describe the historical development of Cochrane's version of the CCAPM. *Third*, I compare what I called CCAPM's fundamental principles and auxiliary assumptions with Lakatos "hard core" propositions and "protective belt" hypotheses. *Fourth*, I show how the CCAPM uses the positive and negative heuristic. *Fifth*, I discuss whether the CCAPM is progressive or degenerating. *Six*, I point towards articulated criticism of Lakatos' description of research programmes. I argue that the criticism can be withstood in the context of my analysis.

3.1 The "research programme"

Blaug, who I referred to above, claims:

> "Lakatos begins with denying that individual theories are the appropriate units for making scientific appraisals; what ought to be appraised, and what inevitable is appraised, are clusters of more or less interconnected theories or scientific research programs." (Blaug 1992, pp. 32).

How does Lakatos characterize the structure of such scientific research programmes? Lakatos explains:

> "All scientific research programmes may be characterized by their "*hard core*". The negative heuristic of the programme forbids us to direct the *modus tollens* at this "hard core". Instead, we must use our ingenuity to articulate or even invent "auxiliary hypotheses", which form a protective belt around this core, and we must redirect *modus tollens* to *these*. It is this protective belt of

> auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core. A research programme is successful if all this leads to a progressive problemshift; unsuccessful if it leads to a degenerating problemshift." (Lakatos 1978, p. 48). [41]

Lakatos offers several examples of a research programme. Here is but one:

> "Now, Newton's theory of gravitation, Einstein's relativity theory, quantum mechanics, Marxism, Freudianism, are all research programmes, each with a characteristic hard core stubbornly defended, each with its more flexible protective belt and each with its elaborate problem-solving machinery. Each of them, at any stage of its development, has unsolved problems and undigested anomalies. (...) This programme [Einstein's] made the stunning prediction that if one measures the distance between two stars in the night, and if one measures the distance between them during day, (...), the two measurements will be different. Nobody had thought to make such an observation before Einstein's programme. Thus, in a progressive research programme, theory leads to the discovery of hitherto unknown novel facts. In a degenerating, however, theories are fabricated only in order to accommodate known facts. Has, for instance, Marxism ever predicted a stunning novel fat? Never! (Lakatos 1978, pp. 4).

Lakatos claims that research programmes evolve over time and have a generic structure. The programmes are seen as a collection of interlinked theories with a common "hard core". In fact, the hard core is the defining element of the research programme. Assumptions at the core are considered "irrefutable". They are surrounded by a flexible set of auxiliary assumptions collected in a "protective belt". These assumptions are open to debate and alterations in view of the development of the research programme. Beside these two theoretical elements of a research programme, Lakatos also argues that a research programme has methodological components in the form of rules regarding how to develop the programme, i.e. what to avoid (negative heuristic), and what to pursue (positive heuristic). When the theoretical and the methodological components are combined, it can be assessed whether a research programme is theoretically and empirically progressive or degenerating.

[41] Modus tollens is the destructive form or argument. If the conclusion of an argument is shown to be false, then at least one of the premises on which the argument is based is false.

In sum, Lakatos' elaborations on the structure of a research programme articulate four main topics or pillars: *First*, an historical component that contains clusters of interconnected theories that evolve over time. *Second*, a theoretical component consisting of rigid "hard core" principles and flexible "protective belt" hypotheses. *Third,* a methodological component that guides the theoretical and empirical development of the programme, i.e. "negative" and "positive" heuristic. And, *fourth*, an appraisal component, i.e. the assessment whether a research programme is "progressive" or "degenerating". With these four characteristics in mind, I can turn to Cochrane's version of the CCAPM and explore whether it can be characterized as a research programme in a Lakatosian sense. At the end of my assessment, which will follow in the next few sections, I conclude that Cochrane's asset pricing effort can indeed be viewed as Lakatosian research programme and, in addition, withstand its critics. Cochrane, namely, *first*, follows a research tradition with deep historical roots, *second*, protects its fundamental principles against attacks, *third*, deductively develops lower level models from these fundamental principles, and, *fourth*, applies statistical tests to gauge CCAPM's theoretical and empirical progress.

3.2 CCAPM's historical development

In the previous *section 1.1*, I already pointed out that CCAPMs microeconomic foundation lies within a neoclassical theoretical tradition. It builds upon the rational, self-interested and risk averse individual whose decision making under uncertainty takes the financial market towards a stable equilibrium situations at market clearing asset prices. Clearly, this starting point has long and complex historical roots. Early dated consumption based asset pricing efforts were led by Hirshleifer (1964, 1965, 1966), Stiglitz (1970), Rubinstein (1976) and Lucas (1978) who all, in their own ways, introduced pieces of a theoretical puzzle that were later combined in today's CCAPM. Breeden (1979), Grossmann and Shiller (1980), Shiller (1981, 1982), Hansen and Singleton (1982, 1983), Mehra and Prescott (1985), Breeden, Gibbons and Litzenberger (1989), Campbell and Cochrane (1999), Campbell (2001), Mehra (2003), Cochrane (2005) all continued to explore the microeconomic foundations. They document variations on the theme that asset prices should be studied in the context of the consumer.

Lakatos claims that distinct research programme evolves over time. In this directional process, the programme carries with it a set of theoretical clusters containing specific assumption. Without dismissing earlier contributions, I search for those clusters and assumptions and start my review with Lucas (1978), who many of his successors list as the main historical references. In a short paper named "Asset Pricing in an Exchange Economy", Lucas describes his own contribution as a:

> "…theoretical examination of the stochastic behaviour of asset prices in a one good, pure exchange economy with identical consumers. (...). An asset is a claim to all or part of the output...(...). Most of our attention will be focused on the derivation and application of a functional equation in the vector of equilibrium asset prices which is solved for price as a function of the physical state of the economy. (...). A general method of constructing equilibrium prices is developed and applied to a series of examples." (Lucas 1978, p. 1429.).

Lucas continues with an informal description of the particular economy he is interested in and then, formally, first, defines and, next, constructs the equilibrium situation in which the necessary and sufficient first order conditions takes a central role. He continues:

> "Consider an economy with a single consumer, interpreted as a representative "stand in" for a large number of identical consumers. He wishes to maximize the quantity $\mathbf{E}\ \{ \sum_{t=0} \beta^{t}\ \mathbf{U}(c_t) \}$ where c_t is a stochastic process representing consumption of a single good, β is a discount factor, **U (.)** is a current period utility function, and **E (.)** is an expectation operator. (...) ...shares are traded, (...), a competitively determined price vector p_t ... (...). ...all relevant information on the current and future physical state of the economy is summarized in the current output vector y. (...) ...consumer behavior determines the equilibrium price function. We close the system with the assumption of rational expectations:... (Lucas 1978, pp. 1430).

In these two statements, Lucas makes several notable references to the assumptions underlying the theory of asset prices in an exchange economy. I take note of the following main characteristics: consumer based micro-economic foundation, identical consumers, i.e. representative agents, rational and utility maximizing behaviour, i.e. first order condition, knowledge with respect to all relevant current and future structure/state of the economy, an equilibrium situation, and one asset. Lucas then goes on to apply this general structure and mentions in the example section of his paper, for example, that "... the price of the ***i*** th asset is the expected, discounted present value of its real dividend stream, conditioned on current information y" and "the elasticity of price with respect to income is equal to the Arrow-Pratt measure of relative risk aversion". Lucas concludes that the paper is primarily methodological, i.e. "an illustration of the use of some methods which may help to bring financial and economic theories closer together".

Following Lucas' contribution towards providing "some methods", the next seminal paper was so-authored by Hansen and Singleton (1982) called "Stochastic Consumption, Risk Aversion, and the Temporal Behavior of Asset Returns". Already in the heading of this paper we find similarities back to Lucas and forwards to Cochrane's version of the CCAPM. The paper sets out to study "the time-series behavior of asset returns and aggregate consumption". Consider the following extracts:

> "In the asset pricing models of Rubinstein (1976), Lucas (1978), Breeden (1979), and Brock (1982), amongst others, agents effect their consumption plans by trading shares of ownership of firms in a competitive stock market. (...). The purposes of this paper are to characterize explicitly the restrictions on the joint distributions of asset returns and consumption implied by a class of general equilibrium asset pricing models and to obtain maximum likelihood estimates of the parameters describing preferences and the stochastic consumption process. (...). the framework for this analysis is a production-exchange economy of identical agents who choose consumption and investment plans so as to maximize the expected value of a time-additive von Neumann-Morgenstern utility function. The joint distribution of consumption and returns is assumed to be lognormal, and preferences are assumed to exhibit constant relative risk aversion (CRRA). (...) Maximum likelihood estimates of the coefficient of relative risk aversion, the subjective discount factor, and the parameters that describe the temporal evolution of consumption are obtained using this closed-form characterization of the restrictions." (Hansen and Singleton 1983, pp 249).

From this extensive quote, it is obvious that the consumption based asset pricing theory continued to evolve around the idealized and fictitious representative investor, the character traits I have referred to earlier as the triad of temperaments, his/her optimizing behaviour with respect to utility of consumption plans, the CCRA utility function, and the equilibrium conditions. An innovation can be seen in the introduction of "the subjective discount factor". This is the same discount factor that I have referred to earlier in *Chapter Two, section 4.3* when discussing the arguments found in Cochrane's stochastic discount factor.

Finally, I want to point towards the influential paper by Breeden, Gibbons and Litzenberger (1989) on "Empirical Test of the Consumption-Oriented CAPM". This paper takes Lucas (1978) theoretical contributions to the empirical arena. It also builds upon Rubinstein (1976) and Breeden (1979). Their idea is to "demonstrate that equilibrium excess returns are proportional to their consumption betas" and also to address particular econometric or measurement problems in the consumption data. The latter, the authors point out, "lowers the variance of measured consumption growth and creates positive autocorrelations", and "underestimates the covariance between measured consumption and asset returns". I point to these particular findings when I discuss my assessment of Cochrane's CCAPM in *Chapter Five*. Consider, however, first, the theoretical set-up. It comes as no surprise to find language also used by Cochrane:

> "Since the CCAPM is well known, a standard review is unnecessary. The following synthesis provides a theoretical basis... (...). All individuals are assumed to have time-additive, monotonically increasing, and strictly concave von Neumann-Morgenstern utility functions for lifetime consumption. Identical beliefs, a fixed population with infinite life-times, a single consumption good, and capital markets that permit an unconstrained Pareto-optimal allocation of consumption are also assumed. From the first-order conditions for individual **k**'s optimal consumption and portfolio plan, it follows ... (...). This is well known (e.g., see Lucas (1978)). (...).Thus, in equilibrium in a Pareto-efficient capital market, the growth rate in the marginal utility of consumption would be identical for all individuals and equal the growth rate in the "aggregate marginal utility" of consumption. (Breeden, Gibbons, Litzenberger 1989, pp 232).

The three authors conclude that:

> "A number of tests of the consumption-oriented CAPM are examined. (...). Another implication of the CCAPM is that the market price for risk should be positive; in other words, the expected return increases as the risk increases. This implication is verified in all sub-periods, and the point

> estimate is statistically significant in most of the sub-periods. (...). Based upon the quarterly consumption data for the overall period, the linear equality between reward and risk implied by the CCAPM is rejected at the 0.05 level." (Breeden, Gibbons, Litzenberger 1989, pp 260).

In these two quotes, we learn that the empirical results are mixed and that no conclusive evidence is presented with respect to deeming the CCAPM's predictions an outright success. This finding is also echoed in Cochrane (2011) and Campbell and Cochrane (2000).

In terms of the language used in the theoretical framework, we, again, meet the consumer, his/her utility and maximizing behaviour, the equilibrium condition, and several of the other "standard" assumptions regarding assets and agent's longevity. Besides the empirical work, we also find the critical covariance between the excess return on an investment is proportional to its "covariance with respect to aggregate consumption". This idea was floated in Lucas (1978) but made more explicit in Breeden (1979) who, in one interpretation of his theoretical work, focuses on the "marginal rates of substitution between consumption today and consumption in the future". He continues:...holding the expected payoff on an asset constant, the value of asset will be negatively related to the covariance with the individual's consumption." (Breeden 1979, p. 277). Cochrane also focuses on the covariance between asset payoff and consumption. This is an important technical piece in the theory of the consumption based asset pricing because the investor is assumed to be interested in assets, i.e. recession proof stocks as Cochrane denotes them that exhibit a negative covariance with consumption. Holding such stocks helps the investor to smooth his/her consumption in bad economic states because they payoff when income from other sources, such as work, is endangered. In *Chapter Two, section 4.4*, I already pointed toward this topic.

It is thus fairly obvious to see how the research on the consumption based asset pricing theory has evolved through time. But does it contain clusters of interconnected theories as Lakatos alludes to? From the historical review, it certainly looks like this is the case. On one hand, I identified the investor, his/her character and behaviour, the financial market structure and the financial assets. Taken together, these three elements certainly form three clusters of theories that in combination interact and sum to a whole, i.e. the consumption based asset pricing theory. If, for example, the investor with his/her triad of temperaments are not incorporated within a financial market structure, he/she cannot execute on the job he/she is asked to do, i.e. make decisions with respect to current and future consumption. And without the financial market assets, the individual would not have any means to transport his/her endowment across time and different economic states.

At this stage, therefore, I conclude that the consumption based capital asset pricing effort has followed a fairly stable development around a few main characteristics of the individual, his/her character, and behaviour, the financial market structure and the financial market assets. Is there sufficient evidence that this close to 30 year effort from Lucas (1978) to Cochrane (2005) can be considered what Lakatos alludes to as a scientific research programme? To answer that question to its fullest, let us briefly recall that all elements of Lakatos research programme that I spelled out in *section 2.3* above need to be confirmed; the programme contains clusters of interconnected theories that evolve through time, it has rigid “hard core” principles and flexible “protective belt” hypotheses, it uses “negative” and “positive” heuristic to guide its development, and in its appraisal, “progressive” and “degenerating” problemshift can be identified. Given these three criteria and a fourth that assess a research programme’s progress, it is too early to draw a final conclusion concerning Cochrane’s asset pricing effort being a Lakatosian research programme.

Now, since I have concluded that the CCAPM research effort has evolved consistently over a period of time, I move to the second criteria and ask the question whether Cochrane asset pricing theory entertains a hard core and a protective belt.

3.3 Cochrane’s hard core and protective belt

In *sections 2.1.1 and 2.1.2* above, I arranged Cochrane’s assumptions into two groups; the first group contains what I call fundamental principles, and the second contains what I call auxiliary assumptions. The question I pursue here is whether these two groups are similar to what Lakatos calls hard core propositions and protective belt hypotheses. If they are, Cochrane has taken a second hurdle on the road towards being characterized as a Lakatosian research programme.

How does Lakatos characterize the programmes theoretical component, i.e. the rigid "hard core" principles and flexible "protective belt" hypotheses? Here is one example:

> "Newtonian science, for instance, is not simply a set of four conjectures – the three laws of mechanics and the law of gravitation. These four laws constitute only the "hard core" of the Newtonian programme. But this hard core is tenaciously protected from refutation by a vast "protective belt" of auxiliary hypotheses." (Lakatos 1978, p.4).

Here, Lakatos points towards the hard core of Niels Bohr's research programme of light emission that is characterized by:

> "...five postulates as the *hard core* of his programme: (1) that energy radiation [within the atom] is not emitted (or absorbed) in the continuous way..(...). (2) That the dynamical equilibrium of the systems in the stationary states is governed by the ordinary laws of mechanics...(...). (3) That the radiation emitted during the transmission of a system between two stationary states is homogeneous... (...). (4) That the different stationary states of a simple system consisting of an electron rotating round a positive nucleus are determined by the conditions that...(...). (5) That the "permanent" state of any atomic system (...) is determined by the condition that..." (Lakatos 1978, p. 56).

We here read about the "rational reconstruction", as Lakatos describes it, of a research programme's hard core. Such a core is thus the most important defining element of any research programme. While the programme as a whole may change through time, its hard core remains the same throughout the sequence of the theory development. Nonetheless, Bohr's hard core was soon proven wrong: "Wave mechanics soon caught up with, vanquished and replaced the Bohr programme." (Lakatos 1978, p. 68).

Blaug (1992) also puts emphasis on the importance of the hard core proposition. They are "rigid" and "irrefutable beliefs", go unquestioned, and remain highly respected by all members of the research programme's community. In the context of asset pricing, this community can be seen as financial economists working with the tradition of the consumption based version as shown in the previous section. By definition, this should exclude, for example, the relative or factor-based pricing programme of Fama and French (see *Chapter Two, section 1.2.3*). However, this might be too quick a conclusion. Later in this section, I assess whether this exclusion is warranted or not.

I suggest that Cochrane's simplified, idealized and constructed fundamental principles, as reviewed in *section 2.1.1* above, can be viewed as a set of Lakatos' hard core (HC) propositions. There are three:

HC 1: A financial market is populated by a rational, self-interested and risk averse representative investor.
HC 2: The representative investor maximizes his/her total expected utility with respect to present and future consumption using financial assets as inter-temporal "transporters".
HC 3: Financial markets are in equilibrium when all assets are held by the representative investor at the desired price.

I claim that these three hard core propositions capture the essence of the consumption based capital asset pricing theory as seen developed from Lucas (1978) to Cochrane (2005). Their common goals are to examine, either theoretically or empirically, the stochastic behaviour of asset prices. Listed in this way, these fundamental principles have proven resilient, i.e. "rigid" and "irrefutable". Let me briefly review them one after the other.

The HC 1 proposition claims the existence of a financial market and an inhabitant. The historical review in the previous section always incorporated a financial market. In some cases, however, it was denoted a "capital market", the "physical state of the economy" or the "production-exchange economy". The company inside this economy issues stocks or produces a good in a "competitive" environment of "trading". In all reviewed historical references, there is only a real economy, i.e. central bank money is not introduced. The inhabitant is an individual who "stands in" for a homogenous, i.e. "identical" population. He/she is at the same time a "consumer" and an "investor". He/she is endowed with "rational expectations", is self-interested, and "risk averse" according to von "Neumann and Morgenstern's axioms". Financial economists working within the HC1 proposition, I find, view them as rigid and irrefutable. They are, what Lakatos would say, the hard core of the consumption based asset pricing theory.

I reach the same conclusion with respect to *HC 2*. Cochrane's fundamental principles concerning investor's behaviour, i.e. utility maximization with respect to his/her present and expected future consumption are considered rigid and irrefutable by its practitioners. We recall from the previous section the considerations of the practicing individual who, with specific "preferences", "maximizes the expected value" of some utility function. Again, Cochrane's integration of utility maximization into his theoretical work helps describe the commitment to a historical tradition that makes his core "rigid".

Finally, *HC 3* is an uncontroversial proposition. If the agent is rational, self-interested and risk averse, seeking maximum level of utility from consumption over a particular time span, there will be an equilibrium situation in the financial markets in which an asset transports consumption and utility.

As a conclusion, Cochrane's fundamental principles, as developed in *section 2.1.1* above, are all part of a theoretical tradition of a specific research programme denoted the consumption based capital asset pricing theory. All in all, HC 1 through 3 spell out a narrow set of rigid and irrefutable propositions that define the ground rules for doing "consumption based asset pricing theory".

I now turn to the second set of propositions that Lakatos suggests, i.e. those belonging to the protective belt (PB). In addition to the hard core, this belt is needed:

> "The protective belt contains the flexible parts of a scientific research programme, and it is here that the hard core is combined with auxiliary assumptions to form the specific testable theories with which the scientific research programme earns its scientific reputation." (Blaug 1980, p. 34).

Lakatos gives the following, short, description of the purpose of the protective belt: "It is the protective belt of auxiliary hypotheses which has to bear the brunt of tests and get adjusted and re-adjusted, or even completely replaced, to defend the thus-hardened core." (Lakatos 1978, p.48).

Based on my review of the historical development of the consumption based asset pricing tradition in the previous *section 2.2.1*, and without any claim to completeness, I suggest a list of possible propositions that can be viewed as auxiliary assumptions carried in the belt. My question here is whether these PB propositions can be likened with those I identified as Cochrane's auxiliary assumptions in *section 2.1.2* above. The short answer is "yes":

PB 1: The representative investor knows the payoff of all future investments.
PB 2: The representative investor knows all future states of the economy.
PB 3: No taxes are paid on capital gains and dividends, i.e. payoffs.
PB 4: Transactions in financial markets are costless.
PB 5: There exists a unique security for all possible future payoffs.
PB 6: The representative investors never die – he/she lives forever.

Protective belt propositions are according to Blaug neither rigid nor irrefutable. On the contrary, the propositions in the belt can, and must be, addressed and adjusted in light of what Lakatos refers to as "anomalies", i.e. when a theory is contradicted by the facts. [42]If the predictions of the asset pricing model, for example, fail to resemble the actual data, such auxiliary assumptions are the theory's first line of defence. They are there to protect the hard core. How financial economists in the consumption based asset pricing tradition, and Cochrane in particular, work with the flexibility of the protective belt is a main subject in *Chapter Five* where I assess its empirical results.

[42] One such anomaly is the equity risk premium puzzle discussed in *Chapter Two, section 6.1*

At this stage of my discussion, it suffices to say that Cochrane's auxiliary assumptions can be seen as Lakatosian protective belt hypotheses. They have provided the consumption based tradition with the necessary flexibility to address unsuccessful statistical tests. Hence, I claim that the consumption based asset pricing tradition has taken yet another hurdle to be considered a Lakatosian research programme. But more needs to be considered.

3.3.1 Combining the hard core and the protective belt

In combination, Cochrane's fundamental, hard core, principles and auxiliary, protective belt, assumptions are well anchored within the consumption based asset pricing research programme. If it can be established that this programme is different from the relative, i.e. factor-based asset pricing effort, we can gather more evidence that the CCAPM is a separate and unique programme (see *Chapter One, section 1.4* on the differences between the two). Below, therefore, I intend to show that Cochrane's absolute pricing programme is decisively different from the relative programme Lakatos would refer to them as "rival methodologies". Let me, therefore, briefly review the main reasons for my claim.

Earlier, I divided the asset pricing research effort into two different camps. I used Eugene F. Fama and Kenneth R. French's multi-factor model as an example in the first group while the consumption based capital asset pricing model exemplified the second. Cochrane referred to these categories as the relative and the absolute tradition. He sees them as: "...two polar approaches to this [*asset pricing*] elaboration." (Cochrane 2005, p. xiv). He goes on to tell us that the relative approach is less "ambitious" than the absolute, but also that: "Asset pricing problems are solved by judiciously choosing how much absolute and how much relative pricing one will do, depending on the assets in question and the purpose of the calculation. Almost no problems are solved by the pure extremes." (Cochrane 2005, p. xiv). These extracts tell us that the two versions have the same goal, i.e. explain and predict observable market prices of financial assets, but that they are in a "friendly competition". In this respect, Cochrane wants us to believe that they belong together under the same umbrella. They are merely two different versions of a larger scientific project. I disagree.

First, consider CCAPM's micro-economic starting point in the representative investor, his/her character and behaviour. Fama and French implicitly argue that it is not necessary to introduce such a character when modelling asset prices. They work at the level of statistical time series analysis. When analysing the pricing data set, however, Fama and French make an important assumption. They assume that the data are generated by market participants who all specialize in using all available public information to evaluate the current stock prices of, for example, Google and Caterpillar. Market participants are thus assumed to react immediately to any new and relevant pieces of information by buying the stock on positive news and selling it on negative. Stock prices, therefore, at any specific point in time, can be seen as reflecting this rational information evaluating activity. The point can, therefore, be made that Fama and French use rationality to describe the data generating process. These activities make stock markets "efficient" (Fama 1970).

Second, consider Cochrane's references to the importance of consumption. Although assuming rational market participants, Fama and French do not connect the assumed rationality in prices with the rationality of an investor in their theory because they do not need to do so. Hence, the two authors are not describing rationality in the same fundamental way that Cochrane does with references to, for example, von Neumann and Morgenstern's axioms. A similar outcome can be seen with respect to self-interest and risk aversion.

Third, in the absence of a representative investor and the assumed triad of temperaments inside the factor-based theory, Fama and French have no need to establish a first order condition in utility maximizing individuals with their inter-temporal consumption now and later choice that take financial markets towards an equilibrium situation. A discussion on the purpose of investment income as a result of not spending all the current endowment is, of course, also absent in Fama and French.

All the three counts lead me to conclude that the fundamental principles in Cochrane's research effort and its historical tradition are not shared by Fama and French. As a consequence, Cochrane's absolute and Fama and French's relative approaches are different. Lakatos' programme terminology, therefore, helps draw a dividing line between the relative and the absolute research programmes This again makes clearer the implications when switching between the two research programmes, as Cochrane suggest we could. Anyone following his advice should, therefore, bear in mind that he/she is also exchanging one set of fundamental principles for a time series analysis based upon a notion of "efficient markets". The Lakatos based analysis has consequences also for Cochrane's ambition. He/she claims to have "*the* theory of asset pricing". Furthermore, that all other asset pricing efforts can be subsumed under his: "All factor models are derived as specializations of the consumption based model." (Cochrane 2005, p. 151). Given my discussion in this section, I believe these statements cannot be defended.

3.3.2 Interlinked theories

A remaining question is whether Cochrane's theory can be characterized as a collection of interlinked theories as Lakatos suggests a research programme is. Given the fundamental, hard core, principles and the auxiliary belt assumption and their relationships, I argue below that Lakatos requirement towards the research programme containing interconnected theories is fulfilled. Hence, the first hurdle I alluded to earlier is definitively met.

Interlinked theories connect various elements and derive specific consequences. In the context of the CCAPM, the three fundamental principles are first established. Then they are connected with the auxiliary assumptions. Finally they are set-off, as a complete programme, to interact in a closed system, i.e. they interact with each other and not with any other, excluded elements. In *section 3.1* above, I showed how various authors in the tradition of the consumption based asset pricing theory explain the starting point and the consequences of this set up. But are these connections strong enough to establish an interconnected whole?

First, consider the connection between Cochrane's triad of temperaments and his/her optimizing behaviour, i.e. the combination of HC 1 and HC 2 that lead to HC 3 as referred to above. In this framework, all hard core propositions are active at the same time, and their relative forces add up to support the outcome. It is thus the specific combination that is exploited by the consumption based theory for the purpose of building an internally consistent outcome.

Second, the triad of sentiments need the other HC principles and PB assumptions to assemble a full theory of asset prices. If we remove, for example, the specific financial market context and focus on the representative investor's character traits only, we find that they are not exclusively held by the investor. In fact, there are good reasons to believe that these sentiments are active in most of us most of the time, i.e. in situations that have little to do with financial markets – for example when we are asked to make decisions with uncertain outcomes. We also recall that von Neumann and Morgenstern initially suggested that these traits are introduced in a theory of gambling. The fundamental principles that Cochrane applies are, therefore, not exclusively "made" for financial market situations. They can even be considered to be "borrowed" from somewhere outside this context - possibly from psychology. Yet again, establishing a relationship between the three fundamental core principles does not give us an asset pricing theory. Cochrane needs more. Let us next consider the auxiliary, belt, assumptions.

Third, for Cochrane to develop a theory of asset pricing, he needs more than the fundamental principles. In other words, they need to be tied down for use in the "Political Economy", i.e. the financial market. It is precisely here that the protective belt assumptions come into play, i.e. PB 1 to 6. Without these assumptions, Cochrane does not have a consumption based capital asset pricing theory. The belt assumptions do not seem to be related to each other in any systematic way. For example, knowing the payoff of all future investments is not in any way connected with the assumptions that no taxes are paid on that payoff or that transactions are costless. The investor's infinite lifespan is also unrelated to the existence of a unique security for all possible future payoffs.

However, these belt assumptions enrich the HC principles and enable them to form a logical whole.

Fourth, the connectedness has several specific attributes. The PB assumptions such as complete markets, Arrow Securities and near to perfect knowledge are elements that do not “fall out” from the other PB assumptions or the HC principles. Like their HC counterparts, they are also “borrowed’ from somewhere else and added to create a financial market structure and particular financial assets. The point I want to make is that most of these initial conditions were not originally linked to each other or with the fundamental principles. These seemingly unconnected elements have thus been put together in a web of propositions that make up the consumption based theory of asset prices.

In sum, the strategy of adding auxiliary PB assumptions to fundamental HC principles is considered uncontroversial and may be regarded as good scientific practices when assembling a theoretical framework. The CCAPM tradition successfully connects a theory of man, his character and behaviour with a financial markets structure and establishes financial asset to transport consumption and utility. From there, CCAPM explores the consequences of very specific derivations towards a set of narrow analytical cases. I think we can agree that Cochrane’s asset pricing theory is a collection of more or less interconnected theories. Every HC and PB proposition can even be viewed as a separate theoretical proposition. When synthesized, they gel to become a theory.

3.3.3 Deductions from the a priori

In this section, I review another relevant aspect of the consumption based asset pricing research effort. It is related to the methodology used by the various authors in its development over time, i.e. how fundamental, hard core, principles and auxiliary, protective belt, assumptions are applied to “do science”. I suggest that a common denominator can be found throughout this development in how the principles are used as starting points from which theoretical consequences are deduced and compared with the real situations. This “theory heavy” deductive strategy is different from the “theory light” inductive strategy followed by, for example, Fama and French's relative, or factor-based research on asset prices and their behaviour which, again, confirms my claim that the CCAPM is a distinct research effort.

I argue my views on CCAPMs methodological case by drawing on John Stuart Mill, who I introduced in *section 1.1.1*, and what he calls the "a-priori" method. Here is a well-rehearsed starting point:

> "Political Economy considers mankind as occupied solely in acquiring and consuming wealth; and aims at showing what is the course of action into which mankind, living in a state of society, would be impelled, if that motive, except in the degree in which it is checked by the two perpetual counter-motives above adverted to, were absolute ruler of all their actions. (...) Not that any political economist was ever so absurd as to suppose that mankind are really thus constituted, but because this is the mode in which science must necessarily proceed. When an effect depends upon a concurrence of causes, those causes must be studied one at a time, and their laws separately investigated, if we wish, through the causes, to obtain the power of either predicting or controlling the effects; since the law of the effects is compounded of the laws of all the causes which determine it." (Mill 1874, Essay 5, v.38).

This is a rich statement. Daniel M. Hausman, in his effort to "resuscitate" Mill's a-priori method, presents the following schemata:

> "1. *Borrow* proven (*ceteris paribus*) laws concerning the operation of relevant causal factors.
> 2. *Deduce* from these laws and statements of initial conditions, simplifications, etc., predictions concerning relevant phenomena.
> 3. *Test* the predictions.
> 4. If the predictions are correct, then regard the whole amalgam as confirmed. If the predictions are not correct, then *judge* (a) whether there is a mistake in the deduction, (b) what sort of interferences occurred, (c) how central the borrowed laws are (how major the causal factors they identified are, and whether the set of borrowed laws should be expanded or contracted)." (Hausman 1992, p. 147)

Hausman's suggested *second* proposition is concerned with deducing predictions from the *first*, i.e. the "borrowed" and "proven" laws", other "initial conditions" and "simplifications". This delivers the foundation for proposition *three*, i.e. tests and, *four*, i.e. corrections – if necessary. Let me dwell upon this set-up in the next few paragraphs.

Mill's elaborations and Hausman's explanations should strike a familiar cord with the reader of my thesis. There is little doubt that the consumption based asset pricing theory proceeds along this methodological structure. Despite the fact that economists in general are vague in the definition of what "causes" and "laws" are, it does not seem far-fetched in the context of my interpretation of CCAPM to classify the investor's triad of sentiments as "causes" and his/her drive towards a first order condition with respect to inter-temporal consumption as a "law". [43] As in Mill, CCAPM practitioners derive consequences from such causes and laws. The consequences, also referred to as predictions, are then compared with respect to their accuracy relative to the real situations. However, this is a peculiar test. Hausman argues: "...verification is essential, but not in order to test the basic laws; they are already established and could hardly be cast in doubt by the empirical vicissitudes of a deduction from a partial set of causes." (Hausman 1992, p. 146). Blaug expands:

> "Over and over again, in Senior, in Mill, in Cairnes, and even in Jevons, we have found the notion that "verification" is not a testing of economic theories to see whether they are true or false, but only a method of establishing the boundaries of application of theories deemed to be obviously true: one verifies in order to discover whether "disturbing causes" account for the discrepancies between stubborn facts and theoretical valid reasons: if they do, theory has been wrongly applied but the theory itself is still true." (Blaug 1992, p 71.).

I think the two interpretations of Mill are broadly in line. The deduced consequences in Mill are logical extensions from a given starting point established, inductively, from observation and introspection. Mill also calls this activity "abstract speculations". Mill

[43] Economists often refer to the "law of demand and supply", "law of one price", "law of diminishing returns", "Okun's law", "Gresham's law", etc. In most cases, they think about them as empirical generalizations. It can be observed, for example, that, most of the time across most of the provided goods and services, demand increases when their prices fall. But this is not always so. And it is certainly not so for all products and services - or financial assets for that matter. Neither do the "law" tell us how much the demand changes when prices fall or - rise. A similar relaxed "law" related attitude amongst economists can be found with respect to their use of "causes". In most cases, the word is applied interchangeable with terms such as "establish", "explain", "determine", "drive", "depends upon", "influence", etc.

himself says that not too much should be expected with respect to the quality of the predictions. In fact, he advices economists to lower their expectations because this set-up is, generally, "insufficient for predictions" but "most valuable for guidance" (Mill 1882, 6.9.2). Comparing model-based statements with the real situation data is, therefore, not about ex ante prediction but ex post verification of an a priori starting point. A successful extension towards the real situations thus confirms the starting point and the logic of the deductions. If unsuccessful, there are, in Mill's world, only one source of errors - "disturbing causes". Given Mill's emphasis on the disturbing causes, the advice is always to look in that space:

> "In this way a nearer approximation is obtained than would otherwise be practicable, to the real order of human affairs in those departments. This approximation is then to be corrected by making proper allowance for the effects of any impulses of a different description, which can be shown to interfere with the result in any particular case. (...). The discrepancy between our anticipations and the actual fact is often the only circumstance which would have drawn our attention to some important disturbing cause which we had overlooked." (Mill 1874, Essay 5, v.38).

In *section 3.1* above, I showed the historical development of the consumption based asset pricing research effort. One of several relevant common threads is the investor and his/her behaviour that leads to the first order condition. Consider an important element as described by Cochrane – also noted in *Chapter Two, section 4.4*:

> "If you buy an asset whose payoff covaries positively with consumption, one that pays off well when you are already feeling wealthy and pays off badly when you are already feeling poor, the asset will make your consumption stream more volatile. You will require a low price to induce you to buy such an asset. If you buy an asset whose payoff covaries negatively with consumption, it helps to smooth consumption and is more valuable than its expected payoff might indicate." (Cochrane 2005, p. 13).

Cochrane captures this *story* in mathematical equations, and when the model-based claims turn out to be inaccurate, they should be corrected as we saw in the case of Breeden (1979) and Breeden, Gibbons and Litzenberger (1989). The attractive negative co-variance set-up is an idealization within a separate realm i.e. it isolates a theoretical case from a fuller and more complex reality. The example includes carefully chosen properties of the human character, market structure and financial assets that are considered to be important for the theoretical analysis. The theory, therefore, only describe the effects of selected causal influences. In a real setting, there is a set of unnamed "disturbing causes" that are active but ignored and excluded from the theory. Adding them in, as Cochrane suggests, might explain away and overcome the differences between the theoretical cases and the empirical situations that are evident in the lack of statistical successes.

Let me now turn to Lakatos and review the similarities between Mill's deductive a priori method and CCAPM practitioner's use thereof in the framework of a research programme. I find that the use of Mill's method is a shared tradition among consumption based asset pricing promoters. Hence, it manifests them as practitioners within a specific research programme.

First, Lakatos sees a research programme containing hard core propositions. I believe these propositions are mirrored in the "causes" and "laws" that Mill alludes to despite me not finding any specific references to these concepts in the historical review of the consumption based asset pricing research effort. Nonetheless, I think Cochrane, and most of his predated peers, view their agents, their character and behaviour as main factors, or causes that influence the pricing of assets. Likewise, the agent's actions and connections with the financial market structure and assets can be regarded as governed by "laws". It might be that these causes and laws are borrowed from outside the realm of economic, for example from psychology and sociology as Mill refers to, but in the context of the Lakatosian review here, I do not need to follow up on that particular point.

Second, while Mill saw the deduced consequences flowing "solely from the desire of wealth", consumption based asset pricing practitioners follow the tradition that their agent is driven by the desire to consume. However, the desire to smooth consumption over time and across different economic states, as Cochrane refers to above, but also in my *Chapter Two, section 3.3*, is an innovation to the *story* that his predecessors told.

Nevertheless, Lakatos identified deductions from a few hard core principles as a defining element of a research programme. Earlier, I referred to the review of Bohr's light emission programme containing five postulates that constituted its hard core. Lakatos used them to show how Bohr deduces their implications. The consumption based asset price followers use the same technique. In this sense, the consumption based agents are well behaved in the sense that he is always going to do what he is asked to do, i.e. optimize the discounted utility of current and future expected consumption. He will never disappoint, and the model outcome is from this point of view rather trivial. The CCAPM tradition thus guarantees internal consistency. It goes without saying that both in the case of Bohr and Cochrane, specific conditions or auxiliary assumptions were needed to further specify in which environment, for example, the deductions take place.

Third, research programmes, Lakatos maintains, use methodological negatives, i.e. "do not" and positives, i.e. "do" rules, i.e. heuristic when encountering anomalies (see *section 3.3* below). So what do Cochrane and Mill suggests doing when the empirical tests turn out to be inaccurate? Again there is a shared remedy. First, acknowledge that the causes captured in the theory were insufficient for explaining the real situation. Next, analyse the reasons for this failure. Then, look for "disturbing causes" that operate in those situations but had been overlooked or deliberately omitted at the starting point. Finally, test again. I will not expand on this aspect here as it is a main concern in *Chapter Five*. The point I want to make, however, is that with respect to the inaccurate claims, Cochrane and Mill are not far apart from what Lakatos finds in a research programme.

Fourth, neither Mill nor the most practitioners in the CCAPM tradition develop theories for their own sake. They share the methodological commitment towards testing their theories against the real situations. This is also, Lakatos explains, an important part of any research programme. It lays the foundation for exploring whether the programme is progressive or degenerating. Apart from the fact that Mill uses logic and the CCAPM practitioners use mathematics when deducing the consequences of their fundamental, hard core, principles, Mill is, as I pointed out above, more cautious with respect to what can be expected from the a-priori method. He is also explicit about how the results should be used, i.e. not as a challenge to the fundamental principles but for "inquiries" inside the field of Political Economy. Cochrane is not as explicit as Mill on this. Nonetheless, both seem to agree with Lakatos' claim that research programmes do not challenge the hard core.

I conclude from the analysis of Mill and the consumer based asset pricing followers, using Cochrane as a main advocate that they follow practices and processes that are similar to those Lakatos claims are present in a research programme. Also on this methodological count, I can claim that the CCAPM research effort fulfils the criteria of a Lakatosian research programme.

3.4 Cochrane's positive and negative heuristic

Having discussed the historical development, as well as the hard core and protective belt considerations of Lakatos' research programme, I now turn to the third aspect thereof, i.e. the methodological component. Now, I illustrate CCAPMs development in view of the "positive" and "negative" heuristic that Lakatos uses to illuminate such evolutions. Lakatos views heuristic as "rules of discovery" that guide problem solving as to what should be pursued, i.e. positive heuristic and what should be avoided, i.e. negative heuristic when developing scientific research programmes:

> "The research policy, or order of research, is set out – in more or less detail – in the positive heuristic of the research programme. The negative heuristic specifies the "hard core" of the programme which is "irrefutable" by the methodological decisions of its proponents; the positive heuristic consists of a partially articulated set of suggestions or hints on how to change, develop the "refutable variants" of the research programme, how to modify, sophisticate, the "refutable" protective belt." (Lakatos 1978, p.50).

Lakatos continues stating that the heuristic is:

> "...a powerful problem-solving machinery, which with the help of sophisticated mathematical techniques, digests anomalies and even turns them into positive results. (...) For instance, if a

> planet does not move exactly as it should, the Newtonian scientists check his conjectures concerning atmospheric refraction, concerning propagation of light in magnetic storms, and hundreds of other conjectures which are all part of the programme. He may even invent a hitherto unknown planet and calculate its position, mass and velocity in order to explain the anomaly." (Lakatos 1978 p. 4).

Lakatos, here, and even elsewhere, is not overly explicit as to what this "problem solving machinery" precisely is. Hence, his advice with respect to how scientists should proceed to address problems in order to protect and grow a particular body of knowledge is not highly specific. But this was not his aim anyway. Nonetheless, let me, informed by Hoover (1991), dwell on how this "machinery" of negative heuristic (NH) and positive heuristic (PH) can be formulated. Consider, therefore, my following suggestions from the consumption based asset pricing theory, which I list, again, without any claim of completeness. Let me start with the negative heuristic along the following lines:

NH 1: Do not violate any hard core propositions.
NH 2: Avoid irrational, selfish, risk taking and non-optimizing individuals.
NH 3: Avoid mortal and uninformed investors.
NH 4: Avoid disequilibrium situations.
NH 5: Avoid more than one asset.

Likewise, I suggest the following positive heuristic along the lines of:

PH 1: Simplify, idealize and construct consumption based asset pricing theories.
PH 2: Isolate a specific area of theory operation and call it the financial market.
PH 3: Use a representative agent with a triad of temperaments.
PH 4: Establish first order condition of optimizing intertemporal consumption and utility.
PH 5: Use state of the art mathematics.
PH 6: Test the ability of the model to explain and predict observable asset prices.

Formulated in this way, the CCAPMs development is methodologically protective regarding the fundamental principles related to the individual, his/her character and behaviour as well as the embedded equilibrium condition. These elements are well anchored in the paradigms found in neoclassical economics. So far, Cochrane has not diverted from this paradigm as changes to it would bring him on a collision course with the hard core propositions of his own research programme. The hypotheses in the protective belt, by contrast, are in flux. Existing ones have been altered or removed, and new were added. For example, Lucas (1978) introduces a two period time-horizon while Cochrane (2005) works with an infinite time horizon. Also, Lucas (1978) was a theoretical work while Cochrane (2005) is both theoretical and empirical. This makes Cochrane consider topics that were not a concern in Lucas. Hence, the heavy lifting with respect to programme development can be found in the belt. Nonetheless, it is difficult to say whether the programme developers have deliberately worked towards protecting the core or just using the greater flexibility of the belt.

The rules of programme protection at the core, i.e. negative heuristics and the rules to modify the belt, i.e. positive heuristics might be considered fairly loose. I could have added in several other of the characteristics that I showed in the quotations from the proponent of the consumer based asset pricing theory. For example, I did not mention specific utility functions, variations on subjective discount functions and risk aversion, information distributions, taxes, as well as time series of consumption data and their measurement frequency, and possibly more importantly, the theory's connection to the rest of the economy, incorporation of business cycle shocks, the monetary sector, and public sector regulators. This multitude of related but not included topics in my, admittedly, incomplete listing of NHs and PHs certainly affect asset prices and their behaviour in real situations. But at this stage of the theory development, the practitioners consider many of the items to be outside both the fundamental core principles and the auxiliary belt assumptions. They are all parked as Maeki stated earlier in *section 1.3.2*, in the Y set. My point here is that Lakatos "powerful problem solving machinery" is available and on duty, but, in the context of the current development of the consumption based asset pricing programme, not entirely called upon to act (see *Chapter Two, section 6.5* on recent development of the programme).

At this stage of my discussion, we can conclude that the CCAPM can take the third hurdle with respect to branding it a Lakatosian research programme. Using the consumption based pricing theory, it is possible to establish a methodological practice that is used by programme proponents to both defend and develop it.

3.5 Progressive of degenerating?

Finally, I briefly review how Lakatos suggests how a research programme is assessed and, thereafter, draw some preliminary conclusions with respect to the CCAPM and its development under the control of Cochrane. I have more to say regarding theory assessment in the final *Chapter Five* of my thesis. I let Lakatos introduce the topic and draw on his discussion of the Bohr's research programme which is, as Lakatos claims, "based on the idea that light-emission is due to electrons jumping from one orbit to another within an atom":

> "The programme lagged behind the discovery of "facts". Undigested anomalies swamped the field. With ever more sterile inconsistencies and even more *ad hoc* hypotheses, the degenerating phase of the research programme had set in. (...) A rival research programme soon appeared: wave mechanics. Not only did the new programme, even in its first version (de Broglie, 1924), explain Planck's and Bohr's quantum conditions; it also led to an exciting new fact, to the Davisson-Germer experiment. In its later even more sophisticated versions, it offered solutions to problems which had been completely out of the reach of Bohr's research programme, and explained the *ad hoc* later theories of Bohr's programme by theories satisfying high methodological standards." (Lakatos 1978, p. 68).

In the above extract, we hear about a research programmes' "degeneration" and its abolishment. In contrast to this development, Lakatos also introduce the term "progressive". In both cases, a programme is evaluated by both its theoretical and its empirical degeneration or progression. It is relevant in this context that it is not a single fundamental, hard core, principle or any single individual auxiliary, protective belt, assumption that is evaluated but the programme as a whole.

For a research programme to be theoretically progressive, its positive heuristic incorporates the hard core principles from the previous version of the programme and develops new protective belt assumptions that account for the novel facts. Empirically, the newest version of the programme must account for all the facts in the legacy programme, and in addition, can generate predictions in new areas of the empirical situations. When these new implications are confirmed by real situation observations, and statistical success is demonstrated, the programme is empirically progressive, i.e. it has predicted “some novel, hitherto unexpected fact”. Degenerating programmes, as was exemplified above in the case of Bohr, do not portray these desirable properties. They are as Lakatos says “fabricated” only in order to accommodate new facts. Elsewhere, he alludes to “ad hoc“ manoeuvring when the functional form of already rejected equations are altered to better fit the facts – as was demonstrated in Bohr’s mathematical structure (Lakatos 1978, p. 67).

It can certainly be said that the way Lakatos connects methodology with historical developments is in itself interesting and debatable. It can be argued, for example, that Lakatos’ review of research programmes leads to requirements that themselves are both simplifications and idealizations of how practitioners proceed. Additionally, Lakatos does not effectively address the role of inductive evidence for a particular model-based claim. The inductive evidence is important for the factor-based modelling of Fama and French, but has no room, it seems, in Cochrane’s efforts. His approach, as we have seen, is deductive from a few hard core principles. The implications of these evaluation criteria, however, for the CCAPM research programme are profound. They help support the practitioners within the research programme to uphold their work when facing, what Lakatos refers to as “anomalies”, i.e. puzzles, inconsistencies or conflicting facts.

The role of the positive and negative heuristic now becomes clearer. Given the history of the consumption based asset pricing model and its theoretical underpinning, a practitioner might believe that it entertains an accurate hard core even if the programme fails to provide statistically successful results. Nonetheless, there must be clear justifications for such a belief in the core, for example, through past explanatory and predictive accuracy.

In order to protect the fundamental, hard core, principles, the programme follower would, therefore, turn to the positive heuristic and make adjustments to the auxiliary, protective belt, assumptions in the belief that they would save the day – as had been evidenced by previous experiences. Nonetheless, should the practitioner face an accumulation of such anomalies, and the positive heuristic is unable to account for them, the programme might, after all, be abandoned. This also raises the important point that protective belt developments do not guarantee that a troubled research programme can be saved. This again challenges the perceived protected status of the hard core propositions.

It goes without saying that the CCAPM research programme has met several anomalies in the course of its confrontations with data extracted from observable asset prices. This is a known fact to the practitioners within the consumption based pricing tradition. As I showed earlier in the literature review, statistical success is hard to pin down. Cochrane acknowledges this (see *Chapter One, section 1.5*):

> "Unfortunately, this specification of the consumption based model does not work very well." [*this motivates the*] "...exploration of different utility functions, general equilibrium models, and linear factor models such as CAPM, APT and ICAPM as ways to circumvent the empirical difficulties of the consumption based model." (Cochrane 2005, p. 43).

The natural question, therefore, is why the consumption based pricing theory has survived these confrontations. An even more concrete question is how the practitioners have applied the negative and positive heuristic in this quest for making improvements to their programme. I have already in *Chapter Two, section 6.3.4* pointed towards one recent development within the programme, i.e. that of "habit-persistence" which could promise theoretical and empirical progress. Again, I hold back on that discussion for now, and refer impatient readers to *Chapter Five, sections 2 and 5*.

Furthermore, the CCAPM research effort is challenged by Lakatos' suggestion that a research programme develops "novel unexpected facts". In this respect, Lakatos explicitly mentions a few examples from Newton's natural sciences (Lakatos 1968-1969, pp. 169). In finance, this requirement is challenging to meet. *First*, it is somewhat unclear what a “novelty” is. Clearly, the equity risk premium, for example, was not “discovered” following CCAPM’s theoretical development. It was rather the outcome of a simple review of asset return data. One could, of course, argue that the habit-persistence asset pricing model was an innovation compared with its predecessor, but this does not qualify when Lakatos’ criteria is to detect so far hidden facts.

Nonetheless, despite some interesting “re-engineering” performed by Cochrane on his version of the CCAPM, it still looks as if the programme is facing persistent challenges that it has yet to solve. These challenges are not emitted by the advocates of the relative, factor-based school. Hence, there is little pressure from within the “asset pricing industry” for the consumption based practitioners to change and adopt. However, it is in its own interest to either continue their work in the protective belt area or start chipping away from the hard core. The latter, we know by now, would seriously challenge the programme at its foundation. However, early Lakatos still leaves the door open for such fundamental surgery: “The actual hard core of a programme does not actually emerge fully armed like Athene from the head of Zeus. It develops slowly, by a long, preliminary process of trial and error.” (Lakatos 1970, p, 133, fn 4).

3.6 Lakatos’ critics

Having motivated my choice of reviewing Cochrane’s CCAPM version from a Lakatosian point of view, I cannot ignore the fact that Lakatos has met criticism on several elements of his description of what characterizes a research programme. Below, I will demonstrate that this criticism can be withstood. Hence, I can feel confident that my choice of using Lakatos as a basis for analysing the CCAPM is well founded.

Maeki (2008) summarizes this criticism well and claims that, within economics, (1) hard core principles are not easily identifiable, (2) predictions of novel facts are not actively generated, (3) social institutions and the history in the development of research programmes are not systematically addressed, and (4) connections between novel predictions and truths about the real situation are not systematically account for. My question here is whether CCAPM can withstand such criticism because if it can, using Lakatos to characterize Cochrane's research effort is valid.

On the *first* point, I think the identification of hard-core principles in financial economics is in many ways a fairly straight forward process. Economics, for example, distinguishes between macro-models that have a microeconomic foundation and those that do not have. Dynamic Stochastic Equilibrium Models (DSGM), for example, uses individual consumers and firm as their foundation (see *Section 1.1.2*). Such micro-based models are distinguishable from vector auto regressive (VAR) time-series models. They merely apply statistical analysis to explain and predict macro-economic variables. A similar distinction can be drawn between macro- and factor-based asset pricing models and theories. The former is akin to the DSGM approach while the latter is similar to the VAR approach.

The distinction between the two schools of asset pricing thought is further detailed when looking at CCAPM's theoretical centre. I have and will continue to draw the reader's attention towards the triad of temperaments that underlies all consumption based capital asset pricing models. Rationality, self-interest and risk aversion have been, as I demonstrated in *section 3.2*, at its centre since Lucas (1978). Hence, I argue that a hard core of fundamental principles exist in the consumption based capital asset pricing research and that it is easily identifiable.

The *second* point refers to Lakatosian requirement that progressive theories must have "excess content" over its predecessor and make "novel predictions". The latter term is central because it means that such theories must be able to predict phenomena not yet known or phenomena that had not been considered when the theory was developed. These requirements certainly raise the bar for financial economists. A novel fact, it can be argued, is hard to come by in financial economics. In economics and financial economics as well, it is often the observation of financial market facts that motivates the development of theories – not a proactive search for "novel facts".

Consider, for example, the equity risk premia puzzle (*Chapter Two, section 6.1*). Mehra and Prescott (1985) established that there had been a significant return difference between investments in bonds and stocks. This empirical fact started a search for a relevant explanation – not a "novel fact". Nonetheless, I think theoretical developments also can be characterized as novel if they can explain existing phenomena in a different way than previously done. In *Chapter Two, section 6.3.4* I show how Campbell and Cochrane (1999) responded to the equity risk premium puzzle by incorporating an innovative argument into the utility function, i.e. habit persistence. This development progresses the scientific project. On this count as well, therefore, my claim that CCAPM can be characterized as a Lakatosian research programme is not derailed.

Third, Lakatos is criticized because he does not systematically allow for the rational reconstruction of the role of social institutions and history in the development of research programmes. This criticism, I believe, is unduly harsh. Most scientists recognize that social institutions and history combine to influence their thinking. For example, Keynesian economics grew out of the Great Depression while DSGE macro-models were developed under less strenuous economic conditions often referred to as the "Great Moderation".

Taken to the extreme, however, critics would demand a socio-economic explanation of the introduction of, for example, habit-persistence into the asset pricing theory. Since Lakatos sees research programmes evolve, fail and succeed over time, an answer might be warranted. The criticism, therefore, bites because Lakatos did not systematically account for how such developments are assessed. However, any review would require a detailed, and possibly, subjective analysis that, in itself, would demand an investigation into the interpreter's own socio-economic background and intention. We have yet to see the development of rational and objective criteria for such an inquiry. I will on this third account, therefore, dismiss the critics who request such developments.

The *fourth* and final point of criticism raised above also relates to the assessment of any given research programme. Such assessment is mainly related to the empirical adequacy of a given claim. For example, the CCAPM has over the past 30 years undergone various empirical tests in order to establish whether it can provide knowledge for use. Such tests have been carried out at specific points in time throughout its existence. There is, in other words, a dynamic element to its appraisal. Now, critics have it that such tests of empirical adequacy seek to progress the research programme towards establishing true claims about the empirical situations but that the connection between claims and truth has gone unexplored. Maeki (2008), for example, maintains that besides complete and detailed truths about the phenomena being investigated, there might also be incomplete truths about "some limited yet significant aspects of the world" and points towards causal mechanisms. His claim is that the Lakatosian framework is unfit for dealing with such "truths". It requires, so Maeki, a more granular analysis than what Lakatos provides.

This criticism in essence boils down to what I earlier labelled the fallacy of simplification and shows how simplified, idealized and fictional concepts are unfit to bridge the gap between the model world and the real situations. On this point, I side with the critics. Lakatos does not give much attention towards this aspect of a research programme. Can this criticism cast doubt on whether it is appropriate to use Lakatos as a guide towards assessing the CCAPM? I do not think so. Lakatos is more focused on how to assess theories in terms of their progression towards excess theoretical and empirical content. This in itself takes the project towards higher level of truth content. I have chosen to accept this goal but also to question how this can be accomplished given the theoretical starting point of the CCAPM. Again, the type of criticism raised in this final point cannot persuade me to abandon my choice of letting my discussion be framed by the Lakatosian suggestions. Lakatos' insight, therefore, offers a good basis for discussing the merits of Cochrane's effort in mobilizing a "general theory" of asset pricing.

Chapter 4:

CCAPM Ontology, Use and Representation

Introduction

The consumption based capital asset pricing model (CCAPM) is used to explain and predict prices of financial market assets. In a long row of proponents, John H. Cochrane is its main advocate today. My focus has been and will be both on the tradition and Cochrane's contributions. Cochrane sets out explaining the observable asset pricing data by, first, developing a plausible *story* of their behaviour. Next, he lets theories and mathematical equations make this *story* "explicit". Thereafter, Cochrane compares the model's predictions against the real situations. Finally, after concluding that the empirical tests are inaccurate when compared with the real situations, Cochrane initiates adjustments to the theory and its models. These methodological steps have been followed as a normal practice among financial economists within the consumption based asset pricing research programme. However, due to the only modest theoretical and empirical progress, we have justified reasons to doubt the research programme's abilities to provide useful knowledge to the various stakeholders, i.e. investors, public policy makers and fellow academics.

As already stated in the introduction to the previous chapter, I believe there are several explanations for CCAPM's lack of notable progress. They can be found, primarily, in the well-rehearsed agent-based equilibrium theories, the elaborate mathematical structures, and the assessment of the model-based claims. While the first topics, i.e. asset pricing theory, was addressed in *Chapter Three*, and the final topic, i.e. model-based claim assessments is reserved for the next *Chapter Five*, I now turn my attention towards the pricing model itself, i.e. the model "M" in the CCAPM.

I start out with a statement from the preceding *Chapter Three's* introduction, in which Cochrane suggests that the asset pricing theory "contains lots of assumptions". This quote has an extension. Cochrane tells us that these assumptions are applied for two specific purposes: "The theory of asset pricing contains lots of assumptions used to derive analytically convenient special cases and empirically useful representations." (Cochrane 2005, p. 35). Now, in its full length, the statement has three parts; "theory and assumptions", "analytically convenient special cases" and "empirically useful representations". Since I covered the first part in *Chapter Three*, I here turn to the two latter parts.

The *purpose* of this chapter it to clarify what the model "M" in CCAPM *is*, what it is *used* for and, what it *represents*. The *goal* is to identify possible problem areas in and around the research programme's modelling effort that can be held responsible for its lack of success in generating knowledge for use. I *conclude* that "M" is an applied mathematical model which is formed by a few fundamental, core, principles. The model "M" is used as the general starting point for deriving lower-level models, referred to as analytically convenient special cases. These specific cases aspire to be empirically useful representations. Although the analytical cases are de-idealized and de-fictionalized versions of their general starting point, they remain within the confines of the theoretical and find few bridges to the real situations they target. Hence, the provision of knowledge for use for practitioners is limited (see *Chapter Two, section 1*).

I argue my conclusions in four sections. In the *first* section, I seek a better understanding of what Cochrane's asset pricing model *is*, i.e. model ontology. My starting point is the *central asset pricing formula* that I describe in *Chapter Two*, *section 4.2*. Next, I give a short overview of various model types, and let myself be informed by Frigg and Hartmann (2006, 2006a) before drawing on Gibbard and Varian's (1978) classic paper on the topic, supplemented by Giere (1999, 2004, 2008), and Friedman (1953). The *second and third* sections are descriptive. I review how the *central asset pricing formula* is used as a general starting point for deriving a hierarchy of increasingly specific equations that Cochrane calls analytically convenient special cases. Next, I review how Cochrane uses these analytical cases as extensions towards the real situations.

Having established what the model "M" is and how Cochrane uses it to derive analytically convenient special cases and empirically useful representations, I turn to their representational roles. In *section three*, I first identify what the analytically convenient special cases represent. I demonstrate how these cases are de-idealized and de-fictionalized versions of their general starting point formed by a few fundamental, core, principles. In the process of deriving these specific cases, Cochrane makes use of the positive heuristic that the consumption based research programme offers and identifies several new auxiliary, belt, assumptions that are then attached to the existing mathematical structure. I argue that the fundamental, core, principles are present in most of these special cases. The principles, therefore, are vertically portable. Since the cases are tailored to a specific situation, I claim that they are not horizontally portable to situations unlike those they are targeted for. Finally, I let Hausman (1992) suggest that the models economists assemble are mainly used for what he denotes "conceptual explorations". But, I believe Cochrane is more ambitious.

The *fourth* section addresses analytical cases that aspire to make empirically useful representations. I find that Cochrane applies a generally accepted HD framework when "working with theory" to develop these representations. However, I find that only a few of these cases can be extended towards the real situations they seek to explain and predict. Most of the cases are burdened by their fundamental, core, principles that use the triad of temperament as their a priori starting point. Hence, Cochrane needs to build bridges to the empirical and relies on *stories* to do the job. I argue that Milton Friedman's "connectors" are too vague, and Carl Hempel's (1965, 1966) concept of "bridge-principles" is challenging to handle. Nancy Cartwright's representative models, however, might point Cochrane in the right direction.

1. What is the consumption based asset pricing model?

We recall that Cochrane's asset pricing research effort starts with the observation and collection of real stock market prices. In a narrative form, Cochrane explains the behaviour of these prices through time and across economic states. This explanatory *story* pictures an investors' choice to consume and invest and his/her specific preference for stocks referred to as “recession proof”. These stocks are held with the expectation that they provide purchasing power when it is most needed, i.e. in bad economic situations. This *story* is then made “explicit” which means that Cochrane gives the story-based script a theoretical underpinning and a mathematical structure.

In this first section, I want to establish what the model “M” in CCAPM is. I first revisit my description of Cochrane’s main mathematical equation, i.e. the *fundamental asset pricing formula*. Thereafter, I review model ontology from a philosophical point of view.

1.1 The model “M” in CCAPM

At the centre of the Cochrane’s consumption based asset pricing model we find the *central asset pricing formula* that I described in some detail in *Chapter Two, section 4.2, footnote 15*:

$$p_t = E_t\left[\beta \frac{u'(c_{t+1})}{u'(c_t)} x_{t+1}\right]$$

Here is Cochrane explanation:

> “Given the payoff $\boldsymbol{x_{t+1}}$ and given the investor’s consumption choice $\boldsymbol{c_t}$, $\boldsymbol{c_{t+1}}$ it [*the central asset pricing formula*] tells you what market price $\boldsymbol{p_t}$ to expect. Its economic content is simply the first order conditions for optimal consumption and portfolio formation. (...). We relate one endogenous variable, price, to two other endogenous variables, consumption and payoffs. One can continue to solve this model and derive the optimal consumption choice $\boldsymbol{c_t}$, $\boldsymbol{c_{t+1}}$ in terms of more fundamental givens of the model. (...) We shall in fact study such fuller solutions below. However, for many purposes one can stop short of specifying (possibly wrongly) all this extra structure, and obtain very useful predictions about asset prices from [*the central pricing formula*], even though consumption is an endogenous variable.” (Cochrane 2005, p. 6).

In this extract, Cochrane highlights several aspects of the *central pricing formula* and the modelling effort in and around it. *First*, the equation contains interpreted elements. Cochrane tells us, for example, that x_{t+1} is the payoff that a stock is expected to return to the investor at the end of the following period. *Second*, the various interpretations are put in the context of a constrained maximization, i.e. the first order condition. Cochrane refers to the first order condition as the formula's "economic content". This content can be explained either through its technical solutions inside the model, or with the help of intuitive *stories* about the inter-temporal transportation of consumption with the help of "recession proof" stocks. *Third*, the formula, with its left and right hand variables, is endogenous, i.e. it forms a closed system. The expected price of a stock is thus determined by the included variables, i.e. the payoff and the stochastic discount factor – and nothing else. The right and left hand variables in the formula, or equation give us what Cochrane refers to as a model. *Fourth*, we read that Cochrane's formula is flexible. It can be used as a foundation for further explorations. This, however, would involve more structure. More structure means more assumptions of the type I discussed in the previous chapter, i.e. fundamental "core" principles and the auxiliary "belt" assumptions.

In this short review, it becomes clear that Cochrane does not differentiate between the concept of a *central pricing formula* and that of a model. In the context of consumption based asset pricing, the two are regarded as identical. I also note that the formula is but the starting point from which more structure can be added so to derive other equations, i.e. lower level models. Can this formula be anything else than a mathematical model as Cochrane suggests? The next sections reveal my answer.

1.2 Model ontology

In this second section, I first revisit the main characteristics of the consumption based capital asset pricing model. Next, I use the model classification scheme offered by Frigg and Hartmann (2006, 2006a) to explore whether the model "M" falls naturally into one or the other categories that the two authors suggest. Thereafter, I contrast Cochrane's view with one held by the two economists Gibbard and Varian (1978). This short paper is by many considered being an early "classic" on the topic. Additionally, I draw on Giere (1979) and Friedman (1953) to complete my discussion on model ontology. Finally, I formulate a working hypothesis for what Cochrane's asset pricing model is. Let me first, however, make a few general remarks on the model "M" in CCAPM.

When asked what a model is, different people have different views. An architect, for example, associates models with something else than what a physicist has in mind. Frank Lloyd Wright's architectural scale models of private homes or the Copernican model of the universe are cases in point. Adding an economists' opinion to the panel opens up yet another perspective. Cochrane, for example, believes, like many of his colleagues, that models are mathematical equations. These equations are mostly clustered into interconnected structures. Philosophers of science are yet another stakeholder in the model centric discussion. They seem to have an on-going debate concerning what models are and how they differentiate themselves from theories. Their views are more complex and intricate than those on offer by the other discussants. As we go forward, I have little to say related architects and physicist's views, but let my discussion on what the model "M" in CCAPM is and how it is used be informed by philosophers of science.

Frigg and Hartmann remind us that models are extensively used in both the natural and social sciences. From the natural sciences, they give us examples such as the Bohr model of the atom and the MIT bag model of the nucleon. They also list economic and finance related models. Four are explicitly named: The "general equilibrium model of markets", the "Mundell-Fleming model of an open economy", the "Phillips-Curve" model of growth and inflation, and the "Black-Scholes" option pricing model.

Scanning the model related writings of other contemporary philosophers of science like Boumans (2004, 2005), Cartwright (1983, 1989, 1999, 2004, 2007, 2008), Giere (1999, 2004, 2008), Hausman (1992), Maeki (1992, 1994, 1994a, 1996, 2005, 2009), Morgan (2001, 2001a, 2004, 2006, 2008, 2008a, 2011), Morgan and Morrison (1999), Morrison (1999, 2007), reveals only a few additions to the ones mentioned by Frigg and Hartman. None of the authors, however, have in their listings the asset pricing models of the type I discuss in my thesis. Economists with strong methodological views like Backhouse (1994), Boylan and O'Gorman (1995), Hands (2001), Hoover (2001), Lawson (1994, 2009), and Rosenberg (1976, 1994) are also silent on the asset pricing research effort

I think there are several reasons why asset pricing models have not been on the forefront of the economist's and philosophers of sciences discussions. One reason is that there are many more economic models than asset pricing models to choose from when the topic is brought up. Another reason is that asset pricing models are considered less "important" than their macroeconomic cousins. Finally, it can be argued that models in finance and economics are quite similar. Nonetheless, as we have and will continue to see, Cochrane argues that the consumption based asset pricing models stand alone and are unique. They represent the "missing link' between micro- and macro-economic theories. At the time of writing, given the severe financial market, banking and sovereign debt crisis in the western world, it seems pressing to address these pricing models – what they are and how they are used. I start with the former and let Frigg and Hartman guide us onwards.

1.2.1 Frigg and Hartman's model classification

Frigg and Hartmann are of the opinion that models can be better understood if they are properly sorted. I agree. In this context, they refer to model ontology. They draw our attention towards six different categories: "physical objects", "fictional objects", "set-theoretical structures", "descriptions", "equations", and, finally "gerrymandered ontologies". On the category of "equations", Frigg and Hartmann say: "Another group of things that are habitually referred to as models, in particular in economics, consists of equations (which then termed "mathematical models") – for instance the Black-Scholes model of the stock market and the Mundell-Fleming model of the economy." (Frigg and Hartmann 2006, p. 744). Although different from the two economic models that the authors mention, it is natural to conclude that the asset pricing equations encountered in *Chapter Two* of my thesis can be characterized as models. Cochrane confirms this. He has repeatedly told us that the core of the CCAPM is the central pricing formula. Is there more to be said concerning such mathematical models? The short answer is "yes". Let me dwell on this in the next sections before I conclude.

1.2.2 Gibbard and Varian's model views

Gibbard and Varian (1978) also offer an explanation on what a model is and what a model does. In this section, I review their views on ontology and postpone a discussion on representation to *sections 4* of this chapter. Let us start with an extract:

> "A model, we shall say, is a *story* with a specified *structure:* (...) The *structure* is given by the logical and mathematical form of a set of postulates, the *assumptions* of the model. The structure forms an uninterpreted system, in much the way postulates of a pure geometry are now commonly regards as doing. The theorems that follow from the postulates tell us things about the structure that may not be apparent from the postulates alone. (...). In economists' use of models, there is always an element of interpretation: the model always tells a story. If we think of the structure as containing uninterpreted predicates, quantifiers, and the like, we can think of a story as telling what kind of extension each predicate has and what kind of domain each quantifier has:..." (Gibbard and Varian 1978, p. 666).

The *first* point that Gibbard and Varian make is that a model is a kind of formal system that is interpreted by a story. The *second* point is methodological; it discusses the deductive form of arguments that many economists sign-up for doing. Their *third* point is that neither a structure nor a *story* on its own can be a model. It is the combination of the two that counts. The *fourth* point is that economists often do not distinguish between the terms "model" and "theory".

The CCAPM research programme resonates well with Gibbard and Varian's assessment. In terms of the confusing role that economists assign to the terms theory and model, Cochrane provides a good example in his book *Asset Pricing* (2005). Part one is called "Asset Pricing Theory" and the first chapter of that part is given the title "Consumption-Based Model and Overview". This would indicate that the model is set, or discussed, within the theory. In the text, for example, Cochrane says: "A wide class of models suggests that a "recession" or "financial distress" factor lies behind many asset prices. Yet theory lags behind; we do not yet have a well described model that explains these interesting correlations." (Cochrane 2005, p. xiv). Here, Cochrane tells us that we need a theoretical foundation to build better models, i.e. the two terms are not the same. Theories could be considered to be hypothetical descriptions or definition of the real, economic world while models, as indicated earlier, are sets of interlinked equations that aim at making these descriptions and definitions explicit. Nevertheless, I do not intend to pursue the debate related to differences and similarities between “theory” and “model” here but refer to the philosophical and economic literature I mentioned previously when listing various contributors. Let us, therefore, proceed for now as most economists do and use the terms interchangeably. When important, I will remind the reader and make the distinction visible.

In order to bring clarity to the theory and model terminology, Gibbard and Varian suggest applying the term *model* “whenever there is economic reasoning from exactly specified premises”. By now we know that Cochrane also makes use of such “premises”. Cochrane, however, calls them “assumptions”. In the previous *Chapter Three, sections 2.1.1 and 2.1.2*, I distributed these assumptions into two categories; fundamental principles and auxiliary assumptions and likened them with Lakatos’ hard core propositions and protective belt assumptions. Gibbard and Varian also tell us that such premises are the foundation for continued elaboration and “reasoning” within a theoretical framework. Cochrane rather denotes this process “derivations”. Earlier we have seen how Cochrane applies the fundamental, core, principles and auxiliary, belt, assumptions to derive analytically convenient special cases. He even speaks about the “economic content” and specifically refers to the first order condition. In all these circumstances, a structure of interlinked equations is involved. In *Chapter Three, section 3.2.3*, I used Mill’s terminology and called reasoning from such “specified premises” the a priori method.

In previous chapters and sections, I have referred to Cochrane’s causal use of *stories*. A *story* is then told to support the outcome of this a priory, deductive method. The theory of, for example, the representative investor, his character and optimizing behaviour, the financial market structure, and the financial assets are embedded in the *story* concerning the investor’s desire for recession proof stocks. The *stories* are thus a means for communicating the narrative to, possibly, a non-scientific audience. Hence, it is told in a casual style. For the technically skilled set of that cohort, the *story* is made “explicit” in a mathematical structure supported by theories.

We can ask the question, however, whether Gibbard and Varian’s reference to the term “story” is the same reference Cochrane gives his. Initially, it seems that the authors agree. A closer look reveals that there is one main difference. While Gibbard and Varian use the story to predicate the abstract mathematical arguments, Cochrane formulates an intuitive narrative that knits his predicates together to an intuitive whole. The difference can be regarded as the one between a dictionary and a novel. Nonetheless, although there are initial differences, I think Cochrane provides a natural extension of Gibbard and Varian’s use of the term “story”. Hence, I argue that the authors would agree on the usages of the term. It is, therefore, the combination of the structure and the story that makes the set-up formal, rigorous and intelligible.

1.2.3 Giere’s pure and applied models

Ronald N. Giere remind us that a mathematical model can come in two different forms - “pure” and “applied” (Giere 1999, 2004, 2008). While pure mathematical models can look like Cochrane’s *central pricing formula*, the applied model has more substance: “Beginning from a *pure* mathematical model, we can construct an *applied* mathematical model by replacing its mathematical elements with models of real objects and relations.” (Giere 1999, p. 6). As an example Giere presents the following “pure” mathematical model. It is one in which the mathematical terms are left uninterpreted:

$$y - ax - b = 0$$

In an “applied” mathematical model, the letters are interpreted and possibly replaced by other terms: “For example, we can create a general model in which the variable y is distance from a fixed origin, **x** is time **t** from an arbitrary starting time, which can be zero, **a** is the velocity, **v** of a moving point, and **b** the initial distance $\mathbf{d_0}$ of the moving point from the origin.” (Giere 1999, p. 6).

Following Giere, I think it is wrong to characterize Cochrane's central pricing formula as a "pure" mathematical model. Cochrane certainly has something in mind when he writes down the formula because he gives precise interpretations, i.e. descriptions of what the various letters and arguments mean. By giving them meaning, Cochrane connects the *story* of what he believes goes on in the financial markets with the equations in the model. In this way, the mathematical language formalizes his *story*. Or in Cochrane's words, the mathematics makes his *story* "explicit". Cochrane's pricing formula is, therefore, richer than Giere's pure model which takes the form of an uninterpreted linear equation. Cochrane insists that a model must have a theoretical basis and the central pricing formula goes beyond this level of "purity".

1.2.4 Friedman's layers

In possibly one of the most widely distributed research papers on economic methodology, Milton Friedman tells his readers that economists should not be judged by the "realisticness" of their assumptions but by the theory's predictive success within its designated empirical area of applicability (Friedman 1953). While I discuss this aspect of Friedman in some detail in *Chapter 5, section 4.1*, I direct my attention towards another topic addressed by Friedman in relation to theories and models:

> "...a hypothesis or theory consists of an assertion that certain forces are, and by implication others are not, important for a particular class of phenomena and a specification of the manner of action of the forces it asserts to be important. We can regard the hypothesis as consisting of two parts: first, a conceptual world or abstract model simpler than the "real world", and containing only the forces that the hypothesis asserts to be important; second, a set of rules defining the class of phenomena for which the "model" can be taken to be an adequate representation of the "real world" and specifying the correspondence between the variable or entities in the model and the observable phenomena." (Friedman 1953, p. 24).

This quote is rich in content. I believe it tells us three important things: *First*, a theory is a collection of isolated "forces" believed to be of importance in explaining events and processes. Friedman's suggestion expresses a view that is in line with my analysis of Mill's and Cochrane's preferred research strategy, as discussed in *Chapter Three,* *sections 1.3.2* and *3.2.3*. *Second*, the theory is manifested by, or made explicit in, as Cochrane uses the term, a model. *Third*, Friedman tells us that such a model consists of two parts, i.e. abstractions and correspondence rules. We are here neither told what a model exactly *is* nor what correspondence rules are. Nonetheless, we gain the impression that the model, like its theory, contains a limited number of "forces" that, possibly, are expressed in mathematical equations. The correspondence rules help translate the abstract model terms to "every-day" concepts, i.e. they establish a connection between the model and the "real world", or, in other words, the rules fit the data to the model. I do not think Friedman means that these rules are an integral part of a model. They are more, I believe, purposeful interpretations by the model developer.

Friedman's model views helps me define what Cochrane has in mind when he claims that a model is a formula that finds specific uses, i.e. derive analytically convenient special cases and empirically useful representations. Friedman, like Giere, does not halt his analysis at the theoretical, level, but extends the model towards the empirical. [44] For that purpose, Friedman needs to interpret the model terms so that the data can be fitted onto the model. From Friedman, therefore, we get the impression that the model plays a dual role. On one hand, the model is characterized by a few, possibly, idealized "forces" that are then used to explore their, theoretical, consequences. On the other hand, the model is applied to the data, i.e. it extends beyond the theoretical towards the empirical. A model, therefore, as I will discuss in some length in *sections 3* and *4* of this chapter, can be used in several ways. Having several uses also implies that the model can represent in several ways, and that we can learn from it in several ways. More on this will follow later in the next two sections. Now, let me return to the topic of model ontology and conclude.

1.3 An applied mathematical model

When presenting us with different categorical "options", Frigg and Hartmann remind us that model classification is not straight forward and that they are all subject to particular

[44] Here, Friedman uses the term "abstract" and not "idealizations". The term, like idealization, has many interpretations. In most cases, idealizations involve distortions by altering some particular features of the object while abstraction is about omitting features, i.e. subtracting features from an object.

points of criticisms. If models, for example, are classified as "descriptions", like in textbooks, a German translation of Cochrane's 2005 book on *Asset Pricing* does not give rise to a new model of asset pricing. Similarly, if Cochrane's pricing model is characterized as a set of "equations", different functional forms of the same situations cannot be characterized as different models. Cochrane tells us, for example: "It is often convenient to express asset pricing ideas in the language of continuous-time stochastic differential equations rather than discrete stochastic difference equations as I have done so far." (Cochrane 2005, p. 25). Cochrane's point is not regarding which of the two equations are right, but which one is more suitable for describing a particular situation. The constructed model man inside the CCAPM, for example, lives continuous and not discrete lives. [45] However, they receive investment payments at discrete points in time - at which they also enjoy consumption. It is, however, difficult to maintain that Cochrane's two stochastic equations give rise to two different models of asset prices. The equations are thus two ways of describing the same thing.

Despite these issues, it seems fairly straight forward, given the offered classification by Frigg and Hartmann and the consideration by Gibbard and Varian as well as Giere and Friedman that the model "M" in CCAPM as presented by Cochrane, i.e. the *central asset pricing formula*, is an applied mathematical model. This interpretation also contains what Gibbard and Varian referred to as a "structure" and a "story". Cochrane's structure is thus equations and their interconnectedness on one hand, and, on the other hand, his *story* interprets these terms. From the previous *Chapter Three, section 3.1* on the historical development of the CCAPM in the context of Lakatos' research programme, it is also clear that the use and development of mathematical models have been a main activity for the consumption based asset pricing advocates. Some of these models, as seen, for example in Lucas (1978), are purely mathematical, but most of them are applied to real, empirical, situations. Knowledge for use by stakeholder, therefore, has been a main direction of this research.

[45] In *Chapter Three, section 1.1.2*, I argue that the model man inside the CCAPM is first made ideal by isolating his sentiments, i.e. rational, self-interest, and risk averse. Then he/she is given distorted extensions such as almost perfect foresight and eternal life. These givens have so far not been found in any real situation counterparts. Hence, by adding such traits, Cochrane and the CCAPM tradition moves beyond idealizations to constructions, i.e. fiction. This might help explain the lack of statistical success.

Having defined what the model "M" in CCAPM is, let me progress and ask what this model is used for. Cochrane has two suggestions. It used for, first, deriving what he calls "analytically convenient special cases" and, second, "empirically useful representations". In the section below, I review the former and leave most of the discussion of Cochrane's second application for *section 3*.

2. How is the consumption based model used?

What does Cochrane have in mind when he says that the theories of asset pricing are used to "derive analytically convenient special cases" and "empirically useful representations"? *First,* we learn that theories are of use, i.e. they are used to derive. *Second*, theories derive both the analytical cases and the empirical representations. *Third*, the convenient cases are not the same as the useful representations. *Fourth*, there is an indication of sequencing – first theories, then cases followed by representations. Not only would this indicate that Cochrane's analytical cases are different from his empirical representation, it would also put the cases closer to the theory than the representations. The representations are then, presumably, closer to the real situations than the cases. By pointing towards these various topics, I seek to reconstruct Cochrane's original statement related to the analytical cases in this section, and, thereafter, in *section 2.2*, follow the same strategy with respect to the empirical representations.

2.1 The analytically convenient special cases

Cochrane uses the framework of the asset pricing theory to introduce a range of what he denotes "classic" research issues in the field of finance:

> "A few simple rearrangements and manipulations of the basic pricing equation (...) give a lot of intuition and introduces some classic issues in finance, including determinants of the interest rate, risk corrections, idiosyncratic versus systematic risk, beta pricing models, mean variance frontiers." (Cochrane 2005, p. 10).

Here, I am interested in what these classical cases are. Below, I demonstrate how Cochrane works from the *basic pricing equation* via the *central pricing formula* to one of them, i.e. the analytical case of "risk correction". To get to the end-point, Cochrane takes us via another classic case, i.e. the risk free interest rate.

The risk free interest rate can be approached in two ways; either the consumer knows at the time of making the decision to consume and invest, what that interest rate is, or he does not know. The latter is for obvious reasons, more difficult to handle due to the introduction of two risk dimensions, i.e. time and uncertain payoffs. Let us start with the certainty case.

Cochrane has told us that the *basic pricing equation* is described as $p_t = E_t\,(m_{t+1}\ \ x_{t+1})$. In the absence of uncertainty, m_{t+1}, i.e. the stochastic discount rate, or SDF, is known. It is, for example, the observable over-night deposit rate an investor can earn on the cash-balance he places with his/her local savings bank. As we know from *Chapter Two, section 4.3*, the pricing equation can then take the following form:

$$p_t = \frac{1}{R^f}\,x_{t+1}$$

where R^f is the risk free interest rate. Cochrane refers to this rate as a discount factor. Next, he opens up for uncertainty regarding the risk free rate. This introduces a new specification of m_{t+1} to account for the subjective discount factor β and the utility function. Using Cochrane's preferred functional form, i.e. power utility:

$$u\,(c_t) = \frac{1}{1-\gamma}\,c_t^{\ 1-\gamma}$$

the risk free rate is (Cochrane 2005, p.11):

$$R^f = \frac{1}{\beta}\,\Big(\frac{c_{t+1}}{c_t}\Big)^{\gamma}$$

Cochrane gives the following explanation:

> "Real interests are high when people are impatient, i.e. when β is low. (...). Real interest are high when consumption growth is high. In times of high interest rates, it pays investors to consume less now, invest more, and consume more in the future. (...). Real interest rates are more sensitive to consumption growth if the power parameter γ is large. If utility is highly curved, the investor cares more about maintaining a consumption profile that is smooth over time and is less willing to rearrange consumption over time in response to interest rate incentives." (Cochrane 2005, p. 11).

Intuitively this makes sense. High inflation adjusted interest rates, i.e. real rates induce investors to save rather than spend. This makes their real wealth grow to the tune of the interest rate, more can be spent on consumption later, and their total marginal utilities increase.

Having reviewed the risk free rate analytical case, we are now ready to derive the "risk correction" case. Risk correction tells us how the investor accounts for the risk that he/she faces with respect to his/her investment when the return of that investment comes in subsequent periods and is conditional upon the realization of a specific economic state. It leads to the concept of an ex ante required return of an investment. Intuitively, the price of risk is positive, i.e. the more risk the investor is willing to take, the higher is the compensation he/she would demand. If this "mark-up" requirement or risk premium is not rendered, the investor will refrain from investing in that particular financial asset. To get to this risk corrected required return, Cochrane introduces the definition of a covariance:

$$\mathrm{cov}\,(m_{t+1}\, x_{t+1}) = E\,(m_{t+1}\, x_{t+1}) - E(m_{t+1})\, E(x_{t+1}).$$

Next, Cochrane suggests writing;

$p_t = E(m_{t+1})\, \mathrm{E}(x_{t+1}) + \mathrm{cov}\,(m_{t+1}\, x_{t+1})$ as;

$p_t = E(m_{t+1}\, x_{t+1})$ and input the risk free rate, we get;

$p_t = E(x_{t+1}) / R^f + \text{cov}\,(m_{t+1}\, x_{t+1})$.

Rearranging, and making a fuller representation of m_{t+1} we get the risk corrected formula (see *Chapter 2, section 4.4)*:

$$p_t = \frac{E(x_t)}{R^f} + \frac{cov\left[\beta\, u'(c_{t+1}),(x_{t+1})\right]}{u'(c_t)}$$

As I already stated, this classic risk correction formula at the end of the above derivations, is a good example of an analytically convenient case. There are several elements attached to it that I now will describe.

Cochrane explains:

> "The first term [*in the final equation*] is the standard discounted present value formula. This is the asset's price in a risk-neutral world – if consumption is constant or if utility is linear. The second term is the *risk adjustment*. An asset whose payoff covaries positively with the discount factor has its price raised and vice versa." (Cochrane 2005, p.13).

The intuition, or s*tory* that Cochrane evokes is that:

> "[*investors*]...do not like uncertainty about consumption. If you buy an asset whose payoff covaries positively with consumption, one that pays off well when you are already feeling wealthy, and pays off badly when you are already feeling poor, that asset will make your consumption stream more volatile. You will require a low price to induce you to buy such an asset. If you buy an asset whose payoff covaries negatively with consumption, it helps to smooth consumption and is more valuable than its expected payoff might indicate." (Cochrane 2005, p. 13).

I interpret such classical cases in asset prices theory to be examples of the analytically convenient special cases that Cochrane alludes to. Like the *central asset pricing formula*, they are interpreted mathematical equations, form closed systems, are flexible, and come with an intuitive *story*. The mathematical extension of the starting point, as shown here, fits comfortably within the tradition of the consumption based capital asset pricing research programme. The fundamental, core, principles described by the triad of temperaments are thus carried forward into other, lower level, equations, while intuitive "rearrangements" and "manipulations" are supported by changes in and additions to the set of auxiliary, belt, assumptions. The heuristic of the research programme is exploited to its fullest by sticking to the axiomatic development methodology that I discussed earlier in relation to Mill in *Chapter Three, section 3.2.3*.

When the mathematical terms in the lower level equations are interpreted, some of the analytical cases are immediately applicable to the empirical situations. The observable risk free rate is one example in which the mathematical name is predicated and attached to a specific time series of data over a specified period of time. However, this equation is not deduced from the fundamental, core, principle but "plugged-into" the consumption based capital asset pricing structure. It is thus included in the "belt" of auxiliary assumptions. Other analytical cases lack the ability to connect to the empirical world. The utility function and the equilibrium condition are examples thereof. In such cases, the triad of temperaments, i.e. rationality, self-interest and risk aversion is the core building block which is also the starting point for mathematical derivations. Hence, not all analytical cases can be taken beyond the theoretical and reach the empirical arena.

Before I start analysing the analytical cases from a philosophical point of view along the dimensions use, representation and learning, let me first turn to the empirically useful representations in the next section.

2.2 The empirically useful representations

Cochrane told us in the introduction to this chapter that the consumption based asset pricing theory can derive both analytically convenient special cases and empirically useful representations. In the previous section, I suggested that the combination of fundamental, core, principles and auxiliary, belt, assumptions derive a hierarchy of specific models called analytically convenient cases. The development from the most general level of the *central pricing formula* to lower-level analytical cases of, for example, the risk corrected equation, gives us new sets of equations, or models that are used to represent specific financial market situations.

So far I have accepted that the framework of the asset pricing theory can be used to assemble such a hierarchy of analytical cases. The question I pursue in this section, is how these cases break out of their theoretical background and connect with the empirical situations. Cochrane does not seem to leave much doubt. He claims it is possible, and even necessary to make this connection. His vision is:

> "The model [*central asset pricing formula*] I have sketched so far can, in principle, give a complete answer to all the questions of the theory of valuation. It can be applied to any security – bonds, stocks, options, futures, etc. – or to any uncertain cash-flow. All we need is a functional form for utility, numerical values for the parameters, and a statistical model for the conditional distribution of consumption and payoffs." (Cochrane 2005, p. 42).

Cochrane adds: "Most of the theory of asset pricing is about how to go from marginal utility to observable indicators." (Cochrane 2005, p. 3). He continues:: "The accent of this book is on understanding statements of theory, and working with that theory to applications,..." (Cochrane 2005, p. xvi). And, finally: "Economic theory and modelling is often portrayed as an ivory tower exercise, out of touch with the real world. Nothing could be further from the truth,..." (Cochrane 1997, p. 12).

Following this myriad of statements, let me make the following two main observations: *First*, Cochrane's vision of establishing *the* theory of asset pricing and its empirical usefulness is extremely broad, certainly not modest, but at least he is clear on what he thinks the model "M" in CCAPM can be used to do. *Second*, Cochrane sets himself the goal to "work with theory", as we have seen several times for example in *Chapter Two, section 6,* to bring the asset pricing theory to the data and evaluate its predictions against the real situations. Cochrane, therefore, suggests making the theory explicit in the pricing model and specifying it in a way that makes it useful for these comparisons.

From Cochrane's point of view, therefore, theory, through its analytical cases, can reach all the way down and "touch" the empirical situations. This is certainly challenging. Below, I review how he intends to fit the model to the data. Consider another classical case in which Cochrane establishes an analytical case that seeks to explain the, empirical, expected excess return from an investment. We recall form *Chapter Two, section 6.1*, that the excess return is the numerical difference between the return on an investment and the short term risk free interest rate.

Cochrane starts with the standard power utility function that we first encountered earlier in *Chapter Two, section 3.2*:

$$u\,(c_t) = \frac{1}{1-\gamma} c_t^{\;1-\gamma}$$

The excess returns should obey (Cochrane 2005, p. 42) :

$$0 = E_t \left[\beta \left(\frac{c_{t+1}}{c_t} \right)^{-\gamma}, R^{e}_{\;t+1} \right]$$

Cochrane then suggests "taking unconditional expectations and applying the covariance decomposition, [*and*] expected excess return [$E(R^{e}_{\;t+1})$] should follow" (Cochrane 2005, p. 42):

$$E(R^{e}_{\;t+1}) = - R^{f} \operatorname{cov} \left[\beta \left(\frac{c_{t+1}}{c_t} \right)^{-\gamma}, R^{e}_{\;t+1} \right]$$

Cochrane concludes: "Given a value for γ and data on consumption and returns, you can easily estimate the mean and the covariance on the right hand side, and check whether actual expected returns are, in fact, in accordance with the formula." (Cochrane 2005, p. 42). Despite this encouragement, Cochrane adds: "Unfortunately, this specification of the consumption based model does not work very well." (Cochrane 2005, p. 43). This statement takes a more central role in *Chapter Five*, in which I assess Cochrane's reactions to statistically unsuccessful model-based predictions.

Cochrane believes that in this example of the expected excess return to have derived an empirically useful representation. It shows how he "works with theory", equations, derivations, data and statistical measures to bring the asset pricing research programme closer to the real situations. In fact, in this example, he only needs to estimate two free parameters, i.e. the risk aversion of the representative investor γ and his/her subjective discount factor β. The rest, Cochrane would probably say, is a matter of mathematics, the availability of data, and their statistical properties. The challenge, however, is to turn "analytically convenient special cases" of excess expected return into "empirically useful representations". And this, as we are told, "does not work very well". Something, therefore, is going wrong. The next *section 4* focuses on possible reasons.

Before I get there, let me sum up. Having established first, that the model "M" in CCAPM is an applied mathematical model, I gave examples of how this model is used to derive a hierarchy of lower level models that Cochrane calls analytically convenient special cases and empirically useful representation. Not all cases can easily be extended to also represent real situations because they involve the unobservable triad of temperaments. The case study of the expected excess return is one such example. This might help explain why consumption based asset pricing, so far, has failed to deliver successful statistical results.

Let us now approach the topic of model ontology and use in the context of a philosophical analysis. In the following sections, I therefore take a different perspective. I will discuss two main topics. *First*, in *section 3*, I discuss how the "analytically convenient special cases" represent the consumption based asset pricing theory. *Second*, in *section 4*, I ask how Cochrane "works with theory" to reach the empirical situations, i.e. fitting theories to the world.

3. *Analytical cases and their representation*

In *Chapter Three, section 1.3*, I centred the discussion on how a rich and complex reality could be simplified and made "scientifically manageable" by suppressing perceived irrelevant details. This strategy, as advocated by James S. Buchanan and David F. Hendry, took us to Uskali Maeki's "process of isolation" and "idealization", to Mary S. Morgan's "fictional constructions", and to Daniel M. Hausman's suggestion that economics can be considered to operate in a "separate realm". This leads to, as I argued in *Chapter Three, section 1.4*, to "fallacy of simplification", i.e. reconciling the theoretical cases with the real situations.

In relation to my assessment of the CCAPM in the context of a Lakatosian research programme (*Chapter Three, section 3*), I demonstrated that Cochrane's asset pricing theory was developed along those lines with respect to his three theoretical building blocks, i.e. the representative investor, his/her character and behaviour, the financial market structure, and the financial assets. In particular, I showed that the programme's heuristic supported such mathematical extensions of an a priori starting point.

Here, I discuss what the analytically convenient special cases represent. Cochrane argues that they are empirically useful representations. I do not agree on all counts. I find that the analytically convenient special cases are lower-level models developed from their more general starting point, i.e. the *central asset pricing formula*, with the help of a myriad of auxiliary, belt, assumption. These specific cases certainly aspire to be empirically useful representation, but I find this goal ambitious. Although the analytical cases are de-idealized and de-fictionalized versions of their starting point, they remain within the confines of the theoretical and find few bridges to cross-over to represent the real situations they target.

It is not surprising that the special analytical cases retain much of their idealized and fictional starting point. I have already concluded in *Chapter Three, section 1*, that the asset pricing research programme's main building blocks are idealized and fictional as well. In this section, however, I demonstrate that idealized and the fictional elements are useful when thought of in the context of the model's mathematical extension. The analytical cases should be used for theorizing. As a consequence, when the analytical cases seek their empirical counterparts, the models' ability to render knowledge for use diminishes. I let my assessment be informed primarily by Frigg and Hartmann (2006, 2006a), Gibbard and Varian (1978) as well as Morgan (2001, 2006, 2008, 2008a 2009).

3.1 What models represent

Frigg and Hartmann (2006, 2006a) take a broad view of what a model can represent:

> "Models can perform two fundamentally different representational functions. On the one hand, a model can be a representation of a selected part of the world (the "target system"). Depending upon the nature of the target, such models are either models of phenomena or models of data. On the other hand, a model can represent a theory in the sense that it interprets the laws and axioms of that theory." (Frigg and Hartmann 2006, p. 741).

Here, we learn that the model has two representational roles; it can represent a "selected part of the world" or it can represent "theory". The two authors are quick to point out, however, that: "These two notions are not mutually exclusive and scientific models can at once be representations in both senses." (Frigg and Hartman 2006, p. 741). Like Frigg and Hartmann, Gibbard and Varian (1978) also explore the representational use of models. Their focus is on models that are developed and used by "economic theorists". These models are divided into the "descriptive" and the "ideal" type. While the descriptive models "attempt to describe in some sense, economic reality", the ideal models: "...are concerned with the description of some ideal case which is interesting either in its own right or by comparison to reality." (Gibbard and Varian 1978, p. 665). The descriptive model again has two branches – "models of approximations" and "models of caricatures": "Approximations aim to describe reality, albeit in an approximate way. Caricatures seek to give an "impression" of some aspects of economic reality, not by describing it directly, but rather by emphasizing, even to the point of distorting, certain selected aspects of the economic situation." (Gibbard and Varian 1978, p. 665).

We also recall from *Chapter Three, section 1.3.2*, that Maeki's suggested process of isolation, i.e. theoretically removing a set of elements "from the influence of other elements in a given situation", leads to idealizations. For example, the triad of investor's temperaments, i.e. rationality, self-interest and rationality is "sealed-off" from other traits such as envy, lust, and pride, sense of duty, loyalty, irrationality, behavioural biases, etc. Frigg and Hartmann show a similar process of isolation when discussing simplifications. They say that certain properties of an object can be removed in order to understand it better: "An idealization is a deliberate simplification of something complicated with the objective of making it more tractable." (Frigg and Hartmann, 2006, p. 741). Maeki, Frigg and Hartmann, and I will include Gibbard and Varian as well, agree on both the process of isolation and its result, i.e. idealization. They also agree that a model can represent its theory. While Frigg and Hartmann are explicitly mentioning this latter possibility, Gibbard and Varian are more subtle. They tell us that the models they review are used by "economic theorists". In the next section, I take a closer look at the idealized model and its relationship with theory.

3.1.1 Idealization and fictions

Frigg and Hartmann maintain that idealized models come in two variations; the Aristotelian and the Galilean. The Aristotelian idealization: "...amounts to "stripping away" all properties from a concrete object that we believe are not relevant to the problem at hand." (Frigg and Hartmann 2006, p. 741). As an example of Aristotelian idealizations in the natural sciences, Frigg and Hartmann mention the Bohr model of the atom and the MIT bag model of the nucleon. In the social sciences, the two authors refer to the "Phillips-Curve" model. [46] The Galilean idealizations seem to be different as they: "...involve deliberate distortions." (Frigg and Hartmann 2006, p. 741). Here, the example the two authors have chosen from the natural sciences are "point masses moving on frictionless planes" and the 'study of isolated populations" in biology. The social sciences are represented by the assumption that "agents are perfectly rational". Frigg and Hartmann also point out that these two types of idealizations, i.e. the Aristotelian and the

[46] This model is often graphically represented by a convex curve drawn in a coordinate system with unemployment on the x-axis and inflation on the y-axis. As unemployment grows, inflation abates.

Galilean are not mutually exclusive. Often, they go hand in hand. This happens, they claim: "...in what is sometimes called "caricature models" which isolate a small number of main characteristics of a system and distort them into an extreme case." (Frigg and Hartmann 2006, p 742).

Distortions, we recall, was also a central topic in *Chapter Three, section 1.3.2*, where I referred to Maeki's who even goes as far as claiming that idealizations encompasses "almost anything that theoretically deforms reality". This points us in the direction of the "caricatures" that Frigg and Hartman as well as Gibbard and Varian make references to. Also Morgan (2006, 2011) makes a similar point: "Morgan (2006) interprets the caricaturing process as something more than exaggeration of a particular feature, rather it involves the addition of features, pointing us to the constructed nature of the exaggeration rather than to it as an idealization." (Morgan 2011, p. 19). To demonstrate this in the context of the CCAPM, I maintained that the representative investor's character and behaviour is not only idealized. He/she is also constructed and take on a fictional character because of his/her allegedly near to perfect foresight and eternal life (see *Chapter Three, section 1.3.2*). Hence, these constructed, fictional caricature cases are taken beyond what can be considered to be the "ideal".

My interest, therefore, is directed towards the combination of the Aristotelian and the Galilean idealizations.

3.1.2 Richer analytical cases

I start my analysis of the analytical case considered here, i.e. the risk correction with the *central pricing formula*. [47] This formula, as we know, is an applied mathematical model. It is used as a general starting point from which more specific analytical cases and empirical representations are derived. While the *central pricing formula* already contains, as Cochrane points out, "lots of assumptions", the more specific cases and representations

[47] Risk correction tells us how the investor accounts for the risk that he/she faces with respect to his/her investment when the return of that investment comes in subsequent periods and is conditional upon the realization of a specific economic state.

contain even more. For example, in the previous *Chapter Three, section 2*, I order the central pricing formulae's assumptions according to their relative importance and grouped them into two main buckets, i.e. fundamental, core, premises and auxiliary, belt, assumptions.

The risk corrected asset pricing formula contains these principles and most of the assumptions. Nonetheless, it is expanded or augmented with additional arguments such as those related to the covariance between the expected future payoff from investments and the expected future consumption as well as the expected future risk free interest rate. This expansion is thus rooted in the *central pricing formula*, but many of the auxiliary, belt assumptions are imported directly into the structure. These imports are, therefore, not always deduced from the fundamental, core, principles. In fact, many of them completely separate from them. The risk free rate is a good example. It is tagged onto the existing mathematical structure. As more and more assumptions are added to the *central pricing formula*, the more able these cases should become to replicate specific real situations. The risk corrected equation, therefore, is richer that its starting point. It is also a more targeted version of its more general starting point. It zooms in on a specific situation. Furthermore, and as a consequence of the targeted approach, a more specific *story* can be told with respect to the investor and his/her behaviour than just his/her demand for what Cochrane alludes to as "recession proof" stocks. Cochrane now builds on that initial, more general *story*, and tells us that the rationale for this demand lies in the way the payoff covaries with consumption.

The risk corrected analytical case, therefore, exposes us to several innovations. Below, I will mention four. *First*, in comparison with the *central pricing formula*, the new risk corrected equation contains two new arguments. This makes the risk corrected case richer in content than its more general starting point. *Second*, and as a consequence of my first point, adding additional auxiliary, belt, assumption to the *general asset pricing formula* places the enriched versions thereof, i.e. the risk corrected version in a better position to target a more specific situation than that under review by its general starting point. The richer, more targeted equation also has a different mathematical structure than its starting point.

Third, the analytical cases tell new *stories*. Now we hear about the risk corrected return an investor would require in order to buy and hold stocks with a specific statistical excess return property, i.e. negative covariance with private consumptions. Earlier, at the more general level of the central pricing formula, we were told that stock prices equals as Cochrane maintains: "...expected discounted payoff. The rest is elaborations, special cases, and a closet full of tricks that make the central equation useful for one or another application". (Cochrane 2005, p. xiii). *Fourth*, when adding the new arguments, the fundamental, core, principles remain untouched, but the catalogue of auxiliary, belt, assumptions increases. In *Chapter Three, section 3.2*, I discussed this strategy as a good example of what Lakatos claims to find in a scientific research programme. At its core of such programmes, there is a stable set of principles that are used to derive more specific lower level models supported by a broader set of flexible auxiliary, belt, assumptions. But not only that. Cochrane also uses the heuristic of the consumption based asset pricing programme. As I showed in *Chapter Three, section 3.3*, the tradition of its practitioners has been to pro-actively develop the auxiliary, belt, assumptions to account for more specific real situations.

Of the four innovations that I mention in the previous section, the first two are of importance at this stage of my analysis. In the next two paragraphs, I will address them.

3.1.3 De-idealization

The first two points I raised above states that a specific analytical case, such as the risk corrected equation, is richer than its starting point because it has been furnished with additional auxiliary, belt, assumptions. The development of the risk corrected equation is not an isolated example. It forms the back-bone of Cochrane's strategy to derive other "classic" issues in finance. A similar method is also used to derive, for example, the "determinants of the interest rate, idiosyncratic versus systematic risk, beta pricing models". Cochrane, therefore, develops a multitude of new and more granular asset pricing models that are interconnected in a web of mathematical structures. It is targeted at representing specific situations observable in the financial market. Whether these analytical cases can reach all the way down, is a topic I discuss later in *section 4.2*. Here, I am interested in the way the central pricing formula is enriched with additional arguments to build a net of interrelated equations.

In the process of adding additional auxiliary, belt, assumptions, Cochrane de-idealises the more general starting point of the *central asset pricing formula*. This process is described by Morgan:

> "One influential defence of idealization is the idea of de-idealization, according to which the advancement of science will correct the distortions effected by idealizations and add back the discarded elements, thus making the theoretical representations become more usefully concrete or particular. A classic formulation of this position was provided by Tjalling Koopmans who thought of models only as intermediary versions of theories which enabled the economists to reason his way through the relations between complicated sets of postulates. In the process of this discussion, ..., he portrayed "economic theory as a sequence of models". (Morgan 2008a, p, 9).

In this extract, my earlier reference in *section 1.2.4* of this chapter to Milton Friedman becomes visible. Friedman (1953) also refers to the dual role of models, i.e. exploratory tools for theorizing within a "conceptual world" thought of as being less complex than its real counterpart, and as connectors that may take the theory all the way to the real situations with the help of what he denoted "correspondence" rules.

De-idealization, clearly, seeks to bring more "realism" into the isolated, idealized, and even fictional cases that in the context of CCAPM describe the representative investor, the financial market and financial assets. Cochrane's development of the CCAPM brings many examples of this strategy. Moving away from assuming zero taxes and transactions costs to assigning them positive values or choosing between a discrete or continuous calculation are cases in point when making a model more tractable than its general starting point. Introducing the risk free rate or the concept of covariance to the simple starting point of discounted payoff are additional examples. We here take note of the fact that these examples of de-idealization relate to the set of auxiliary, belt, assumptions. So far, little progress has been seen in terms of de-idealizing the fundamental, core principles. The triad of temperaments remains fairly resilient towards such attempts although Cochrane, and other financial economists, have experimented with, in particular, rationality and risk aversion. For example, in the next *Chapter Five, section 2.2*, I will, discuss de-idealization again in the context of Cochrane's "habit-persistency" argument and the utility function of the representative investor.

De-idealization, as Morgan points out elsewhere, can, in some cases, also be problematic. She points, for example, to mathematical challenges as de-idealization can upset the derivations "to go through", and to disruption in the model's "causal structure":

> "If it really is the case that there are only a very few or one strong causal factor and the rest are negligible then the minimizing strategy suggests that adding more detail to the models may in fact render the model worse from an epistemic point of view. It makes the explanatory models more complicated and diverts attention from the more relevant causal factors to the less relevant." (Morgan 2008a, p. 12).

In the context of the CCAPM, in which just a very few causes have been isolated, idealized and even given fictional character, this problem seems acute. However, as we shall see in Chapter, section 2, Cochrane, very effectively, deals with the challenges Morgan here alludes to.

In sum, de-idealization aims at making theories more realistic than their general, idealized and fictional starting point. The enhanced lower level equations, therefore, have been mathematically extended to offer more precision and applicability to real situations where the assumptions may approximately hold. For every analytical case, therefore, there is a specific claim, and as Cochrane explores new and possibly granular cases, the more specific these claims get. This process, however, is not always straight forward – a topic in the two next sections.

3.1.4 Vertical Portability

There is another aspect to consider with respect to the research programmes' practice of adding assumptions to a general starting point in order to account for specific situations. It is related to how the more general starting point, i.e. the *central asset pricing formula*, is transported across these more specific analytical cases. The portability of the starting point is, therefore, interesting in its own right. Let me call it "vertical portability". It differs, as I will show in *section 3* below, from the portability of analytical models across different real situations. This I refer to as "horizontal portability".

Vertical portability thus refers to where and how the fundamental, core, principles travel through the hierarchy of increasingly specific analytical cases. In the example I am pursuing here, i.e. risk correction, this journey becomes apparent. Risk correction, as I have already stated, is related to how the representative investor accounts for the risks that he faces with respect to his/her investments when the return is uncertain in terms of both the timing of the payoff and its size. It is clear from this definition of the risk correction, that the concept is well embedded in both a first order condition and an equilibrium condition. Both elements are thus a consequence of the fundamental, core, principles. These principles are thus extended, vertically, down to the lower level case of risk correction. One could argue that the strength of the fundamental principles has been diluted because new mathematical arguments have been added in the process. I am here in particular thinking of the risk free rate that has been imported into the mathematical structure of the CCAPM. However, I would dismiss this idea on the ground that however weak the fundamental principles, they are active and in control of deriving the final, equilibrium, result. Vertical portability is therefore an important aspect of the consumption based asset pricing research programme.

3.1.5 Horizontal Portability

Let me now address the issue of portability in another context. The type of portability I have in mind is related to the way the analytically convenient cases can be re-used in situations unlike those they are targeted for. I refer to this as "horizontal portability". I let Cartwright guide us:

> "If the deductions have been carried out correctly and the general principles employed are true in the target situations, the result of the model will obtain in any real situation that fit the description that the model provides. And in general we have no reason to think they will obtain anywhere else." (Cartwright 2007, p. 225).

Cartwright here tells us that a model that is specific, de-idealized and de-fictionalized enough will be able to describe at least one specific case. Such models will render correct predictions concerning a given situation because they are tailored just to that situation. That kind of model will thus only describe other cases that satisfy almost all the same assumptions. This implies that the horizontal portability of Cochrane's analytical cases is restricted.

In another context Cartwright asserts:

> "The situation must resemble the model in that the factors that appear in the model must represent features in the real situation (...) But it must also be true that nothing too relevant occurs in the situation that cannot be put into the model." (Cartwright 1999, p. 187).

These requirements again raise the bar for the horizontal portability of Cochrane's analytically special cases, but for a different reason. Cochrane, I believe, can in a reasonable way comply with Cartwright's first point. It can thus be argued that the fundamental, core, principles, i.e. the triad of temperaments are "factors" that represent "features" in the real situations. Cartwright's second point, however, is more challenging. It seems reasonable to expect that the idealized and fictional model world can hardly, even with a host of additional auxiliary, belt, assumptions account for all the factors that are at work in specific real situations. So we tighten the requirement and ask the model to incorporate only the "relevant factors at work in the real situations". This, however, is Cochrane's stumbling block because, so far, he has been struggling to identify the other relevant factors – beyond those incorporated in the fundamental, core principle.

We thus have the following conundrum. The triad of temperaments can be considered important enough to be carried over, i.e. horizontally transported to all situations that are targeted by the asset pricing theory. The de-idealized and de-fictionalized analytical cases are customized to very specific situations and lack horizontal transportability. And, finally, there is "something too relevant" occurring in the real situations that has yet to be identified. If it is possible to clarify the final point on which factors are missing and then incorporate them into the model, Cochrane might improve the horizontal portability of the consumption based asset pricing model. But if a general model makes too general claims, and a specific model makes to specific claims, what does model in between the two extreme really tell us? Cartwright warns that we should not raise our expectations too high.

To their defence, however, it can be said that the analytically convenient special cases are rigorously connected to the central pricing formula and so internally consistent. The hierarchy of models, therefore, creates an asset pricing theory "whole" that can guide the users in a plethora of situations. Seeking even more refinements and lower level granularity of the analytically convenient cases is achieved by using the auxiliary, belt assumptions to "correct" the predictions of the fundamental, core, principles, when they act on their own and there are many disturbing causes at work in the real situations. In this way, the cases can be fitted onto at least some very specific real situations. Cochrane thus shifts the theory away from the more general starting point. What he ends up with, however, is the de-idealized and de-fictionalized leaf at the tip of a branch while the trunk of the tree is still not well understood. But these leafs might in some cases provide attractive insights and even an accurate and reliable understanding of those very specific situations. I am here, for example, thinking of the "risk corrected" required return type of derivation Cochrane exposed us to, or even Fama and French's assertion that small, undervalued stocks have done better in terms of return than their larger, overvalued peers (see *Chapter Two, section 1.3*). Nonetheless, as I have pointed out elsewhere, the understanding of such very specific situations is, in some, cases rather trivial and raises the questions how we can use this outcome to improve our understanding of the trunk of the three.

Below, in the next section, I continue to explore the representational role and use of the analytical cases. Now is a good time to invite a clear philosophy of science view of the situation.

3.2 Hausman's "conceptual explorations"

Daniel M. Hausman, whom I introduced in the context of my discussion on asset pricing assumptions in *Chapter Three, section 1.3.3*, also has a view on the type of "developments and derivations" that economists embark on in their research efforts. Hausman claims that economists:

> "...are merely constructing concepts and employing mathematics and logic to explore further properties which are implied by the definitions they have offered. Such model building and theorem proving does not presuppose that one believes that the particular model is of any use in understanding the world. (...) Insofar as one is only working with a model, one's efforts are purely conceptual or mathematical. One is only developing a complicated concept or definition." (Hausman 1992, p. 79).

In this extract, Hausman makes two main observations. *First*, he claims that economists develop models that are primarily useful for mathematical and logical explorations within a set of definitions: "That is, they may sometimes wish to investigate the properties of models without worrying about whether those models depict or apply to any aspect of the world." (Hausman 1992, p. 79). *Second*, economists can do so with little concern for the real situations - with a disclaimer that model-based claims can be compared with the real situations: "Their point lies in the conceptual explorations and in providing the conceptual means for making claims that can be tested and can be said to be true or false." (Hausman 1992, p. 78).

Hausman, therefore, wants us to believe that economists, first and foremost, see models as "definitions of hypothetical economies or markets". These definitions do not necessarily have anything to do with the real situations. It would be a "category mistake", Hausman tells us, to ask if the models economists develop are true or false, or even test them. Models that economists develop are, as a consequence, neither true nor false or just trivially true, Hausman explains. They are used for “conceptual explorations” only. Yet, at the end of the “explorations”, the model can be applied to make claims regarding the real situations. It is therefore not the model itself, but its claims that are compared with the real situations. These claims about the empirical world are, as a consequence, either accurate or not.

At a first glance, there are several commonalities between the way Hausman explains how he thinks economists “construct concepts and employ mathematics” to develop models, and how Cochrane develops the analytically convenient specific cases. What immediately stands out is that Cochrane, as Hausman correctly assumes, starts from some fundamental, core, principles, and develops more specific cases. For example, by claiming that investors are rational, self-interested and risk averse, Cochrane has said nothing related to the domain in which these characteristics can be applied. Only the triad of temperament is portrayed. Cochrane then offers more. He situates these sentiments in a financial market context and provides financial assets to the investor. The investor is next asked to make decisions concerning consumption and investments, and, in addition, Cochrane informs us, he should be mindful with respect to keeping his/her consumption at a stable level. In this way, Cochrane introduces and constructs “concepts” and mathematically explores, inside the model, further properties and the consequences of the fundamental, core, principles. This type of activity has merits on its own. It certainly led to the “discovery” and developments of the consumption based asset pricing theory in Lucas (1978) and the capital asset pricing model in Sharpe (1964).

Besides common themes as the one alluded to above, Hausman and Cochrane seem to disagree on one main thing. While Hausman maintains that models are for “conceptual explorations”, and not for “empirical theorizing”, Cochrane, I believe, thinks differently. He is certainly interested in Hausman’s “explorations” but his emphasis is on “empirical theorizing”. That is visible in the way the fundamental, core, concepts are mathematically extended with the help of the auxiliary, belt, assumptions. And in this theoretical field, Cochrane has left the profession of financial economists baffled and impressed by his dexterity. However, Cochrane wants more out of his pricing model than “conceptual explorations”:

> “The absolute approach is most common in academic settings, in which we use asset pricing theory positively to give an economic explanation for why prices are what they are, and to predict how prices might change if policy or economic structure is changed.” (Cochrane 2005, p.xiv).

Cochrane leaves no doubt that the “M” in CCAPM is developed to “explain” and “predict” asset prices and their behaviour. Ultimately, it is less related to an extension of theoretical concepts and an effort to improve our understanding of the internal logic portrayed in the specific analytical cases. I believe Cochrane’s asset pricing research project must be considered much richer than a just a “story” attached to a “structure” as Gibbard and Varian alluded to in their model definition or plainly “conceptual explorations” as Hausman suggests. Cochrane wants to tell us something with respect to the real world and his CCAPM aspires to provide knowledge for use in that world.

In light of the commonalities and the differences, I suggest a compromise. I think Hausman’s notion of a “model” and Cochrane’s “analytical cases” are just local stops on the way to comparing the model-based claims against the real situation data. This comparison also connects to my earlier references to Friedman in *section 1.2.4* above, and his view that the model has a dual role, both in theorizing and in fitting to real situations. Hausman tells us that it is not the model itself but the claims that the model makes that are tested against the empirical situations. Cochrane, I believe, does not foresee that the analytical cases are immediately applied to the empirical situations and tested. Nonetheless, Cochrane seems to develop the “M” in CCAPM to a degree that it is as “similar” as possible to the real situation so that they can be directly compared. This happens when the mathematical terms are fully specified, i.e. the letters in the analytical cases are interpreted and directly linked to specific time series of observable data. But this decisive step has not yet been taken. Before we let Cochrane take us there in the next section, let me sum up.

So far we have been concerned with the analytical cases and their derivations. We found that fundamental, core, principles and auxiliary, belt, assumptions combine to develop these cases within a rigorous mathematical setting. I do not consider this strategy to be wrong and have little ground for opposing it. This is, as I pointed out, well within the heuristic of what Lakatos calls a research programme. Nonetheless, deductive mathematical reasoning creates challenges for Cochrane. The fallacy of simplification, which I introduced in *Chapter Three, section 1.4*, is not only active at a theoretical level, but also in situations in which these theories are made explicit in a mathematical structure of specific analytical cases. It shows how simplifications originating in an a priori environment of hard to justify fundamental, core, principles, leads to unintended consequences in the form of unsuccessful empirical tests. This is, of course, regrettable. I think these results have little prospect of improving when financial economists remove themselves from the real situations by developing increasingly hypothetical and constructed assumptions regarding investors, markets and assets. It might be a good time to de-idealize or de-construct such deductive systems by re-visiting the classic writers on the subject, i.e. Smith and Mill who advocates insights anchored in observation of real situations and introspection. Nonetheless, given my interpretation of Hausman, Cochrane might resist such a recommendation.

I will, for now, leave the discussion of the analytically convenient cases and turn to the second part of Cochrane's statement I presented up front at the beginning of this chapter. It is related to Cochrane's assertion that the consumption based asset pricing theories are used not only to derive analytically convenient special cases but also the empirically useful representation. With respect to these representations, I will assess how they are made to represent, and what it is they really represent. This is the topic for the next section.

4. Empirical representations of analytical cases

In the previous *section 3*, we learn that the model may have several representational roles; it can represent a "selected part of the world" or it can represent "theory" – or both. I have thus far focused on how the analytically convenient cases can be characterized as representations of the consumption based asset pricing theory. In this section, I change track and assess how the analytical cases are transformed into empirically useful representations. The goal of this transformation is, as Cochrane claims, to establish empirically useful representation.

Consider, first an extract from Varian (2006). Almost thirty years after Gibbard and Varian (1978), he has not changed his views on modelling:

> "Economics proceeds by developing models of social phenomena. By a model, we mean a simplified representation of reality. The emphasis here is on the word "simple". Think about how useless a map on a one-to-one scale would be. The same is true of an economic model that attempts to describe every aspect of reality. A model's power stems from the elimination of irrelevant detail, which allows the economist to focus on the essential features of the economic reality he or she is attempting to understand." (Varian 2006, pp. 1).

Already in the preceding section, I showed how Gibbard and Varian's earlier views related to what a model can represent were shared, in many central aspects, by Frigg and Hartmann (2006, 2006a). Not only are the authors in-line with respect to models representing their theories, but they are also in agreement that models can represent the real situations as well. The idea that models might represent both its theory and the real situations were flagged by Friedman (1953), as I showed earlier in this chapter's *section 1.2.4*. This view is also shared by the four authors reviewed here. Since I covered the first or the two roles that models might have, i.e. theory representation I here turn to the second, i.e. how the analytical cases can represent the empirical.

I think it is important to state that the empirical representations that Cochrane alludes to do not represent a second set of cases separate from the analytically convenient ones. In fact, I argue that empirically useful representations are identified when the analytical cases have been properly predicated and connected with appropriate empirical data. The empirical representations are thus proper extensions of the analytical cases towards the real situations. I let my assessment be informed primarily by Frigg and Hartmann (2006, 2006a), Morgan (2001, 2006, 2008, 2008a 2009) as well as Hausman (1992) and Cartwright (1999).

I argue that the analytical cases in many instances cannot properly be connected with the real situations. The reasons are provided *first* and foremost by their idealized and fictional character that makes them unsuitable for empirical representations. *Second*, despite the fact that Cochrane develops the CCAPM within a recognized "hypothetico-deductive" account (see *section 4.3*) of theory appraisal, he still relies on *stories* to informally de-idealize and connect his theories to the world. *Third*, even in situations where a connection could have been established, the correspondence rules as Friedman calls them, or the bridge-principles in Cartwright's terminology, raise serious methodological obstacles.

As a consequence, the analytical cases are mostly not able to extend their reach beyond the theoretical and into the empirical. The models' ability to render knowledge for use is therefore threatened. In *section 4.4*, I discuss some routes Cochrane can explore in order to make the analytical cases provide better empirical representations.

4.1 Analytical cases and real situations

On the face of it, Cochrane's script for the consumption based capital asset pricing research programme seems to be close to what we know and observe in our daily life of social interactions. We acknowledge that there are financial markets, investment opportunities, and decision making consumers. Investors certainly reap satisfaction from being successful in buying the stocks which generate positive returns in "bad" economic states. This supports an increasing level of wealth, and their total well-being might improve as well. Consumers think ahead and save for future purchases of goods and services. Some of them are risk takers while others are not.

From this perspective we get the impression that Cochrane is good at representing this peculiar finance related corner of our social world and in capturing its interactions. He uses every-day concepts to describe complex processes and interactions. This intuitive narrative of individuals and their decisions, the financial market structure and the financial assets, therefore, makes sense. In *sections 3*, I described how Cochrane makes the *story* explicit in a structure of equations referred to as convenient analytical cases and how these cases extend towards the real situations. I am here interested in how Cochrane bridges the gap between the explicitly formulated mathematical structure and the real world. This is the topic for the next section.

4.2 Cochrane "works with theory"

In this section, I consider how Cochrane envisages what he suggested, i.e. "working with theory" as alluded to in *section 3* above, to establish a link between the mathematical expressions in the analytical cases and the empirical world. Even more concretely, Cochrane seeks ways for the asset pricing research programme to go from "marginal utility to "observable indicators". We also know that his analytical cases, which I consider being detailed, idealized and fictional applied mathematical models merely reflect the axiomatic starting point in a certain logical and hierarchical way. Internal consistency, therefore, is assumed to be assured. Finally, I established that most of the analytical cases have not been specified to the degree necessary to make them represent empirical situations. In their current form, therefore, the analytical cases do not reach all the way "down" to the empirical. Something is missing.

As a starting point consider Cochrane's analytical case regarding the risk correction or, alternatively, the expected excess return as described in, respectively, *sections 2* and *3* earlier. We recall from *section 2.1* that the risk correction is given by:

$$p_t = \frac{E(x_t)}{R^f} + \frac{cov\left[\beta\, u'(c_{t+1}),(x_{t+1})\right]}{u'(c_t)}$$

And from *section 2.2* we get the expected excess return:

$$E(R^e_{t+1}) = -\,R^f \operatorname{cov}\left[\beta \left(\frac{c_{t+1}}{c_t}\right)^{-\gamma}, R^e_{t+1}\right]$$

We also recall Cochrane's statement that the analytical cases, such as the two equations here, do not represent the target situation particularly well. This is intriguing. On one hand, Cochrane claims that formulas such as the risk correction and the expected excess return are "useful", but, on the other hand, they do not "represent" well. This raises the question why, in his view, useful empirical representations fail to give us reliable knowledge? To answer this question, let me review Cochrane's translation from the fundamental, core, principles to the empirical situations in some more detail. The purpose is as stated above; identify paths taken from the theoretical to the empirical, and point to possible obstacles that hinder a successful translation.

Cochrane interprets the mathematical terms in the risk corrected and the excess return equations shown above by predicated them. For example, R^f is the risk free interest rate, the cov is a statistical co-variance matrix, and the parameter γ is the representative investor's risk aversion. We therefore get the impression that these interpretations have found their real situation counterparts. We know, for example, what a risk free interest rate is and where to look for it in the readily available statistical time series. Then, we can subtract the risk free rate from the time series of historical equity returns to find the so-called equity risk premium. This premium is then important for the risk correction because it indicates the required pick-up an investor would demand for shifting his/her savings into a portfolio of publically traded stocks. Since we also know where to find the per capita consumption data we are getting closer to establishing the co-variance matrix. All we need are, again, some time series data, their statistical correlations and volatilities. Finally, the risk aversion parameter can be statistically estimated or, if necessary, given an adequate value, i.e. calibrated. But hold on. Are these examples given not of very different natures? I think the answer is yes.

4.2.1 Category one cases

Most challenging, I believe, is that several of the mathematical terms in the equations are neither observable nor do they have direct empirical counterparts. I am specifically thinking about the stochastic discount factor (SDF) and its two elements, i.e. the "utility function" and "subjective discount factor". Even deeper inside the utility function, for example, we find the sentiments of rationality, self-interest and risk aversion. None of these fundamental, core principles are directly observable. Additionally, Cochrane is often targeting next period's realisation, i.e. (t+1). The future values of the risk free rate and the consumption data are, of course, unknown at the current time (t).

The point I want to make here is that Cochrane's commitment to the development of empirically useful representations from the analytical cases meets obstacles. They are mainly related to interpretation of the equations' functional arguments. While some of these arguments contain elements that have direct empirical counterparts, others do not. For example, the risk free rate has but the risk aversion parameter does not. Let me, therefore, suggest that we distinguish between two categories of analytical cases. The first category of cases provides arguments that can be empirically identified. These cases must be considered what Cochrane claims to be "useful". I am in particular thinking about some of the classic cases in finance that Cochrane lists such as the risk free interest rate. These analytical cases have in common that they do not carry in them assumptions regarding the representative investor, his/her character and behaviour. They are based upon descriptive statistical results that are extractable directly from the time- or cross-sectional data. [48] Let me call analytical cases that do not contain the fundamental, core, principles *category one* cases.

The category one analytical cases, however, face two challenges. One is related to measurement, and the other is concerning their importance. It is clear that the analytical cases, such as the two equations shown above, do not themselves carry any indications of their predicates, their interconnectedness, and measurement. It is the financial economists, following the heuristics of the research programme, who decides on these issues. In order to reach the empirical level, Cochrane needs to make several steps. The *stories*, for example, of consumers' demand for recession-proof stocks leads to the so-called Arrow Securities or "contingent claims" inside the analytical cases. And from

[48] Such cases and their direct connection with the data are the area of specialisation of Eugene F. Fama and Kenneth R. French, who I discussed earlier in the context of the multi-factor pricing models in *Chapter Two, section 1.3*.

there to a specific stock with empirical names such as Google and Caterpillar. Finally, Cochrane would line up, for example, Caterpillar's daily stock prices between 19 August 2004 and 31 August 2013 as I showed in the graphical representation in *Chapter Two*. A similar process needs to be applied for the other functional arguments that can find an empirical counterpart. This is not complicated and the problems encountered are mostly measurement related.

The second challenge, beside the measurement topic, is the importance of the empirical concepts within the mathematical structure of the consumption based capital asset pricing research programme. The analytical case I have been focusing on the most, i.e. risk free interest rate, serves as a good example. I stated earlier that this concept is not an original part of the *basic pricing equation* that merely states that a "stock price equals expected discounted payoff". The discount rate, i.e. the risk free interest rate that Cochrane here mentions needs, however, to be explicitly defined. There is, of course, nothing in the fundamental, core, principle that would help him achieve this goal. Hence, the discount rate cannot be derived from the triad of temperament and the equilibrium condition. So Cochrane needs to look elsewhere. And he finds the concept of a risk free interest rate in standard micro-economic literature. This formula is then imported into a financial market structure. And this is my point with respect to the importance of the analytical cases that are empirically useful. They are trivial. In Cochrane's analysis of the two equations I referred to above, i.e. risk correction and expected excess return the risk free rate of return, for example, can be the well-known 3 month USD Libor rate. This rate is certainly needed to complete the specification of such analytical cases, but there is little to learn from it.

The case I describe here, i.e. the risk free interest rate, is not an isolated example. Consider, for example, the relationship that Cochrane establishes between the data distribution and the utility function. In this case, the statistical properties of a price-distribution are not the derived outcome of the first order condition. There is nothing in the first order condition that demands a particular data distribution. In fact, they are two very separate elements.

4.2.2 Category two cases

Beside this first category of analytical cases, I suggest there is a second. The second category contains arguments that cannot directly be assigned to their empirical counterparts. Hence, these cases cannot be linked to the real situations. They are thus less useful in providing the analytical cases their proper empirical representations. Their commonality is that they evolve around the fundamental, core, principles, i.e. the representative investor, his/her character and behaviour. The triad of temperaments, i.e. rationality, self-interest, and risk aversion cannot be identified by any specific time series of data. They are not directly observable. The representative agent, therefore, do not have any direct empirical counterpart. The same can be said with respect to his/her constrained maximizing behaviour. This behaviour, as we know, leads to the first order condition which is a prerequisite for the equilibrium market condition. This condition is not observable. It needs to be assumed.

Let me refer to such analytical cases as *category two* cases. While analytical cases in category one are challenged by their relevant identification and accurate measurements, category two cases face their own challenges. One such challenge is that they can neither be observed nor measured. This, however, is a common topic across various scientific projects. Another challenge is whether we trust the main arguments presented to us in the category two cases. I am in particular thinking of the fundamental, core, principles. Earlier, Mill reassured us that the "economic man's" character and behaviour could be established by observation and introspection. Cochrane, however, did not present such justification. His representative investor came fast and furious. Let us explore.

We know by now, as in described in *Chapter Three, section 3.2.3* that Cochrane's research method is deductive from an a priori starting point as formulated in the *central pricing formula*. Cochrane thus derives analytical cases that carry with them arguments that can be either directly connected to the empirical situations (category one) or not (category two). In the latter group of analytical cases, the arguments remain theoretical, and their content and meaning need to be assumed. My concern is here not so much related to the unobservable elements. They are part of most theories – social and natural sciences alike. I am more focused on the fundamental, core, principles and ask whether we can trust them.

Cochrane's research project is namely critically dependent upon the quality of the chosen fundamental principles, i.e. the triad of temperaments that lead towards the first order condition an equilibrium situation in the financial markets. I believe there are several reasons why we should doubt the validity and soundness of Cochrane's fundamental principles. Consider *first* how they are established. Mill tells us that they have been established "beyond doubt", they are "obviously" true and verifiable through introspection. In this sense, they build on inductive "evidence". Inductive evidence, as we know, is considered to be weak evidence. Subjective awareness is no real basis for generalisation, of course, and gives a fragile, or even, false foundation.

Next, Cochrane's CCAPM has so far failed to explain and predict observable asset prices and their behaviour. Cochrane is the first to admit this and points out to us that: "Unfortunately, this specification of the consumption based model does not work very well." [*this motivates the*] "...exploration of different utility functions, general equilibrium models, and linear factor models such as CAPM, APT and ICAPM as ways to circumvent the empirical difficulties of the consumption based model." (Cochrane 2005, p. 43). Here, we see Cochrane's doubts with respect to the quality of the fundamental principles he uses in his asset pricing research approach. From what we read, it even seems that Cochrane is ready to abandon the theorizing concerning the representative investor and revert to the statistical time series analysis offered by the alternative models (see *Chapter Two, section 1.3*). Nonetheless, it is difficult to reject Cochrane's hypothesis that stock prices move and that investors through their actions influence these changes through their actions. Maybe stock prices are indeed discounted cash-flows, and that they fluctuate randomly around a preconditioned idea that the level of the equity risk premium should be, as an accepted rule of thumb, 6% (I discuss this number in the following *Chapter Five*). So far, however, the CCAPM has failed to confirm Cochrane's suggestion.

Finally, can it be that the financial markets and their activities do not render themselves to the type of ordered analysis that Cochrane advocates? The representative investor is set up to reduce and not endorse heterogeneity. He/she is a "calculating machine" in a system that possible requires some quite different specifications of the individual, the market and assets. This was a main topic of mine in *Chapter Three, section 1.3*, and I will have more to say in *Chapter Five, section 4.2.*

There might, therefore, be something fundamentally wrong with Cochrane's approach. Nicholas Kaldor reminds us:

> "My basic objection to the theory of general equilibrium is not that it is abstract; all theory is abstract and must necessarily be so, since there can be no analysis without abstraction; but that it starts with the wrong kind of abstraction and, therefore, gives a misleading "paradigm" (...) of the world as it is: it gives a misleading impression of the nature and the manner of operation of economic forces." (Kaldor 1975, p. 347).

I think Kaldor addresses two main issues. The first is related to how the fundamental, core, principles are established and, second, how appropriate or sustainable they are for the tasks at hand. My concern, therefore, is that we cannot take too much comfort from the starting point of Cochrane's asset pricing theory. And if the starting point is wrong, the end-result is possibly wrong as well.

Where does this leave us? I remain sceptical towards Cochrane's ontological commitments towards the fundamental, core, principles. I acknowledge, however, the flexibility of the auxiliary, belt assumptions that may help Cochrane defend the fundamental, core, principles and develop analytical cases that zoom in on particular questions that the real situations may generate. Nonetheless, I believe Cochrane's references to the competing asset pricing theories are generous in the sense that he advocates "horses for courses", i.e. depending upon the situation and asset class, apply either the fundamental or the relative approach. This indicates that he is not completely locked into his position that requires representative investors to solve for asset prices.

Lakatos might cry foul at this stage because of the lack of commitment towards the core principles within a research programme while Cochrane would, possibly, argue for research flexibility and more pragmatism. After all, holding onto core propositions for the sake of it would indicate that science cannot err. Nonetheless, Cochrane and others should be reminded that they also switch between two highly different research programmes when "betting" on different horses. In the end, therefore, I side with Kaldor and his scepticism, i.e. the core is not to be trusted beyond a reasonable doubt. Cochrane's general statement with respect to taking the analytical cases directly to the market place must, as a consequence, be rejected.

Given my scepticism towards the inability of the category two analytically cases' ability to reach down to the real situations thereby making them empirically useful, I now examine whether Cochrane, in his research programme endeavours apply generally accepted methodological standards. The reason for this examination is twofold; *first*, it enables me to break the research programme into clearly identifiable steps, and *second*, it facilitates my search for reasons why the programme, so far, has failed to deliver knowledge for use to its stakeholders, i.e. investors, public sector entities and fellow academics.

4.3 A stylized HD account

I think there are good reasons why it is challenging for Cochrane to go directly from a theoretical concept such as "marginal utility' to a set of "observable indicators". There are certainly several intermediate steps that need to be taken before that can be the case. In the example I provided in the previous section in relation to the modelling of the risk correction and the expected excess return, I showed how Cochrane envisaged such a progression. At the end of that process, we would like to know whether Cochrane's theories are good or not and whether we can trust the knowledge he offers for the various stakeholders to use. Here, I am not focusing on the empirical evidence given in favour of his theories - that will be a discussion point in *Chapter Five*. I rather review how Cochrane's leads his theories to the empirical, i.e. the derivation of empirical representations. I will use a familiar view of theory appraisal in the natural and social sciences. It is referred to as the "hypothetico-deductive" account – HD for short.

The HD account for theory appraisal can be formulated succinctly as follows (Hausman 1992, p. 123):

1. *Formulate* a hypothesis
2. *Deduce* a prediction from the hypothesis and other statements
3. *Test* the predictions
4. *Evaluate* the hypothesis on the basis of the test results.

In this stylized four step process, we first observe that the hypothesis is not undergoing a direct empirical test. That role is reserved for the prediction. The prediction is formulated from the "hypothesis and other statements". To understand the reason for making these two steps, we would need to do some archaeology related to the "syntactic" account of theories. Space does not permit an elaborate explanation and relevant literature can be found elsewhere. [49] Yet, the short comment in this context is that advocates of the syntactic view believe that theories are typically related to processes or mechanisms we cannot directly compare with the empirical situations by observation or experiment.

Alexander Rosenberg, for example, explains:

> "The axiomatic approach [*also referred to by Rosenberg as the "syntactic" or HD account of theories*] begins with the notion that theories are, (...), axiomatic systems in which the explanation of empirical generalizations proceeds by derivation or logical deduction from axioms (...). Because the axioms – the underived laws fundamental to the theory – usually describe an unobservable underlying mechanism (...) they cannot be directly tested by any observation or experiment. These underived axioms are treated as hypotheses *indirectly* confirmed by the empirical laws derivable from them, which can be directly tested by experiment or observation. It is from these two ideas, that the foundation of a theory is hypotheses supported by the consequences deduced from them, the name hypothetico-deductive models derives." (Rosenberg 2000, p. 76).

[49] da Costa and French (2000, 2003), Rosenberg (2000), Suppe (2000)

Rosenberg tells us that the HD account acknowledges that theories are axiomatic systems that cannot directly be tested. Hence, testable predictions must be deduced from the axioms. If the predictions are confirmed in empirical tests, then there are good reasons to believe the hypotheses arising from the theories. This is referred to as an *indirect* confirmation of theories. Rosenberg goes on to a critical assessment of the HD account. Again, I will leave this, elaborate, discussion aside and focus on the asset pricing project. Is the HD method relevant for Cochrane's own procedures for taking the theories to the test? In some aspects it is, but in others it is not. The starting point is the hypotheses.

Proponents of the HD account are fairly generous in the answer as to the origin of the hypotheses. Nonetheless, a common starting point is found in theories with known empirical or pragmatic virtues. Rosenberg, for example, refers to axiomatic systems such as the "kinetic theory of gases", "theory regarding the molecular structure of the gene" or "Newton's three laws of motion". Does Cochrane make reference to similar systems of theories in his asset pricing approach? Does he use them in formulating "hypotheses"? The short answer is a possible "yes".

Cochrane uses a few fundamental "core" principles to get the ball rolling. His most dominant are as we know the principles that lead to the development of the utility theory. Therein we find, for example, the various "axioms of rational choice", and how they formally show how individuals rank all possible situations from the least to the most desirable. Add uncertain outcomes, and we are in the von Neumann and Morgenstern "choice-arena" and the expected utility theorems. It is challenging to defend the assertion that these theorems have the same status as the axioms and theories alluded to above by Rosenberg in the field of natural sciences. Cochrane's departure point is a tentative one that is established as Mill reminded us, by inductive "evidence" and "introspection". Nonetheless, they could be viewed as a launch pad akin to step one in the HD schemata.

Cochrane suggests above that theories are used to "go from marginal utility to observable indicators". This would indicate that the triad of temperaments is not directly observable or testable. They are part of the axiomatic marginal utility theory. Cartwright, Giere, Frigg and Hartman have also suggested "rationality" is one of the few "fundamental principles" used in the development of theories and models in economics. Both Cartwright and Giere agree that fundamental principles, at least in the natural sciences, do not make any empirical claims. Giere explains:

> "I think it is best not to regard principles themselves as vehicles for making empirical claims. Newton's three laws of motion, for example, refer to quantities called force and mass, and relate these quantities previously well-understood: The Law makes references to terms like force, mass and velocity. However, they do not themselves tell us in more specific terms what might count as a force or a mass. So we do not know where in the world to look to whether or not the laws apply." (Giere 2004, p. 745).

In addition, Cartwright says that the fundamental principles are neither empirical nor modalized generalizations, i.e. neither universally nor by necessity do they hold. Nonetheless, Cartwright would claim that the fundamental principles, although not ripe for a comparison with the real situations, are true when used inside models. Principles, therefore, are true within models in cases where they are understood as definitions. Definitions are thus trivially true. Or as Giere puts it: "...if understood as universal generalizations, the resulting statements [*from the fundamental principles*] are either vacuously true, or else false and known to be so." (Giere 2004, p. 745). The philosophers of science are here backing up Cochrane's statement that he needs to find "observable indicators" for his "marginal utility" theory because utilities are not directly observable. So far, therefore, Cochrane is within the context of the HD account.

Consider, next, how Cochrane formulates a hypothesis from his theory of asset pricing: "Asset pricing theory tries to understand the prices or values of claims to uncertain payments. (...) Asset pricing theory all stems from one simple concept, (...) price equals expected discounted payoff." (Cochrane 2005, p. xiii). Cochrane here tells us that a statement from the theory can be regarded as a hypothesis. The hypothesis is, for example, that investors are rational, self-interested and risk averse, or that asset prices equals "expected discounted payoffs". As Cochrane works his way into point 3 in the HD account, i.e. towards testing predictions from the hypothesis, he adds several other assumptions related to the investor and his behaviour, the financial market structure, and the financial assets, i.e. Hausman's "other statements". In line with Lakatos, I would characterize these theory enriching assumptions as the auxiliary, belt, assumptions. Also on step 2, therefore, I can agree that Cochrane, bar some practical impediments, follow the HD schemata.

Nonetheless, point 2 of the stylized HD account, i.e. deduce predictions, need more analysis. We have thus far only arrived at the "analytical cases" and they are still a step away from being "empirical representations", i.e. predictions or scientific claims. As indicated in the quote above, Cochrane wants to "work with theory" all the way down to its applications. This also involves empirical test of the predictions, i.e. step 3 in the HD account and its evaluations, i.e. step 4. These two final steps are discussed in *Chapter Five*. Before we get there, however, I need to spend more time on the second step, i.e. deduce a prediction from the hypothesis.

4.4 Stories and other bridges to the empirical

I demonstrated that Cochrane is well within the standard methodological framework in terms of theory appraisal. As a follower of the stylized HD account (see *section 4.3*), however, he meets challenges. I am in particular thinking of the way Cochrane seeks to transform the analytically convenient special cases to become empirically useful representations. Below I assert that when Cochrane's mathematical dexterity ends, he lets *stories* take over. In other words, a narrative steps in to plug the gap between model-based entities and their empirical counterparts. I think it is questionable to let *stories* perform this important task as they go beyond what the model's mathematical expressions support. Let us, once again, revisit one such *story*.

4.4.1 Cochrane's story

By now, we know Cochrane's story about the representative investor, his/her preferences and the choices he/she faces (see *Chapter Two, section 2.2* and *Chapter Four, section*

1.2.2). Additionally, we have been exposed to the mathematical asset pricing model and its many arguments. The story and the structure, as Gibbard and Varian point out, combine to offer an explanation of real phenomena (see *section 3.1*). But what is the systematic question that stories address? I will argue that Cochrane uses *stories* to connect the mathematical model to the real world. The *stories* thus inform and entertain beyond the mathematical structure. But they cannot replace the model's own reach towards the empirical situations. In particular, *stories* cannot be used effectively as connecting devices or middleware between the analytical convenient special cases and the empirical situations because they lack theoretical and methodological rigour.

Morgan (2001a) reminds us that philosophers of science often have analysed the relationship between the theories and their models. Economists, however, seem to be more concerned with the relationship between the models and the real situations. In the former relationship, logic, deductions and mathematical derivations play an important role. It is all about "theorizing". We recall Hausman's (1992) view that economists engage in "conceptual explorations" (see above *section 3.3*):

> A theory must identify regularities in the world. But science does not proceed primarily by spotting correlations among various known properties of things. An absolutely crucial step is constructing new concepts – new ways of classifying and describing phenomena. Much of scientific theorizing consists of developing and thinking about such new concept, relating them to other concepts and exploring their implications." (Hausman 1992, p. 13).

An excellent example of what Hausman here describes, i.e. conceptual innovations is Cochrane's (in cooperation with Campbell) answer to the equity risk premium puzzle as introduced in *Chapter Two, section 6.3.4*. There, I showed how they developed the concept of "habit-persistency" and integrated it into the consumption based utility function. (In the next *Chapter Five*, I will have more to say about this topic.)

Nonetheless, I also argued that Cochrane wanted more than just "conceptual explorations" across theoretical entities. And this is where his narrative plays an important role. *Stories*, Morgan claims, are either *stories* about the model world or *stories* about the past, present or the future real world. Morgan (2008) explains that economists' mathematical models are "...designed only to *approximate* the world, and, unlike econometric models which go through a serious process of fitting to the world, they are casually connected to the world by "stories" which interpret the terms in the model to elements in the world". Morgan here wants to draw a distinct line between mathematical models used for "theorizing" and econometric models used for "fitting theories to the world". Theorists' explorations are thus contrasted with econometricians' data analysis.

In the context of my discussion of Cochrane's version of the consumption based asset pricing research programme, I think we can see elements of both theorizing and conceptual explorations on one hand, and the desire, on the other, to fit his theories to the world. As a consequence, mathematical structures, *stories* and real situation data interact. In this respect, Cochrane's research programme cannot be fitted entirely into one of the categories that Morgan develops. Cochrane spans both of them.

The aim of his endeavour however, is to develop knowledge for use. Is Cochrane successful? I have already, and will continue to point out, that the empirical tests of Cochrane's model-based claims, so far, have been unsuccessful in providing adequate predictions. Nonetheless, the mathematical structures are impeccable and the *stories* he tells about them are intuitively convincing. So at least on those two counts, Cochrane is convincing. The concern I have in this context, however, is the dominant role of *stories* in Cochrane's set-up. On one hand, Cochrane predicated the mathematical terms of the model by giving the parameters in the utility functions names such as subjective discount factor and risk aversion. But he extends the theoretical interpretations to tell *stories* about these parameters. For example, he tells us why investors are risk averse. On the other hand, Cochrane uses *stories* to tell us how these elements connect to the real situations. For example, he says that such risk averse investors seek recession proof stocks. Here, Cochrane refers to the everyday concept of financial market assets, i.e. stocks. In this regard Cochrane's narrative is portrayed at two interconnected levels; the *first story* is told within the model, and a *second story* is told with respect to real situations outside the model.

I see two challenges. *First*, the *stories* told within the model are interpretations of parameters and the derived, theoretical, results. The mathematic ensures that these results are internally consistent. But can we say the same thing about their interpretations? It is certainly challenging to develop the same type of precision and accuracy within a narrative structure. For example, we know from *Chapter Two, section 3.5* that Cochrane, and other financial economists, have a preference for using the power utility function in their theorizing due to its mathematical properties. These properties are used to develop the concept of the risk averse investor. But we know that the investor can also be modelled as risk neutral or risk seeking. In these cases, different *stories* must be told.

My *second* point is related to Cochrane's *stories* that reach outside the model. In this respect, Cochrane uses the *central asset pricing formula* to fit the consumption based asset pricing research programme to the world. Such models are no longer theoretical but applied econometric models. They explain some observed facts. But how does the model's mathematical structure give rise to Cochrane's *stories*? There is, for example, nothing in the *central asset pricing formula*'s payoff notation, i.e. $\mathbf{x_{t+1}}$ that tells us that these future, expected, cash-flows stem from recession proof stocks. The *story* Cochrane tells, therefore, goes beyond what the mathematical structure can reveal. Morgan explains:

> "We can only ask questions and tell stories about terms and relations that are represented in the structure and only within the range allowed by the mathematics or material of the structure". (Morgan 2001a, p. 369).

I think the consequence for Cochrane's double narrative is twofold. The first level narrative tells *stories* inside models. Such *stories* are used for theorizing. They are explorative in nature and might lead to new classifications and innovative concept as exemplified by the "habit-persistency' argument placed within the representative investor's utility function. On this first count, Cochrane is on safe ground. The second level narrative tells *stories* outside models. Here, the derived consequences of the theorizing are compared with the real situations. Cochrane explores, for example, to which extent the representative investor is risk averse or how much recession protection can be found in the time series of excess returns. On this count, Cochrane needs to be cautious. On one hand, empirical tests of the consumption based programme are not overly successful. On the other hand, narratives extend beyond their fundamental, core, principles and the auxiliary, belt, assumptions. They tend to take a life on their own. Practitioners should, therefore, be warned not to dwell too long on *stories* about the real world but to pay more attention to their lack of empirical success.

Given the challenges Cochrane faces with respect to rendering plausible narratives when linking the analytical cases to the real situations, can we think of alternative ways for him to reach his/her stated goal? The answer to this question takes me to the next section related to connectors and bridges.

4.4.2 Friedman's connectors

We recall from *section 1.2.4* above that Friedman, as well, is concerned with the way theories are connected to the empirical. In particular, he mentioned the importance of "specifying the correspondence between the variable or entities in the model and the observable phenomena". I think there are two ways of thinking of his comments. On one extreme, as we have seen being preferred by Cochrane, the variables in the model are linked casually with the real situations. This is the story-telling account. On the other extreme, given the weaknesses of using *stories* as connectors, we find econometric models that seek a rigorous connection between the two. Morgan informs us:

> "...econometricians' arguments about model derivation and selection, along with their battery of statistical tests, are really all about how to get a correspondence via models in fitting theory to the world: one might even say that econometrics could be broadly described as a project developing the theory and practices of correspondence rules for economics." (Morgan 2008a, p. 32).

In its early historical developments, models were used: "...as vehicles for bridging the gap between theories of the business cycle and specific (time and place) statistical data of the cycle." (Morgan 2008, p. 3). Positioning the model in between theory and empirical, however, does not solve the topic of correspondence rules that Friedman is concerned with. Morgan tells us: “Developing correspondence rules has formed one of the major difficulties for economists seeking to defend the method of modelling, and for philosophers and methodologists seeking to account for the work done by economic models.” (Morgan 2008a, p. 32). I will not be able to contribute to this debate in the context of my thesis. [50]

4.4.3 Hempel’s bridges and Cartwright’s two models

We have seen that Cochrane struggles to connect his analytical cases to the target system, i.e. real situations when seeking to develop empirically useful representations. So far, Cochrane lets intuitive, causal and even informal *stories* and ad-hoc translations dominate where mathematically derived representations fail. Gibbard and Varian, we recall, were also retreating to casual *stories* in such circumstances. Is there another strategy? Philosophers of science might answer this question with a “yes” and refer to the so-called “bridge-principles” But what are they, and is there room for them in Cochrane’s research programme?

Drawing on Hempel’s insights (1965, 1966), Cartwright (1999) tells us that there are two groups of principles. As I indicated earlier in *Chapter Three, section 2.1*, the first group has many names; theoretical-, internal-, first- or *fundamental-principles*. The second is called bridge-laws or *bridge-principles*. Since I dealt with the fundamental principles in the previous chapter, I now briefly turn to the use of bridge principles. Bridge-principles were initially introduced to cover the distance between what was perceived as “theoretical-” and the “observational-terms”. Carl G. Hempel says that the bridge-principle: “…will indicate how the processes envisaged by the theory are related to the empirical phenomena with which we are already acquainted, and which the theory may explain, predict or retrodict.” (Hempel, 1966, pp. 62). Strict procedural rules should, therefore, be in place to define how to connect theory with the empirical reality.[51] The

[50] There is, of course, a rich literature on how modelling can be viewed as fitting theories to the world (Morgan 2008).
[51] From the previous *section 4.4.2*, intuition and creativity are in more demand than strict procedural rules.

bridge-principles should give the theoretical concepts meaning, i.e. they interpret the theory.

Cartwright tells us that bridge principles are essential – at least in physics:

> "...physics needs bridge principles because a large number of its most important descriptive terms do not apply to the world directly; rather, they function as descriptive terms.(...) The *quantum Hamiltonian*, the classical *force function*... are all abstract. Whenever they apply, there is always some more concrete description that also applies, and that constitutes what the abstract concept amounts to in the given case. Mass, charge, acceleration, distance, and the quantum state are not abstract. " (Cartwright 1999, p. 189).

Cartwright, here, gives a clear account for a process by which an abstract term turns more concrete. In fact, this process makes two steps. The first step deals with the de-abstraction of the term "force" as a traditional mechanical concept to another, more concrete term, such as "mass", position", 'extension", "motion". Cartwright tells us that there are specific rules for how to fill in the force variable with a more specific force function and that these rules are given by the bridge principles. She calls the outcome of this process an "interpretative model". The second step in the process is taken when a specific prediction of a real situation is requested. It is then necessary, Cartwright claims, to specify a particular value for the force, "such as 10 dynes". (Cartwright 2008, p. 134). If one, therefore, applies a bridge principle to a fundamental principle, one ends up with a version of the fundamental principle that has real empirical content. In this sense, Cartwright's bridge principle gives an empirical translation of a theoretical term in a model, and the result is that the model can represent more easily. Cartwright calls these second-step models "representative models":

> "The first are models that we construct with the aid of theory to represent real arrangements and affairs that take place in the world – or could do so under the right circumstances. I call these *representative models*. (...) Theories in physics do not generally represent what happens in the world; only models represent in this way, and the models that do so are not already part of any theory. (...) When we want to represent what happens in these situations [*very specific kinds of situations*] we will need to go beyond theory and build a model, a *representative model*. (...) This

> is an old thesis of mine. If we want to get things right, we shall have to improve on what theories tell us, each time, at the point of application." (Cartwright 1999, pp. 180).

Cartwright, here, makes several points that are relevant for our discussion relating to fitting theories to the world, i.e. establish connections between the theoretical and the empirical. The main thrust is that scientists need to go beyond theory to build representative models that tell us what goes on in the world. [52] The reason for this is that theory "need not contain the resources to represent all the causes and effects in its prescribed domain". Theory, therefore, should not be regarded as a "vending machine", as Cartwright portrays it, in which prescribed input is fed, and desired output is generated:

> "The vending machine view is not true to the kind of effort that we know it takes in physics to get from theories to models that predict what reliably happens; and the hopes that it backs up for a shortcut to warranting a hypothesised model for a given case – just confirm theory and the models will be warranted automatically – is wildly fanciful. For years, we insisted theories have the form of vending machines because we wished for a way to mechanise the warrant for our predictions. (...) The first step beyond the vending machine view is various accounts that take the deductive consequences of a single theory as the ideal for building representative models but allow for some improvements, usually improvements that customized the general model produced by the theory to the special needs of the case at hand." (Cartwright 2009, pp. 185).

More than good theory is, therefore, needed if we want to account for real situations. And this is where the bridge principles and other assumptions enter. Representative models are thus constructed in a "co-operative effort" between the theory, bridge principles and auxiliary assumptions – often "borrowed" from outside the domain of the theory. Theories on their own, therefore, are insufficient when trying to reach all the way down to the real situations.

How do these elaborations on the bridge principles and the representative model resonate with Cochrane's set-up? In some areas better than in others, I will argue. I have already suggested that Cochrane's central pricing formula is an applied mathematical model. It is

[52] The theories Cartwright discusses here are related to the natural sciences, i.e. physics.

used to derive more specific models, i.e. the analytically convenient cases. There exists, in other words, a hierarchy of increasingly more granular analytical cases that are de-idealized and de-fictionalized relative to its general starting point with the help of the auxiliary, belt, assumptions. The question Cartwright answers with the help of the bridge principles is how to go from such abstract cases, first, to more concrete cases, and finally to the empirical representation. We thus need to identify which "translators" or "interpreters" Cochrane might use to help him bridge the gap.

Let me start with Hempel first before I turn to Cartwright's suggestions. Hempel requires that the theory should "envisage" a process for how it wants to relate itself to the empirical phenomena. This vision, I would argue, is not stated in the asset pricing theory itself, but is more a matter of the heuristics as followed by the practitioners within the consumption based capital asset pricing programme. In the previous *section 4.2*, I have indicated how Cochrane, as a leading advocate of this tradition, "works with theory" to take the theory downstream from the theory of "marginal utility to observable indicators".

Now to Cartwright. Her starting point is clear: "I begin from the assumption that it is the job of any good science to tell us how to predict what we can of the world as it comes and how to make the world, where we can, predictable in ways we want it to be." (Cartwright 1999, p. 181). In order to reach this goal, she suggests bringing our theories closer to the real situations, because "the fundamental principles of theories in physics do not generally represent what happens". Cartwright, therefore, sets out to work with what she calls interpretive and representative models. How does Cochrane's theory of asset pricing fit with Cartwright's set-up and what are the consequences if it does?

I think Cochrane shares Cartwright's vision of what science is about. I have given evidence for this previously when pointing toward his endeavour to explain and predict stock prices, excess returns and the equity risk premium. I have also shown how he starts in an axiomatic system of a priori principles and from there, deduce the implicit consequences. Such mathematical derivations, however, are supported by a host of auxiliary, belt, assumptions. With the support of such assumptions and the dexterity of his mathematics, Cochrane is able to take the asset pricing theory downstream towards the empirical situations.

Consider, for example, Cochrane's starting point in the representative investor. Cochrane needs to formulate an hypothesis that this individual can be described by reference to some behavioural instincts. These instincts are then spelled out and take the forms of rationality, self-interest and risk aversion. Risk aversion, for example, is more concrete than the term behavioural instinct. However, the term is not yet concrete enough to be applied to a specific choice situation. That situation needs to be defined by its boundaries. In this respect, Cochrane refers to a particular financial market structure and the choices to which the representative investor will be confronted. There are two; save and invest and which portfolio of assets to hold. In this confined situation, the risk aversion makes an entry into the utility function that Cochrane has chosen to be the best representation of the real choice situation, i.e. power utility. Even at this stage of more concrete terms and representation, Cochrane is still working with the confines of his mathematical structure. He has yet to make the leap across to the real situations.

As we can see from this cascading down of concepts, Cochrane is willing to engage in the way Cartwright discusses. The lower-level, specific analytical cases are connected to the more general fundamental, core, principles and enriched with auxiliary, belt, assumptions. As such, they are part of what Cochrane calls the theory of asset pricing. However, these models are not contained within the principle in a way that they are deduced results from these principles only. Along the way towards the empirical situations, they are enriched by specific conditions and their interpretations. These are plug-ins and not the derived outcome of the principles. As such, one could argue that the hierarchy of increasingly specific models, i.e. analytically convenient special cases, are similar, at least in spirit, to Cartwright's representative models that are assembled in a "co-operative" spirit. They are thus not the outcome of, as Cartwright warned us against, a "vending machine" process but genuine steps in the right direction, i.e. they take "the deductive consequences of a single theory as the ideal for building representative models but allow for some improvements", as I quoted Cartwright saying above.

Chapter 5:
CCAPM Application and Assessment

Introduction

In the last chapter of his textbook *Asset Pricing*, John H. Cochrane claims: “As the Campbell-Cochrane model is blatantly (and proudly) reverse-engineered to surmount (and here, to illustrate) the known pitfalls of the representative consumer models, the Constantinides-Duffie model is reverse-engineered to surmount the known pitfalls of idiosyncratic risk models.” (Cochrane 2005, p. 477). Only insightful students of financial economics are, off-hand able to make sense of this statement. But this is not the point to make here. I have two other topics in mind; the statement is extracted from the “Empirical Survey” part of this book, and it directs attention towards “reverse-engineering”. Both references are central topics in this chapter.

After a brief review of the empirical asset pricing literature, Cochrane, in his book, concludes that many observable asset prices are predictable. He, however, also highlights several data-puzzles or anomalies as Imre Lakatos called them. These are a frequently chosen terms when the empirical world does not behave in a way the model says it should. Cochrane is in particular alluding to the so-called “equity risk premium puzzle” first comprehensively documented by Mehra and Prescott (1985). The two authors demonstrate that the consumption based capital asset pricing model (CCAPM) cannot adequately explain why investments in “risky” stocks, over a long period of time, have returned so much more than investments in “riskless” government-guaranteed interest bearing assets.

This empirical puzzle, I claim, gives rise to what Cochrane above denotes reverse-engineering. The term refers to how Cochrane “blatantly” and “proudly” manipulates the CCAPM to make the puzzle go away. He is not alone in such a response. The reference made above to his financial economist colleagues George Constantinides and Darrell Duffie is a case in point. Neither is this reverse-engineering technique entirely new. Mary

S. Morgan reflects: “Understanding twentieth-century economics as a science in the mould of engineering is to see that the economic profession came to rely on a certain precision of representation of the economic world along with techniques of quantitative investigation and exact analysis alien to the experience of the nineteenth century economics when the extent of such technologies of representation, analysis and intervention were extremely limited.” (Morgan 2001, p. 3). Morgan here relates engineering to techniques, quantitative precision, exact analysis, and intervention to which Cochrane’s modern term can now be counted. The goals of Cochrane’s manipulations are to improve our understanding of asset price behaviour, solve practical problems and, having done so, offer policy advice. Reverse-engineering, therefore, is a means to reach that end because it works backwards from the data, seeking validation and, where necessary, improvements to the model and its underlying assumptions.

Building upon the two previous chapters on asset pricing theories (*Chapter Three*) and asset pricing models (*Chapter Four*), I now focus on the third central topic of my thesis, i.e. how Cochrane confronts his version of the consumption based capital asset pricing research programme with the accessible asset pricing data. In fact, both of the previous chapters lead into and build towards this final chapter. The *purpose* here is to examine how the CCAPM is applied and assessed. For that, I draw on the equity risk premium puzzle as a representative case study. The *goal* of this chapter is threefold; show that forecastability, initially, is the accepted “litmus test” for assessing the progress of the CCAPM research programme, point towards the two main obstacles that the programme is confronted with, i.e. unrealistic assumptions and socio-economic complexities, and propose a solution for the programme to consider overcoming the double-trouble.

I *conclude* that Cochrane’s reverse-engineering approach falls within the heuristic of what Lakatos calls a research programme, and that the CCAPM programme has experienced modest theoretical and empirical progress. Since I believe financial market activities do not easily surrender to an a priori based deductive mathematical reasoning which seeks to capture event regularities in the time-series of asset prices, more of that sort could hinders rather than fosters its development. As a consequence, Cochrane, being the CCAPM programme's main promoter, is well advised to move away from the model-based "point-forecasts" approach towards making claims about *tendencies* with respect to empirical situations. This re-direction could replace Cochrane's own suggestion to reduce the importance of standard statistical tests in evaluating the model-based claims, and, in addition, offer a sound foundation for emitting knowledge for use to the various stakeholders.

The chapter has six sections: The *first* section defines the empirical research question related to the well-known equity risk premium puzzle and reflects on how, in general, financial economists have reacted to the fact that their models do not explain the data particularly well. From there, in section *two*, my interest turns towards Cochrane and how he reverse-engineers his asset pricing model in light of its disappointing statistical performance. His reaction is to add a "habit-persistence" variable to the standard utility function. I refer to this innovation as the "triad of temperament plus habit". This innovation modestly progresses the theoretical and empirical relevance of the pricing model. Nonetheless, building on Robert M. Solow and Nancy Cartwright, I discuss whether adding this auxiliary, belt, assumption leads to the "right kind" of model. In section *three*, I show how Cochrane, despite its statistical inadequacies, defends the CCAPM as the "correct" starting point for assessing the behaviour of asset prices. The correct endpoint for Cochrane's research effort can, initially, be found in the empirical tests, i.e. how well the model-based claims represent the real situations. Borrowing from Alexander Rosenberg, I refer to this as the "litmus-test".

In the *fourth* section, in view of the CCAPM's disappointing empirical success, I review two of the programme's main challenges; *first*, its use of unrealistic assumptions, and, *second*, the complexities of the empirical situations it seeks to explain and predict. The double challenge raises high hurdles for the programme's a priori, deductive mathematical approach. I am encouraged to advice Cochrane to de-emphasise "point-forecasting" and instead focus the programme on making claims about *tendencies*. The identification of *tendencies* is, however, complicated because the current methodological tool-set available to financial economists do not easily support their identification and extraction. My discussion is informed by John Sutton, John Dupré, Tony Lawson, Milton Friedman, and Nancy Cartwright.

In section *five*, I turn to what Cochrane calls "statistical philosophy". Following the disappointing model predictions against the real situation data, Cochrane unexpectedly claims that statistical success is not "the last and decisive" check to evaluate whether a theory is acceptable or not. These tests must be complemented with other metrices. Cochrane then explicitly tells us that model choice is made more on the basis of a "persuasive", "coherent", "interesting" and "clean" *story* than on a high statistical significance. I advise against this suggestion. Finally, in section *six*, I indicate that Cochrane cannot, at the same time, make predictability the litmus test for model-based claims, and then chose to ignore their rejections. Again drawing on Rosenberg (1994), I claim that Cochrane pays "lip-service" to Karl Popper.

1. The research question, complications and rejections

From *Chapter Two, section 6*, we recall that Mehra and Prescott (1985) found that the inflation adjusted, i.e. real total return on a portfolio of US stocks held from 1889 to 1978 provided investors, on average, almost 7 percent per annum. In the same period the annualized return on a three month risk free US Treasury Bill was around 1 percent. The positive average difference of 6 percentage points is the so-called "equity risk premium". This premium handsomely compensated the investors who, over the historical period under review, were willing to hold risky stocks instead of riskless T-Bills.

Mehra and Prescott (1985) thus formulated an empirical research question that has occupied almost a whole generation of financial economists: Why have investments in stocks issued by the private sector companies provided a higher return than investments in the public sector, government guaranteed fixed interest bonds? When Mehra and Prescott (1985) approach the question more thoroughly in the context of the standard Lucas (1978) consumption based capital asset pricing model similar to the one Cochrane promotes, they discovered that their intuition is right, but the model-based risk premium is much lower than what is observed in the data. In fact, CCAPM tells us that the premium should not have been the observed 6 percentage points, but closer to 0.4 percentage points per annum. The stock market has, in other words, performed too well relative to the risk free asset – or the return on the risk-free asset has been too low. Since Mehra and Prescott (1985) could not explain the large return difference between the two asset classes, they labelled it the *equity premium puzzle*.

Financial economists were challenged by these findings, and much research effort has been undertaken to make the puzzle go away. The effort has taken three avenues; review the data set, alter the theories and their assumptions, and, finally, make changes to the structure of the applied mathematical model (see *Chapter Two, section 6.3*).

2. Cochrane's response

Cochrane, as we know by now, is also disappointed by the statistical performance of his version of the CCAPM when it is confronted with the data. In his book *Asset Pricing* (2005), Cochrane acknowledges the equity premium puzzle and seeks ways around it:

> "We want to end up with a model that explains a high market Sharpe ratio, and the high level and volatility of stock returns, with low and relatively constant interest rates, roughly i.i.d. consumption growth with small volatility, and that explains the predictability of excess returns in the future. Eventually we would like the model to explain the predictability of bond and foreign exchange returns as well, time-varying volatility of stock returns, and it would be nice if in addition to fitting all of the facts, people in the models did not display unusually high aversion to wealth bets." (Cochrane 2005, p. 465).

These requirements are neither modest nor trivial. Nevertheless, at least Cochrane has a good idea of how to proceed in terms of developing a general asset pricing model. When encountering anomalies, as Imre Lakatos called them, Cochrane's answer is "reverse-engineering". Reverse-engineering works backwards from the data seeking validation and where necessary, improvements to the model and its underlying assumptions. Let me review Cochrane's response (see also *Chapter Two, section 6.3* for a more general discussion).

2.1 Habit-persistence

In *Chapter Two, section 6.3.3 and 6.3.4*, I described the development of the so-called Campbell-Cochrane habit-persistence version of the standard CCAPM (Campbell and Cochrane 1999). Let us briefly review the main topics.

Constantinides (1990) had argued that the consumer's utility is not only shaped by the trade-off between current and discounted future marginal utilities. The investor's wellbeing should also be affected by his/her past consumption. Earlier references can be found in Ryder and Heal (1973) and Deaton and Muellbauer (1980). A consumer's past consumption can be viewed as an "internal" wellbeing benchmark. In this sense, the consumer's utility is impacted, or even "spoiled" by his/her own historical spending pattern. As he/she got used to an adequate and possibly increasing level of past consumption, he/she also seeks to upkeep it in the future. Constantinides here is reflecting on the "habitual" level of spending. This is also intuitive. Technically, the investors' marginal utility from consuming today can be modelled as an increasing function of his/her past consumption.

However, there are also "external" benchmarks that might have an effect on the total utility of the investor. Financial economists refer to this phenomenon as "keeping up with the Joneses" (Abel 1990). This consideration originates from across the street, i.e. it is external to the investor. In this interpretation, the investor's neighbour's consumption is a vital consideration for his/her own. As the investor monitors the consumption pattern of others, he/she is concerned that it will affect him/her as well. For example, when he/she sees that the Joneses are tightening their consumption belt, he/she assumes that he/she will soon have to do the same. In the model context, the Joneses expenditures are proxied by the time series of aggregated per capita consumption. Linking slow-moving macro-economic data into the utility-function that explains, fast-moving asset prices are technically and theoretically challenging. That aside, it seems intuitively right that our own decision to consume and invest is affected by how other people behave.

Both the internal and the external benchmarking call for a modification of preferences in the sense that utility flows from a variety of different sources other than exclusively from own consumption now and later. Cochrane continues: “The Campbell-Cochrane model is a representative from the literature that attacks the equity premium by modifying the representative agent’s preferences.” (Cochrane 2005, p. 466).

In the end, Campbell-Cochrane (1999) settles for an external benchmark akin to that discussed in Abel (1990): “An individual’s habit level depends on the history of aggregate consumption rather than on the individual’s own past consumption.” (Campbell and Cochrane 1999, p. 208). This decision is not explicitly defended. It seems, however, to be out of convenience or, possibly because by considering this version, the model performs better when compared with real situation data than its “internal” benchmark alternative.

A relationship between per capita, aggregate societal consumption and habit-persistence is thus established both theoretically, and as we shall later see, empirically. It can be captured, Campbell and Cochrane tell us, by what they call the “surplus consumption ratio” S_t (Campbell and Cochrane 1999, p. 209):

$$S_t = \frac{C_t - X_t}{C_t}$$

C_t is the current consumption and X_t is the measure for the habitual level of consumption - both at the current time t. The ratio is thus the fraction, or “surplus” of an investor’s consumption that exceeds the eternal, aggregate level. This surplus is said to influence the utility of the consumer. In dire economic states, the surplus in excess of the benchmark consumption level might collapse towards nil. But it can also, in good states, expand towards one. When the surplus decreases and falls towards the per capita consumption trend-line, the investor’s risk aversion increases. The perspective of a possible drop below this level is thought of as being very bad indeed because the investor might not do as well as the Joneses. [53]

In these cases, when the economic activity typically falls, the marginal utility of consumption is increasing. Alternatively, when the economic activity slows or even falls when in recession, the prospects of losing income from employment increases because the investor might become unemployed. At that time, receiving income in the form of payoffs from the investment portfolio is a good thing. Investors are thus afraid of holding stocks because they do poorly in a recession type of economic environment, i.e. when the surplus consumption ratio falls towards zero. Hence, they demand a high equity risk premium for holding financial assets that, possibly, are not recession proof.

After developing this idea and wrapping the intuitive *story* into the narrative I just referred to, the question is how to integrate it into the CCAPM. Again, Campbell and Cochrane takes out the power utility function and adds in the "surplus consumption ratio" as an additional argument. Here is how it looks like in the form of the intertemporal marginal rate of substitution M_{t+1} (Campbell and Cochrane 1999, p. 210):

$$M_{t+1} = \delta \frac{u_c(C_{t+1}, X_{t+1})}{u_c(C_t, X_t)} = \delta \left(\frac{S_{t+1}}{S_t}\frac{C_{t+1}}{C_t}\right)^{-\gamma}$$

The externally driven habitual level of consumption is thus incorporated in the standard power utility function used in the CCAPM. Cochrane tells us: "This specification means that a habit can act as a "trend-line" for consumption; as consumption declines relative to the "trend" in recession, people will become more risk averse, stock prices will fall, expected returns will rise, and so on." (Cochrane 2006, p. 34).

[53] Mathematically, however, a negative surplus ratio is excluded due to the non-linear functional form of the way habit responds to changes in the consumption.

2.2 Reverse-engineering

Cochrane's reverse-engineering takes place inside the utility function. Here are some further examples of what a slow moving, habitual consumption level can do. It:

> "...eliminates terms in marginal utility in which extra consumption today raises habits tomorrow, while retaining fully rational expectations. (...) ...produces slow mean-reversion in the price-dividend ratio, long horizon return forecastability,... (...). We specify that habit adapts non-linearity to the history of consumption. The non-linearity keeps habit always below consumption and keeps marginal utility always finite and positive in an endowment economy." (Campbell and Cochrane 1999, p. 208).

Cochrane's reverse-engineering, therefore, works backwards from the observed data towards the appropriate model specification. This involves choosing and setting the parameters of the model to the desired levels – as I showed in detail in *Chapter Two, section 6.4*. Putting the technical language aside for now, I *first* direct my attention towards how these results are generated. In particular, I note that there is little evidence that would indicate that they are derived results from the fundamental, core, principles of a rational, self-interested and risk averse investor. But it is not necessary for the principles to deduce all results entirely by themselves. Additional information can, and in many situations, must be added to reach for the theory to reach all the way down to the empirical (see, for example, *Chapter Four, section 3.1.2* and *3.1.3*) From a Lakatosian point of view, therefore, Cochrane is well within the heuristics of the consumption based research programme.

Second, the extracts are all good examples of how Campbell and Cochrane use the data and let them define functional forms and calibration of parameters:

> "Rather than dream up models, test them, and reject them, financial economists since the work of Mehra and Prescott (1985) and Hansen and Jagannathan (1991) have been able to work backwards to some extent, characterizing the properties the discount factors must have in order to explain asset return data." (Cochrane 2005, p. 455).

Financial economists, in other words, know how large the equity risk premium has been historically, and they know they have to work towards altering the stochastic discount factor (SDF) to meet the empirical requirements.

The results are extracted with one goal in mind, i.e. fitting theories to the world. And this requires a model in-between the two and lots of imagination, intuition and technical skills. I believe this approach to the problem at hand, i.e. the equity risk premium puzzle, is justifiable from a research programme point of view. Changing functional forms, adding new auxiliary, belt, assumptions and re-formulating the narrative have all been traditional means since Lucas (1978) to establish analytically convenient cases and turn them into useful empirical representations (see *Chapter Four, section 3* and *4*). The *story* around the "Joneses" and the habitual level of consumption are outstanding examples of this practice. And I may point out that this practice is far away from Hempel's strictures related to the use of so-called bridge-principles that I discussed in the previous *Chapter Four, section 4.4.3*, in relation to theories reaching all the way down to the empirical situations. It, therefore, comes naturally that the programmes' heuristic accommodates innovative approaches that over time will integrate more and more realistic sources of utility. After all, habitual consumption seems to reflect what we observe and, possibly, from an introspective point of view, it makes sense as well. An interesting, still open issue at this time, is a careful analysis of how much the new argument contributes to utility when acting on its own independent of the other factors Cochrane has identified in the utility function. He must be warned, therefore, not to keep adding in whatever it takes to engineer the right prediction.

2.3 Statistical results

I now, briefly, address how empirical tests of the consumption based model are conducted. Let us start with the model evaluation and then proceed to the outcomes,

2.3.1 Model evaluation

Cochrane tells us that the habit-persistent extended CCAPM explains the equity risk premium better than the standard CCAPM: “The model replicates the level of risk free rate, the mean excess return (equity premium), and the standard deviation of excess stock returns. Most importantly, the models fit the dynamic behaviour of stock prices.” (Campbell and Cochrane 1999, p. 207). These results are impressive, but they come at a cost.

Recall that Cochrane added an additional argument to the standard CCAPM, i.e. the exogenous per capita consumption trend. The movement of the investor’s consumption relative to this “external benchmark” directly influences his/her own consumption and thus marginal utility. In particular, if the surplus consumption ratio is low, the marginal utility of consumption is high and vice-versa. In this dynamic adjustment process, the representative investor’s character does not change. He/she continues to maximize his/her total utility through time and across economic states relative to the available budget. The theoretical foundation, i.e. the fundamental, core, principles as portrayed in the triad of temperaments are thus kept intact – as the heuristic of the programme demands. The adjustment, therefore, came as an additional auxiliary, belt, assumptions because it increases the number of causal factors the investor needs to consider when making choices with respect to the distribution of current and future consumption over time and through various economic states. In other words, the investor is given a new argument that needs rational considerations.

Although little was changed in the standard CCAPM, the results seemed to have improved. What happened? In Campbell-Cochrane (1999), we are told that the fully specified enhanced CCAPM model gained in structure as several new parameters were added to the central pricing formula. In fact, there are seven of them ranging from the “mean consumption growth” and ‘standard deviation of consumption growth” to “utility curvature” and “log risk free rate”. An additional three parameters are “implied”. They are the familiar “subjective discount factor” and the new ‘steady state surplus consumption ratio” and the “maximum surplus consumption ratio”. All these parameters are given specific values so that they match particular moments of the post-war US consumption data I pointed towards earlier. For example:

> "Since the ratio of unconditional mean to unconditional standard deviation of excess returns is at the heart of the equity premium puzzle, we search for a value of *[utility curvature*] on that the returns on the consumption data claims match this ratio in the data." (Campbell and Cochrane 1999, p. 218).

Now, having calibrated the model using real data, some oddities emerge. [54] Campbell and Cochrane, for example, point out:

> "It is important to understand that with these parameter values the model uses high average risk aversion to fit the high unconditional equity premium. Steady-state risk aversion is (...) 35. In this respect the model resembles a power utility model with a very high risk aversion coefficient." (Campbell 2001, p. 60).

During some time periods, the habit-persistence model does fairly well. In others, however, it fails to do away with the risk premium. On a longer term historical average basis, it gets the risk aversion parameter γ down to the value of 6 – which is still far off the lower number needed to explain away the equity risk premium, i.e. 0.4% in the traditional CCAPM. Nonetheless, a parameter value of 6 is more plausible than 55 as Black (1972) suggested in *Chapter Two, section 6.2*. Out-of-sample forecasting is crucial for addressing concerns that the Campbell-Cochrane model over-fits the data because it is calibrated to match historical stylized facts concerning equity returns.

This confirms my point related to the costs of making better models. While the enhanced CCAPM model produces an improved statistical fit with the data from real situations, it still depends upon unrealistic assumptions concerning, for example, the representative investors' near to perfect foresight of future economic states or the calibrated high level

[54] Calibration is a step-wise simulation procedure often used as an alternative to the traditional econometric analysis of estimation. A model draws on its theoretical framework to identify the observable variables it would need to be confronted with the data. The coefficients are thus not directly estimated but given a particular value, i.e. calibrated according to what is observable in an economy. For example, the average p.a. growth in aggregate house-hold consumption numbers can be directly used in the modeling.

of risk aversion - which in the habit-persistence model is time-varying and not constant as in the standard CCAPM. There is as a consequence little benefit when Campbell claims:

> "The time-variation in risk aversion generates predictable movements in excess returns (...), enabling the model to match the volatility of stock prices even with a smooth consumption series and a constant riskless interest rate." (Campbell 2001, p. 61).

Cochrane seems to agree:

> "This model does have high risk aversion. (...) many authors require that a "solution" of the equity premium puzzle display low risk aversion. This is a laudable goal, and no current model has attained it." (Cochrane 2005, p. 473).

Both authors have thus admitted that they have failed in their endeavour to model the risk aversion factor down to a level consistent with their asset pricing theory.

Besides calibrating the model's parameters, they can also be estimated. For the standard CCAPM Cochrane tells us:

> "Our first task in bringing an asset pricing model to the data is to *estimate* the free parameter: β and δ in $m = \beta\ (c_{t+1}/c_t)^{-\boldsymbol{\delta}}$. Then we want to evaluate the model. Is it a good model or not? Is another model better?" (Cochrane 2005, p. 187). [55]

Based upon either calibration or estimation, Cochrane also suggests that comparative statistics is a good way to distinguish and choose between different models, i.e. rank them according to the quality of their predictions against real situation data. He continues on this point: "Even when evaluating a specific model, most of the interesting calculations come from examining specific alternatives rather than overall pricing error tests." (Cochrane 2005, p. 304). Here, Cochrane sows some *first* seeds of doubt related to the absoluteness of using predictability as the sole measure for selecting between

[55] We note that he is here referring to the subjective discount factor parameter β and the risk aversion parameter δ found inside the stochastic discount factor.

alternative asset pricing models. More than a statistical result seems to be needed. I have more to say with respect to t this topic in the up-coming *section 5*.

2.3.2 Rejected models

Early statistical tests of William F. Sharpe's CAPM indicate that the pricing model had explanatory powers (see *Chapter Two, section 1.3*). In subsequent tests, the model's claims were, however, rejected. Well-known empirical references are Black, Jensen, Scholes (1972), Miller and Scholes (1972), Blume and Friend (1973), Litzenberger and Ramaswamy (1979), Shanken (1985), Campbell, Lo and MacKinlay (1997), Fama and French (2004), Ang and Chen (2005). But, as Cochrane rightly points out, the CAPM was not abandoned despite its empirical inadequacies. It is still alive and doing well – at least in the academic literature and university level introductory courses in finance. The same can be said in relation to the multi-factor style models that Eugene F. Fama and Kenneth R. French have been promoting. They have also been rejected. Despite this, the multi-factor model has severely challenged, if not dethroned, Sharpe's CAPM. Of late, Sharpe has even turned sceptical towards his own asset pricing research efforts.[56] Finally, the CCAPM has also been rejected. Campbell and Cochrane claims:

> "The development of the consumption based asset pricing model ranks as one of the major advances in financial economics during the last two decades. (...) Unfortunately, consumption based asset pricing models prove disappointing empirically. Hansen and Singleton (1982, 1983) (...) reject the model on U.S data, (...). Wheatley (1988) rejects the model on international data." (Campbell and Cochrane 2000, p. 2863).

A newer contribution by Cochrane (2011) also expands upon the topic of predictability but does not offer new empirical insights. As I have pointed out earlier as well, no asset pricing efforts have withstood standard statistical tests of predictive significance. Some of these models, however, have rendered better results than others.

[56] Sharpe (2007) claims that his capital asset pricing model (CAPM) "…was built from Harry Markowitz's view that an investor should focus on the expected return and risk of her overall portfolio… (…) This is not an entirely happy state of affairs. There are strong arguments for viewing mean/variance analysis as a special case of a more general asset pricing theory." (Sharpe 2007, p 4). This "more general theory" Sharpe refers to is indeed a version of the macro-model that Cochrane advocates.

2.4 Assessing Cochrane's habit-persistence response

On one hand, Cochrane's reverse-engineering approach modestly improved the enhanced model's empirical estimates relative to the standard CCAPM result. Historically, during some short time periods, the habit-persistent enriched CCAPM even delivers modestly improved statistical results. Longer term, however, the results are statistically rejected as I indicated in the previous *section 2.4*. On the other hand, since the models' ability to forecast has modestly improved, it might be that Cochrane has identified and added in a right additional "cause". It implies that habit-persistence possibly is a so far neglected causal variable that should be included as it has contributed positively towards improving the models' qualities in predicting the equity risk premium.

Yet, the work to include habit-persistence into the utility function is more than just technical dexterity. It is a good example of how financial economists proceed in their ways of "doing" science within their research programme in their quest to improve predictability of phenomena under investigation. Nevertheless, many observers have criticised the profession of becoming too mathematical. Robert M. Solow:

> "Many outside observers and some critics from within the profession have interpreted this development as a sweeping victory for "formalism" in economics. The intended implication is that economics has lost touch with everyday life, that it has become more self-involved and less relevant to social concerns as it became more formal (and more mathematical)." (Solow 1997, p. 42).

Solow here makes the claim that no distinction is drawn between formalism and mathematics. He wants this line to be drawn. By doing so, he does not deny that practicing economists have increasingly turned to mathematical models when analysing an increasingly larger set of data, but it has not lost touch with reality:

> "A good model makes the right strategic simplifications. (...) Economic models are usually stated mathematically, but they do not have to be. (...) But mathematics turns out to be a very efficient way to express the structure of a simplified model and it is, of course, a marvellous tool for discovering the implications of a particular model. That is probably why outsiders tend to think of

> model-building as just more formalism. That is a mistake. The mere use of mathematics does not constitute formalism." (Solow 1997, p. 46).

He continues by stating that mathematical models, often, focus on: ...one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality interact." (Solow 1997, p. 43). Solow's description of a mathematical model particularly well portrays my own understanding of what Cochrane's "M" in CCAPM represents.

If mathematics does not constitute "formalism", what does? Solow explains:

> "If "formalist economics" means anything, it must mean economic theory constructed more or less after the model of Euclid's geometry. One starts with a few axioms, as close to "self-evident" as they can be – although this is harder to do when the subject matter is more complicated than points and lines in a plane – and tries to work out all the logical implications of those axioms." (Solow 1997, p. 42).

Here, we are told that formalism is related to axiomatic systems and deductions. In *Chapter Three, section 3.2.3*, I show that Cochrane applies this methodological strategy. Solow seems to imply that this is the turf of natural sciences and cautions against its use in the social sciences. Solow thus argues first that mainstream economics has become more mathematical and second that many mistake mathematics for "formalism".

Let me make three points. *First*, I do not think there is much of a difference between the mathematical model's "one or two causal" factors and the "self-evident axioms" Solow makes references to in these quotes. Consider, for example, Von Neumann and Morgenstern's axioms of rationality. They may be considered to be both "causal factors" and "self-evident" axioms. The axioms are considered to be valid starting points for both formal "deductions" and mathematical "derivations". This is also the approach Cochrane takes when he sets out with the triad of temperaments. But Cochrane does more. He applies what Lakatos calls the positive heuristic of the research programme and establishes a new argument. i.e. the "surplus consumption ratio" as an auxiliary, belt, assumption and integrates it into the stochastic discount factor (SDF). In this way, Cochrane augments the function, creates a specific analytical case, and compares its predictive success against the real situation data. Cochrane is thus following good financial economic practice. He makes the right "strategic simplifications" and uses mathematics as a "marvellous tool" as Solow sees it, to derive and discover. The distinction Solow draws between mathematical and formal economics might, therefore, not be as clear cut as he wants it to be in the case of Cochrane's elaborations.

More importantly, and this is my *second* point, Solow cautions against the use of formalism in economics. He claims that:

> "To the extent that economists have the ambition to behave like physicists, they face two dangerous pitfalls. The first is the temptation to believe that the laws of economics are like the laws of physics: exactly the same everywhere on earth and at every moment... (*and second*) ...there is a tendency to undervalue keen observation and shrewd generalization... (*and that*) ...there is a lot to be said in favor of staring at the piece of reality you are studying and asking, just what is going on here? Economists who are enamored of the physics style seems to bypass that stage, to their disadvantage." (Solow 1997, p. 56).

Solow seems to advice his fellow practitioners to take a less formalistic view of economics and engage more with reality as they observe it. Therefore, observation and causalities become more prominent than axioms and deductions which are perceived to be "formalistic, abstract, and negligent of the real world". On my second point, Cochrane is a multiuser of the various tools that Solow discusses. Cochrane, as we know, takes a top down "formal" approach, enriches and extends it with mathematical derivations and takes the results to the empirical situations. It seems, therefore, that Cochrane's tool-box is both large and versatile as he uses all available means to reach the goal of providing knowledge for use. On this point, Cochrane would have been supported by Lakatos who promotes positive heuristic that contributes to progressive the empirical and the theoretical reach of a research programme.

Third, and finally, Solow is keenly aware of the pitfalls when working at a low, empirical data level. He even characterizes data as "expensive" and theory as "cheap". With this suggestion, Solow suggests that theory outruns the data:

> "In economics, model-builders' busywork is to refine their ideas to ask questions to which the available data cannot give an answer. Econometric theorists invent methods to estimate parameters about which the data has no information". (Solow 1997, p. 57).

Despite this warning and having dismissed "formalism", Solow does not see any real competing alternative to progress in the field of economics. I cannot find that Cochrane has a clear view on Solow's final advice. On one hand, we have heard in *Chapter Four, section 1.2.2*, that Cochrane considers "theory" to lag behind the work done on asset pricing models. In contrast to Solow's view, Cochrane argues that it is the theory that is "expensive" because it cannot provide a robust underpinning of his *stories*. On the other hand, Cochrane's parameter estimates and calibrations can hardly tell us that asset prices are "efficient" in the sense that they are generated through processes that involve the representative triad of temperaments plus habit. In this sense, therefore, Cochrane indeed has embarked upon a trip against which Solow warns.

In the end, Solow cautions against both formalism and too much "data-mining". Instead, he endorses curiosity through observing the real world around us and the development of small scale mathematical models with only a limited number of causal influences. Cochrane, as I have just shown, traverses across all these dimensions and is an avid user of the available tools from fundamental principles to analytically convenient special cases that may also represent useful empirical situations.

I think Cartwright would enjoy Cochrane's versatility. But it has to be channelled in the "right" way. It involves drawing on three interconnected building blocks, i.e. fundamental principles, bridge principles, and auxiliary assumptions (see *Chapter Four, section 4.4.3*). Earlier, I explained that many philosophers of science believe economists have only a few fundamental principles and that they are used as premises for theorizing. In particular, I pointed towards two popular references; rationality and equilibrium principles. Given the scarcity of such fundamental principles, economists, therefore, draw on a host of assumptions that in reality are very concrete concepts. Cochrane uses, for example, every-day terms such as financial markets, consumption, companies, investors, stocks, prices, returns, interest rates, risk premia, etc. Cartwright calls these references "socio-economic quantities". Cartwright suggests there are good reasons why economic and financial models for that matter have these characteristics:

> "If you have just a few principles, you will need a lot of extra assumptions from somewhere else in order to derive new results that are not already transparent in the principles." (Cartwright 2007, p.228).

In Cochrane's context, for example, the result of the triad of temperaments plus the habit-persistence argument in the enhanced version of the CCAPM cannot be judged without a particular setting, i.e. that of financial markets and assets. These concrete concepts, therefore, increase the "range" of the derived consequences. Cartwright argues that what might happen in such models is problematic:

> "The problem is that we often do not know by looking at them that the specific derivations made in our models depend on the details of the situation other than just the mechanism itself operating on accord with our general principles. (...) We know that the results obtain because we know that they follow deductively given the formal relations of all the factors that figure in an essential way in the proof. But the whole point (...) is that the background factors should not matter to what happens. We are supposed to be isolating the effects of the features or process under investigation acting on its own, not effects that depend in a crucial way on the background." (Cartwright 2007, pp. 231).

What Cartwright here refers to as "general principles" and the "details of the situation" or the "background factors" are, I believe, respectively akin to CCAPM's fundamental, core, principles and the auxiliary, belt, assumptions. The challenge for Cochrane is thus to distinguish the effects that these two broad sets, i.e. principles and assumptions have, individually, on the model-based results in relation to their empirical exposure. How much rationality, self-interest and risk aversion are there, for example, in the equity risk premium, and how much of the premium can be allocated to other aspects of the specific situation and background factors such as consumption habit, the Joneses, almost perfect foresight, and the representative investor's eternal life?

I see two ways for Cochrane to proceed from here. In my *first* option, he continues to develop the CCAPM as before by searching for new and innovative additions to an existing framework as we have seen in the extension of the utility function towards habitual consumption. There is, for example, a growing literature that investigates how investor's behaviour influence asset prices, i.e. behaviour finance. This direction of research might result in a larger set of auxiliary, belt, assumptions and, possibly, to better point-forecasts towards ever increasing specific analytically convenient special cases. Adhering to the negative heuristic with respect to the fundamental, core, principles, this first option will increase the model's exposure towards the "background factors" that Cartwright discusses. In fact, if Cochrane seeks improved point-forecasts, he should include any facts concerning the targeted situation that makes a difference.

The first option, I believe, would still protect the fundamental, core, principles. But there might be limits to what can be achieved. We recall that Cochrane feels "theory lags behind". This might give rise to Cochrane's view that:

> "No model has yet been able to account for the equity risk premium with low risk aversion, and Campbell and Cochrane (1999) offer some reasons why this is unlikely ever to be achieved." (Cochrane 2008, p. 266).

On one hand, this view is hard to reconcile with other statements in which Cochrane expresses the opinion that he indeed seeks a resolution to the risk premium puzzle. On the other hand, he seems to allude to the difficulties of working within the confines of the current research programme. [57]

[57] In a recent contribution, Cochrane (2011a) first states that "in fact, many economic events *should* be unforecastable, and their unforecastability is a sign that the markets and our theories about them are working well". The he continues arguing for "getting the big picture of cause and effect right" when explaining a phenomena. The big picture Cochrane alludes to focuses on "one analytical tool", keeping "analysis simple and elegant", applying "simple and compelling logic", and "stick to a few core principles because (…) nobody really know the detailed answers". He contrasts this strategy with one that foresees a "wide assortment of analytical tools", mixing "information from diverse sources" such as "economics,

In my *second* option, Cochrane could opt for establishing what Cartwright alludes to as claims about *tendencies*. This second option does not prevent him from augmenting the existing theory and model. It merely tones down the programmes ambitions with respect to deliver point forecasts. The discussion of these two options will take place later in *section 4.3*.

Let me conclude. I see reverse-engineering as a mainstream activity. Nonetheless, mathematical dexterity and innovative auxiliary, belt, assumptions should not leave formalism too far behind. Cochrane seems to agree: "In general, empirical success varies inversely with theoretical purity." (Cochrane 1999, p. 40). This makes my point. In which direction should the consumption based research programme then be taken? It can continue its elaboration to accommodate the data with the inclusion of an increasingly larger pool of auxiliary, belt, assumptions. Or the research programme can be re-directed towards what Cartwright (1999) calls *tendency* claims (see *section 4.3* for a discussion).

3. *Justifying CCAPM and the empirical tests*

Earlier I showed how financial economists, like Cochrane, sought answers to a long standing research question in finance, i.e. the equity risk premium puzzle. We recall that the standard CCAPM and even the enhanced version including the external habit-persistence formation could not fully explain this puzzle. In the next sections I, *first*, review why Cochrane still believes that the CCAPM, despite its empirical weaknesses, is the correct starting point for exploring the statistical behaviour of asset prices. *Second*, I review Cochrane's suggestions on how the asset pricing research effort should be evaluated, i.e. what is the "litmus-test". *Third*, and finally, I ask which parts of the research project is compared with the real situation data.

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political, sociological, psychological" perspectives. Cochrane's analysis, I think, mirrors preference for a few fundamental core principles at the detriment of an extensive application of auxiliary, belt, assumptions to accommodate the "analytically convenient special cases".

3.1 The CCAPM is the correct starting point

From previous chapters we know that fundamental, core, principles and auxiliary, belt, assumptions combine to establish the central pricing formula, as well as the analytically convenient special cases. These cases are detailed mathematical models that define highly specific financial market cases. Only a few of these idealized and even fictional cases can be considered to be useful empirical representations. And when they are, they do not tell us much. In *Chapter Four section 4.2*, I discussed this in detail. Ultimately, however, the habit-persistent extended CCAPM does not deliver statistically successful results. Knowledge for use to the various stakeholders is, therefore, lacking. The model's results are too dependent upon the "background" situations established by the auxiliary, belt, assumptions and are, as a consequence, not useful in situations different from those detailed and specified by the analytical cases. In this respect I referred to their lack of horizontal portability (see *Chapter Four, section 3.1.5*).

Under these circumstances, we could expect that CCAPM's modelling effort is at least questioned if not abandoned. Questioned it was as we saw in the reverse-engineering reaction. Abandoned it has, so far, not been. Cochrane, therefore, leaves no doubt. The CCAPM is the correct starting point – there is no substitute. Why is that? I think the main answer can be sought in his commitment to the fundamental, core, principles as I developed them in *Chapter Three, section 2*. In other words, Cochrane seeks to stay within what Lakatos referred to as the research programme and its heuristic in which the triad of temperament remains a protected element and the myriad of auxiliary, belt, assumptions progress the programme. I identify three reasons for this mind-set.

First, Cochrane rejects the factor-based, "relative" approach taken by fellow financial economists such as Sharpe (1963, 1964) as well as Fama and French (1992, 1993, 1995, 1996). (see *Chapter Two, section 1.3*). Cochrane makes his criticisms on three counts; how the explanatory factors are chosen, what they proxy for, and the achieved predictability against real situation data. It goes without saying that he believes his own version of CCAPM is able to hold its own and withstand this type of criticism. In fact, Cochrane believes that all this research effort can be subsumed under his, more general, asset pricing theory. By now, however, we know that this is not defendable as I showed in *Chapter Three, section 3*.

Cochrane's criticism thus starts with the chosen "explanatory" factors. He says it is too easy to assign one or more factors such as the market portfolio in CAPM or the value and growth portfolios provided by Fama and French to a pricing model. He states: "By blindly including right-hand variables, one can produce models with arbitrarily good statistical measures of fit. But this kind of model is typically unstable out of sample or otherwise useless for explanation and forecasting." (Cochrane 2005, p. 125). Next, Cochrane asserts that these types of statistical models do not explain where the factors come from in the first place and what they represent: "...one should always ask of a factor model, "what is the compelling economic *story* that restricts the range of factors used?" (Cochrane 2005, p.125). Not the movement of nearby assets, indices or portfolios, therefore, but underlying microeconomic set-ups and macroeconomic events influence the average returns, volatilities and co-movements. Cochrane, however, assures us that this is accounted for in his version of the CCAPM.

Fama and French were aware of these shortcomings. Fama (1991) had prior to Cochrane's critique anticipated some weakness in his framework with respect to how factors were "discovered". Financial economists should be mindful not to go out with a "fishing licence" when searching for explanatory variables. Having made the factor choices, which are motivated by empirical experience, Fama continues by telling us that it leaves: "...one hungry for economic insight about how the factors relate to uncertainties about consumption and portfolio opportunities..." (Fama 1991, p. 1594). Fama thus played the ball back into Cochrane's court.

Cochrane finally also criticizes the factor models because of their insufficient ability to predict real situation data. He thus echoes earlier studies, which have shown that both the single- and multi-factor models have failed to produce robust statistical evidence for their claims. Nonetheless, Cochrane claims as we know that the results have improved to the extent that they are now on the verge of "statistical significance". However, we also know that he is critical of the empirical performance of his own research effort: "Unfortunately, this specification of the consumption-based model does not work very well." (Cochrane 2005, p.43). But he quickly adds: "To understand the market premium, there is no substitute for economic models such as the consumption based model outlined above ..." (Cochrane 1997, p. 16).

My *second* point for why Cochrane sees no alternatives to his own version of the CCAPM is that it should be considered the "missing" middleware that connects the real economy with financial markets. Cochrane thus portrays financial market activities as the crucial intersection between the two. The same cannot be said regarding the relative, factor based research programme. In fact, it was not Fama and French's intention that it should fill the gap that Cochrane alludes to. Their goal was to predict time series of returns with other times series of returns. Nevertheless, Cochrane lets us know that something is wrong with this interface:

> "Understanding the marginal value of wealth that drives asset markets is most obviously important for macroeconomics. The centrepiece of dynamic macroeconomics is the equation of savings to investment, the equation of marginal rates of substitution to marginal rates of transformation, and the allocation of consumption and investments across time and states of nature. Asset markets are the mechanism that does all this equating. (...) In fact the first stab at this piece of economics is a disaster in the way first made precise by the "equity premium" puzzle. (...) Clearly, finance has a *lot* to say about macroeconomic and it says that something is desperately wrong with most macroeconomic models." (Cochrane 2008, p. 242).

So far, main-stream macro economists have chosen to ignore Cochrane's criticism and insights from the consumption based capital asset pricing research project. Of late, however, following the eruption of the global financial market and sovereign crisis in 2007, voices of concerns have been raised in this direction (Borio 2012). Nonetheless, Cochrane, as a financial economist, is adamant with respect to the "M" in CCAPM: "Something like the consumption-based model – investors' first order conditions for savings and portfolio choice – has to be the starting point." (Cochrane 2005, p. 455). As above, we are again told that the correct starting point for asset pricing research is the consumption based pricing approach.

Third, as we have discovered, when Cochrane meets challenges with respect to statistically insignificant predictive results, he goes back to the drawing board and modifies his *story* based CCAPM from a theoretical and empirical point of view. The standard CCAPM has thus been overhauled in several reverse-engineering steps to make it comply with the real situations: "...these models are truly drastic modifications; they fundamentally change the description of the sources of risk that commands a premium in asset markets." (Cochrane 1997, p. 13). This approach made us conclude that this is how models are built by the profession of financial economists. It is supported by John Stuart Mill as shown in *Chapter Three, section 3.2.3*, and by Lakatos, as referenced in *Chapter Three, section 3*. Cochrane, in other words, shows us how science is practiced. We also found that the fundamental, core, principles were left almost unchallenged in the reverse-engineering step. Most modification took place in the set of auxiliary, belt, assumptions.

I think the practice of reverse-engineering reveals two interesting points: *First*, it indicates Cochrane's thinking with respect to his world-view. It shows his ontological commitment both to the domain of finance and to the underlying order of things in that very domain. Cochrane tells us that this order can indeed be described in mathematical models that work only with a few main causes. If the first trial fails to predict the real situations, the auxiliary, belt, assumption, are reviewed and adjusted. Predictive failure should energize the scientist to go beyond his starting point of reviewing just a limited number of forces in isolation. New causes should be "added in" so that empirical tests may be run again. When, for example, the traditional CCAPM was unable to justify the large equity risk premium, the new habit-persistent CCAPM was developed. This, I think, is a good example of how scientific explorations have evolved in the field of finance. Nonetheless, it leaves us somewhat unguided in the "discovery phase" of the causes that would be "added" to the existing framework when encountering problems and its methodological basis. This commitment is certainly difficult to uphold in the light of the poor empirical results delivered from the enhanced CCAPM.

Secondly, Cochrane's research effort also exposes the readers to a belief that the more specific asset pricing theories become and the more specific the models of analytical cases and their extensions towards the real situations are, eventually, a scientific "break-through" with respect to an adequate level of knowledge for use can be expected. Given the underlying complexities and richness encountered in the social and economic life of individuals, Cochrane's effort is certainly ambitious. Can it be, we may ask, that the complexities are too high for his research project to be successful? Can it be that the real situations do not give rise to a theoretical description? If the answer is "yes" or even "may be", one conclusion is that Cochrane's asset pricing research approach is reaching self-imposed limits and, in the end, adds little to what informed investors, public policy makers and fellow academics already know, or can know. Empirical results, thus far, seem to support the possibility of answering the questions in this stylized way. Such a conclusion certainly would question the role of offering knowledge for use and giving policy advice stemming from this type of research. Additionally, it would give weight to my suggestion that Cochrane might want to redirect his focus away from point-forecasts of very specific financial market situations to a more general view of model-based *tendency* claims (see *section 4.3* later).

But hold on. Before we continue exploring the topics just raised, we still have a few other open questions to answer. *First*, what are the criteria used to judge the qualities of the CCAPM? Or to quote Alexander Rosenberg what is the "litmus-test"? (Rosenberg 1992). *Second*, what is actually being tested against the empirical situations? The CCAPM as a whole, its fundamental "core" principles, the specific "belt" assumptions, the central pricing formula, the convenient analytical cases category one, the analytical cases category two, i.e. the useful empirical cases, their functional forms - or even something else? Answering these questions are the topics for the next two paragraphs. Let us start with the first, i.e. the benchmark against which progress can be measured, i.e. the "litmus-test".

3.2 The "litmus-test"

Cochrane has written both theoretical and practical texts on finance and economics and their interconnectivity. These combinations are meant to advance the understanding of such matters and guide decision makers – be it investors, public policy makers, or fellow academics. A central theme has been the ability to predict the prices and returns on financial market assets. From Cochrane's many publications, we can extract both his disappointments and encouragements. He tells us that the standard CCAPM "does not work very well"; but that single- and multi-factor models seem to "perform better"; that "daily, weekly, and monthly stock returns are close to unpredictable"; but that specific financial and economic variables "can, in fact, predict substantial amounts of stock return variation". Many of his research papers are also forward looking and advice-centric. Here, are a few titles: "Where is the market going – uncertain facts and novel theories." (1997), "New facts in finance." (1999), "Portfolio advise for a multifactor world." (1999a), and, finally, "Explaining the Poor Performance of Consumption-Based Asset Pricing Models." (Campbell and Cochrane 2000).

Based upon these title-samples and findings in previous chapters of this thesis, we can comfortably conclude that forward looking analysis, continued improvement in predictive success of the theories and models, and, in the end, specific policy advise count towards how success – or failure – of his project should be judged. This is also reflected in Cochrane's efforts to explain away the equity risk premium, i.e. he cares about empirical confrontations and their evaluation. His effort boils down to finding and integrating the "right" explanatory variables into the analytical cases. They are thought of as stable and reliable causes that underlie and explain the surfacing pricing data. If these model adjustments can be adequately accounted for in the utility function, better prediction of future price and return levels could be expected.

Predictability, as we have seen, is also one of the main items on Lakatos' (1978) agenda, as it gauges the progression or degeneration of a scientific research programme. Research programmes are therefore not theoretical exercises. Lakatos saw them brought to the empirical situations and tested against the available data, i.e. they must be falsifiable. In cases in which the predictions do not meet the required standards, the programme's "hard core" is left unchallenged while the "protective belt" is adjusted in order to get the programme back on track again. We recall from *Chapter Three, section 3.3*, that Lakatos referred to this practice as the negative and the positive heuristic. Should these changes result in improved predictions, Lakatos tells us that the programme is theoretically and empirically "progressive". The changes enable the programme to expand in a positive way, i.e. it contains "excess empirical content" and "this excess empirical content is well corroborated". But changes to the protective, belt, assumptions can also harm the research programme. In these cases, the programme "degenerates". This is often the situation when the "belt" just accommodates new assumptions in an ad-hoc manner whenever predictions fails – Lakatos explains.

Against this background, we can conclude that Cochrane's research programme seeks progression along the lines that Lakatos suggests. The benchmark used to measure this success is thus the quality of the predictions. Rosenberg reminds us: "...I held that long-term improvement in predictive success is a necessary accomplishment of any discipline that claims to provide knowledge, and especially to provide guidance to policy. (...) Hereafter I shall assume most economists share the commitment to improvement in predictive adequacy as a necessary condition for the certification and expansion of economic knowledge." (Rosenberg 1994, p. 56). This statement, I think, is highly relevant to Cochrane's own effort. His "litmus test", to borrow Rosenberg's terminology, is predictability. Successful predictions, therefore, it is taken as a basis for generating knowledge for use. But what is tested?

3.3 What is "tested"?

Cochrane, as I pointed out earlier, is not overly explicit in the way he distinguishes between theories and models. He is in good company with most fellow financial economists. It might be, I concluded, that it does not matter to him because he uses the terms interchangeably. However, when it comes to choosing between different models, but also theories, Cochrane seems to become more specific and, even, a bit picky. We have been told, for example, that, despite its empirical inadequacies, "something like" the

CCAPM and not Fama and French's multi-factor portfolio models is the correct starting point. Cochrane gets even more concrete by saying: "...when evaluating a specific model..." (Cochrane 2005, p. 304). From this, we can extract that Cochrane has a preference for a certain type of *model* and that it is the model that is brought to the empirical road-test. The model's claims are compared with the real situation data, and even if it does not perform well, it should not be rejected.

Nonetheless, by preferring CCAPM over the multi-factor models, Cochrane, implicitly, also chooses a specific theoretical framework - one that is supported by his programme's fundamental, core, principles and auxiliary, belt, assumptions. This contrasts to the factor-based pricing models which do not use the same principles and assumptions. We may ask, therefore, whether it is the theories and not the model that are being compared with the real situation data. Here is the answer: "Statistical testing is one of many questions we ask in evaluating theories..." (Cochrane 2005, p. 305). Surprisingly, Cochrane here focuses on the testability of a *theory*. Analogue to what I said above on *models*, it is the theory that is brought to the empirical road-test and tested against data. Even if does not perform well, it should not be rejected.

This is confusing. We are here confronted with a situation in which Cochrane lets both models and theories be subject to "evaluations", i.e. "litmus-tests". In earlier chapters of this thesis, I concluded that there is a difference between the two. For the purpose of the discussion here, recall that Cochrane told us that the analytical cases are derived from the *central asset pricing formula*. Within this hierarchy of detailed analytically special cases, we find a few that represent the empirical situation. I indicated that the risk free interest rate is such a case. Other analytical cases do not represent well. My favoured example is the utility function with its risk aversion and subjective discount factor.

My conclusion is that the complex of fundamental, core, principles and the specific, belt, assumptions in one form or the other are represented in these analytical cases. Following the HD account I introduced in *Chapter Four, section 4.3*, I suggest, therefore, that the analytical cases are used as a basis for making explicit predictions concerning financial market prices, valuations and returns. Since these predictions, or claims, are deduced from various hypotheses as reflected in the fundamental principles and supported by various background situations, they contain theory, empirical concepts, as well as the structural relations within the model. As a consequence, it is particularly difficult to say decisively which part of the asset pricing research programme that is being compared with the real situations.

In the previous *Chapter Four, section 3.2*, we recall that Hausman suggested that the point of a model lies in the "...conceptual explorations in providing the conceptual means for making claims that can be tested and can be said to be true or false." (Hausman 1992, p. 78). I think Hausman is telling us something right. At the end, it is the claims that a model makes that are tested against the real situations. These claims are, for example, regarding realized stock returns, interest rates, and the size of the equity risk premium. No claims, however, related to the rationality of the investor or his/her self-centeredness are made. These are assumed to be installed a priory.

In the end, I think we can follow Lakatos idea, i.e. it is the research programme as a whole that is brought forward to the empirical situations and not its individual elements. In this set-up, models are assembled with the help of the core and the belt. Then the claims of the model are tested against the real situation data. If these claims are false, the research programme is modified in accordance with the programme's heuristic by protecting the core and altering the belt. Then a new empirical test is run in the hope to achieve a better result. If this should be the case, the programme can be characterized as progressive. The models and the theories are thus tested directly while the research programme is tested indirectly.

4. CCAPM, the challenges and a possible solution

Having established Cochrane's belief that the CCAPM is the right starting point and that its theory and model supported predictions are simultaneously tested against the real situations, I can continue my discussion. The topic I now want to address is related to the main challenges Cochrane's research programme encounters when taken to the data. In the following three sections, I address the challenges one at a time. The *first* points towards the realisticness or realisticness of the assumptions underlying Cochrane's analytical cases. The *second* questions whether satisfactory predictive results are too difficult to achieve because the world is not as "ordered" as Cochrane wants it to be. *Third*, and finally, I ask whether the model-based predictions are claims about *tendencies* and not as Cochrane presents them, point-forecasts. When discussing these three points, I let my discussion be informed, primarily, by Milton Friedman and Nancy Cartwright.

4.1 Idealizations, fictions and unrealistic assumptions

My concern here is related to the tension between model-world and the real-world. The model-person, for example, is troubled with his/her first order related decision related to his/her preferences for risky assets that come with a an uncertain, contingent, future pay out. In contrast, the real individual has other interests. He/she is a family person. There are many of them. Their lives are lived in several socio-economic spheres. Most of their activities unfold outside the domain of the financial markets. They might be engaged in social and cultural networks. They do not only care about consumption and investments. They do not buy Arrow-Securities but stocks in companies like Google and Caterpillar. And they do not know the expected statistical distribution of future economic events and payoffs. Now, how can Cochrane reconcile these two worlds? The problem he faces is twofold; first, he knows that the model-world is constructed upon idealized and fictitious assumptions, and, second, he knows that he has not been able to make accurate predictions concerning actual values in the real situations, i.e. point-forecasts with this specific type of model view.

My focus in this section is, therefore, on the ideal and fictional assumptions that Cochrane uses and their role in the development of the consumption based asset pricing model. These assumptions change or distort some particular features of a concrete object. Near to perfect foresight, first order condition, equilibrium situation, and eternal life spans are my favoured examples. Such idealizations and fictions are thus different from an abstraction. An abstraction emerges with its relevant features intact for the task at hand after irrelevant features have been subtracted. The triad of temperaments are thus an example of an abstraction. Fictions, therefore, depart from the truth while abstractions retain their truth-value. I argue that idealized and fictitious assumptions create unrealistic pictures of the real situations they seek to explain and predict.

Does Cochrane care about whether his asset pricing assumptions are realistic or unrealistic? As far as I can see, he makes no explicit references to answer this question. Nonetheless, it is safe to assume that this topic is on Cochrane's mind:

> "We can use the theory positively, to try to understand why prices or returns are what they are. If the world does not obey to a model's predictions, we can decide that the model needs improvement. However, we can also decide that the *world* is wrong, that some assets are "mis-priced", and present trading opportunities for the shrewd investor. This latter use of asset pricing theory accounts for much of its popularity and practical application." (Cochrane 2005, p. xiii).

Besides using the terms "theory" and "model" as if they are the same, Cochrane in this quote, confirms "predictability" is the litmus-test according to which a model shall be judged. This practice of developing theories and models to support the advancement of scientific knowledge puts Cochrane firmly within the standard framework of other scientific endeavours and the consumption based research programme. His view, however, that the world might be wrong is adventurous. One consequence thereof would be, for example, that the observed historical equity risk premium may be regarded as wrong while the modified CCAPM including habit-persistency is right despite its unrealistic assumptions. If this view is upheld, all statistically rejected asset pricing models are right because none of them "did away with" the equity risk premium. In fact, the shrewd investor could feel inclined to follow the advice of a model that predicts a zero, or even negative, equity premium. This investor's portfolio would contain stocks in the anticipation that all other market participants would, sooner or later, discover the under-valued asset class and flock in and bid the equity prices up. This would earn the initial investors a healthy holding period return. Many investors, however, have suffered considerable financial pain because they fell in love with their idea that markets are "wrong" and would correct from an undervalued price level and return to the "right" price.

I think, therefore, Cochrane would be better off dismissing his own suggestion that the pricing model built on unrealistic assumptions could be right and the market, i.e. the empirical observations are wrong. And, with little doubt, I think he agrees. This would bring Cochrane back to the drawing board in order to ponder over why his model claims do not explain the real situations well. One insight might be that Cochrane decides that the model needs a different or an altered set of fundamental, core, principles and auxiliary, belt, assumptions than those currently put to work because they do not give good statistical results. A good example of Cochrane's response to inadequacies of the CCAPM's ability to make precise forecast concerning actual values was found in the way the habit-persistence argument was added to the power utility function. In this case, the fundamental, core, principles were left unchallenged and more realisticness was embedded in the *central asset pricing formula*. I think this added realisticness acts at two levels. First, habit-persistent driven consumption is in itself a realistic assumption. It is observable in the data and could be confirmed by introspection (we care about the Joneses' don't we?). In addition, it tells a good *story*. The second level of added realisticness is related to the asset pricing model itself. When a realistic assumption is included into its utility function, the whole model becomes more realistic. It, therefore, increases the completeness of the model. Since the predicted results modestly improves on the accuracy of empirical tests performed with the original CCAPM, the research programme has experienced both a "theoretically and empirically progression" – to use Lakatosian terms.

Unrealistic assumptions are also a topic in a much referred to publication by Milton Friedman in 1953. Sixty years hence, this text remains remarkably influential when the topic of "method" is discussed amongst economists and philosophers of science alike. [58] It will inform my discussion on the use of unrealistic assumptions in theories and their models. In the context of my analysis, I focus on a few particular aspects around the empirical model-claim tests and their assumptions. I am thus interested in how Cochrane's research approach "fits" with a few central arguments found in Friedman.

[58] A good summary is Uskali Maeki's *The Methodology of Positive Economics* (2009).

According to Friedman, a scientific theory should be judged only by its predictive success within the particular domain in which it is intended to be used. At the time of writing in 1953, it can be assumed that Friedman honestly thought that such success could be achievable:

> "Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain". Only factual evidence can show whether it is "right" or "wrong" or, better tentatively "accepted" as valid or "rejected". As I shall argue at greater length below, the only relevant test is the *validity* of a hypothesis is a comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted..." (Friedman 1953, p. 8).

Friedman tells us that theories, which also Cochrane believes to be a complete and integrated set of assumptions, are brought directly to the real situations:

> "...the relevant question to ask about the "assumptions" of a theory is (...) whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works." (Friedman 1953, p. 15).

Unrealistic assumptions, which we can read as being "untrue", are used in all sciences. In fact, Friedman claims that: "...to be important (...) a hypothesis must be descriptively false in its assumptions." (Friedman 1953, p.14). The fact that unrealistic or even false assumptions are used to create these theories should not, according to this view, undermine our confidence in them.

Friedman's position is often referred to as "instrumentalism". There are several interpretations of this influential "methodology" (Maeki 2009). According to one of these views, which I adopt for the purpose of the following discussion, theories are used as instruments or tools to develop testable hypothesis. Provided that such predictions are statistically successful, the individual assumptions underlying the theory were of little relevance. Theories, as a consequence, are judged by the quality of their predictions in their designated areas, and not by the realisticness of their assumptions. Solutions to practical problems are, therefore, the main thrust – and not their "truth-value". We also note that single assumptions are not the subject of comparison with the real situation data – theories are, and theoretical hypothesis should be rejected whenever the predictions were contradicted. [59]

According to this brief stylized view, we could interpret Friedman to advice Cochrane not to be overly concerned regarding his collection of unrealistic assumptions spanning from the representative investor's near to perfect foresight via zero taxes and transaction costs, to immortality. As long as the original and the new habit-persistent CCAPM versions and it theories predict well in their designated areas, unrealistic assumptions can be considered secondary to the research project. They are in other words, "sufficiently good approximations for the purpose in hand".

In contrast to Friedman's view, I think Cochrane should care about the assumptions he deploys. They also matter when the predictions are inaccurate relative to the real situation data. Cochrane seems to agree. He included habit-persistency in the original CCAPM in light of its predictive failures. I suggest however, that Cochrane's idealized and fictional assumptions distorts the real situations and are unable to account for and capture the complexities of the realm in which he develops his theory. But this is deliberate Cochrane tells us. He needs to reduce complexity in order to make his theory manageable when making point-forecasts in the designated area.

But Friedman's instrumentalism and advice put Cochrane under pressure. What shall he do when the consumption based pricing model fails to give acceptable predictions related to the real situations? Should Cochrane's asset pricing research programme be rejected based upon its unsuccessful statistical test results at a particular level as exemplified by the equity risk premium puzzle?

[59] Of course, it is challenging to test an assumption by its own. Consider, for example, Cochrane's notion of the first order condition. This assumption does to emit a predictive implication on its own but is interconnected with several other assumptions.

As we know from *section 3.1* earlier in this chapter, Cochrane clearly believes in the merits of the consumption based asset pricing research programme. It is the only and the correct starting point for any research into the behaviour of asset prices and their behaviour. To Cochrane, a failure to make accurate point-forecasts in specific situations is not deemed as a failure of the whole research programme. It is, therefore, not the programme that is under review, but more the analytically convenient special cases that were fitted out to address a specific case. We recall the following statement:

> "Unfortunately, this specification of the consumption based model does not work very well. (...) As you can see, the model is not hopeless – there is some correlation between sample average returns and the consumption based model predictions. But the model does not do very well." (Cochrane 2005, p.43).

Taking this view, i.e. comparing the accuracy of the claims made by the analytical cases and even see them unsuccessful, leaves Cochrane undisturbed with respect to a request to abandon the consumption based asset pricing programme on the grounds of rejected claims. A failure in a part of the programme is even to be expected, Mill told us. And I think he is right. An isolated failed empirical test should, as a consequence, not question, in a serious way, the whole. I, therefore, believe Cochrane could, on one hand, follow Mill's and Lakatos' advice and continue to explore innovations to the theoretical structure that can bring more statistical success home when the analytical cases do not perform well in a first empirical test-run. He has already demonstrated how this can be done with regards to the adoption of the habit-persistence argument. Nevertheless, on the other hand, Cochrane might want to tone down the dominance of CCAPM's point-forecasting abilities as we saw, for example, in the study of the equity risk premium puzzle. I will have more to say about this topic in the upcoming *section 4.3*.

In sum, Cochrane tells us that a progressive asset pricing research programme rests on underlying theories concerning ideal and fictitious assumptions with respect to the investor, the markets and the assets. He also tells us that the consumption based asset pricing programme should be evaluated by the statistical success of its model-based predictions. I argue that such assumptions are unrealistic because they distort their real situation counterparts. Friedman, however, tells Cochrane not to worry because every theory uses unrealistic assumptions. What matters is that the predictions do well within the designated area. But the problem is that Cochrane's idealized and fictional analytically convenient special cases do not perform well when confronted with the real situations – as was demonstrated with the equity premium puzzle case.

In light of the predictive deficiencies, I think Cochrane's option number one, as pointed out earlier in *section 2.5*, is to try even harder within his chosen a priori mathematical deductive methodological approach, relying on a few fundamental, core, principles, and continue to adjust the auxiliary assumptions situated in the "belt", to improve the point-forecasting abilities of his analytical cases. The second option, I think, is to argue that the model-based claims are not what they are perceived to be, i.e. point-forecasts of asset price behaviour as shown, for example, in the historical equity risk premium puzzle case. The first option is the topic for the next *section 4.2*, and thinking about the second option is the topic in *section 4.3*.

4.2 A complex socio-economic world

Cochrane has thus far defended his version of the CCAPM as the best alternative when exploring asset price behaviour, accepted that several of his assumptions are unrealistic, and advocated that such assumptions should not stand in the way of scientific progress. All these components are, however, questioned by the fact that neither the original nor the new, habit-persistent CCAPM, so far, have rendered statistically significant results in their designated areas. Let me first let Mill remind us before I turn to a modern date writer who points to similar topics:

> "We cannot try forms of governments and systems of national policy on a diminutive scale in laboratories, shaping our experiments as we think they may most conducive to the advancement of knowledge. We, therefore, study nature under circumstances of great disadvantage in these sciences; (...); in circumstances, moreover, of great complexity, and never perfectly known to us; and with far greater part of the process concealed from our observation." (Mill 1874, E5, V51).

Can it, therefore, be that John Dupré is right when stating:

> "In short, the failure of economics to predict future phenomena might reflect not merely the inability of economists to take account of the numerous factors impinging on economic reality, but rather the inherent unpredictability of the phenomena concerned." (Dupré 1993, p. 8).

This is a radically different view than those encountered earlier in Mill, Lakatos, Friedman and Cochrane. Dupré here questions whether there is, at all, an underlying order in the social realm in general, and economics in particular, that renders itself to investigations along the lines economists proceed.

Dupré suggests that economists may respond to his challenges in two ways. Either they reveal themselves as "empirical optimists" or "metaphysicists". Dupré immediately rejects the latter in his short text mentioned above and refers to his book *The Disorder of Things* (1993a), in which he argues against such a response. I will not discuss Dupré's elaborations on metaphysics here, but move on to his views related to "empirical optimism". He tells us that we can certainly try to improve model specifications and hope for more accuracy in empirical test results. This effort, however, can best be characterized, as Dupré calls them, "blind faith" and "forlorn hope". Given the relevance of these comments, let me explore "empirical optimism" in the context of Cochrane's asset pricing research approach.

I claim that Cochrane is an empirical optimist. His research project starts out with the observation of phenomena in real situations. It is a fact that individuals buy and sell stocks in financial markets, and that the outcome of such interactions manifest itself in collectable asset prices. It is also a statistical fact that stocks, over a long time horizon, have returned more than investments in risk-less assets such as US Treasury Bills. It is also an observed fact that investors consume. From these various activities in and around financial markets, Cochrane pulls together *stories* which he makes explicit in asset pricing theories and models. As I have pointed out earlier, there is no doubt that Cochrane believes in his profession's effort to uncover main mechanisms that generate the observable data. I exemplified this in my earlier discussions around the re-engineered extensions of the utility function to incorporate the term habit-persistence as developed in the context of the Campbell-Cochrane (1999) enhanced CCAPM.

Cochrane, of course, knows that the representative investor is a theoretical construct. He furnishes him/her with two main behavioural drivers, i.e. rationality and self-interest, and one counteractive, i.e. risk aversion. In this respect, Cochrane idealizes, deforms and even fictionalizes the real investor's many sentiments. Cochrane's approach, therefore, wants to capture the non-random, or systematic part of individual behavioural causes, and then explore, in a mathematical rigorous way, the consequences of the individual's decisions. When those consequences are derived, the results are compared to the real situations. Implicitly, Cochrane assumes that the representative investor behaves like the real investor. The hypothesis is thus that the triad of temperaments plus habit-persistency explains the behaviour of an individual in a real situation. As we know, this hypothesis is rejected. Cochrane, I believe, is neither surprised nor disappointed. It was just a first approximation. More theoretical and modelling work in the form of reverse-engineering is thus needed and well supported by the heuristic of the research programme.

But what does a first approximation exactly mean? In principle, it envisages a final grand-solution that can explain and predict all asset prices along these lines: "The consumption based model is, in principle, a complete answer to all assets pricing questions, but works poorly in practice." (Cochrane 2005, p. 41). This ambition requires ontological and methodological commitments. And it seems that Cochrane is willing to sign on both counts.

Cochrane thus demonstrates *first*, the ontological commitment towards the special domain of financial markets within the field of economics. Markets exist, they are ordered, asset prices are emitted from this isolated "world", and they give the foundation for further exploration. *Second*, Cochrane also shows methodological commitment that follows a stylized HD account as I showed earlier in *Chapter Four, section 4.3*. This commitment, as we know, goes beyond the relative pricing models presented by financial economists like Eugene F. Fama and Kenneth R. French encountered in *Chapter Two,* *section 1.3*. Cochrane seeks as the NBER charter advises him, to "understand and measure the sources of aggregate or macroeconomic risk that drive asset prices" (see *Chapter One, section 1.3*). Despite these commitments, the CCAPM has not yet reached its zenith. This raises two main topics: If the *first* approximation from these commitments is wrong, how can Cochrane justify continuing within this particular framework, i.e. "try a bit harder", and hope that the result will be absolutely or approximately right? *Second*, is the socio-economic world inherently unpredictable, as Dupré suggests, in the sense that it does not lend itself to the order Cochrane seeks to impose on it?

Earlier I established Cochrane's view that the CCAPM is the "correct" starting point for asset price research. This locks him into an ontological commitment that seems to imply "trying a bit harder" inside that paradigm would lead to a final break-through with respect to explaining and predicting the behaviour of stock prices. "Trying a bit harder", however, does not always lead to better results.

This is in particular the case if what so far has been done has not led to improvements. Can it be that Dupré is right after all? Tony Lawson claims that, over the past fifty years, "contemporary orthodox economics" has made little progress. He identifies the main reason for this in the use of a deductive approach that practicing economists borrowed from the natural sciences:

> "According to deductivism (as I shall use the term here) to be able to explain something is to deduce it, or a statement of it, from a statement of initial and boundary conditions plus universal laws of the form "whenever event (type) x then event (type) y." (Lawson 1994, p. 259).

He continues: "Just as the persistent search for event regularities of a probabilistic kind characterizes econometrics, so the positing of strict constant conjunction is a condition of axiomatic-deductive "economic theory." (Lawson 1994, p. 260). Lawson is not particular fond of what he here alludes to, i.e. deductive mathematical reasoning that presupposes a "constant conjunction" ontology. [60] He advocates a modelling approach that takes the

[60] "Constant conjunction" expresses a view that events co-occur, i.e. event regularities of the form "whenever event x then event y". These event regularities are not necessarily "universal". They can also be

causes as having real *tendencies*. I think Cochrane could subscribe to Lawson's suggestions, and I will explain why in the upcoming *section 4.3*.

The problem Lawson has with the "orthodox economists", and I think we can count Cochrane to that particular pool, is that they do not pay enough attention to the ontology of the phenomena they set out to research. There is little attention, Lawson claims, towards what the ontology of the "nature of social material" truly is. This limits our understanding of the phenomena under review, i.e. asset prices, and what kind of knowledge that can be extracted. His solution, therefore, is to take a "realist" stance. This approach would reveal that the social context is not founded upon "constant conjunction" regularities in the data, and an a priori reasoning supported by deductive mathematics. On the contrary. Lawson's view implies an ontological commitment towards terms such as unobservable "structures", "mechanisms", "powers", "tendencies", "capacities", etc.

Again, as pointed out above, I think Cochrane's modelling approach is not too different from what Lawson advocates. Certainly, I cannot find any commitment from Cochrane towards the concepts that Lawson mentions. Nonetheless, Cochrane does not just write down an abstract set of axioms and make deductions. Cochrane does "causal modelling". He starts with agents of a certain type, i.e. representative investors and let the triad of temperament plus habit-persistence dictate their behaviour and actions. Then he adds in more causes to get more concrete and realistic models. Cochrane calls these more specific models analytically convenient cases. They aim at stretching themselves all the way to the real situations, i.e. their goal is to give empirically useful representations. Beside Lawson, as I showed in the previous *Chapter Four, section 4.4.3*, also Cartwright advocates this type of causal modelling. However, Lawson is not fond of the specific set of initial causes.

In a more recent contribution, Lawson (2009) proves himself to remain sceptical towards the "orthodox" economics methodological approach. When analysing the current

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probabilistic. Most models applied to data in the socio-economic sphere are structured to account for probabilistic events.

economic and financial market dislocations, which I have mentioned elsewhere in my thesis, he claims:

> "For the most fundamental problems of recent years, I shall argue, is not so much the use of specific inappropriate models, but the emphasis on mathematical deductivist modelling *per se*. Such models can provide limited insight at best into the workings of the economy. Indeed I will suggest that the formalistic modelling endeavour mostly gets in the way of understanding." (Lawson 2009, p. 760).

Lawson thinks that this view is defendable, and I share his view:

> "...with a bit of reflection both on the nature of social reality, and also on the sorts of conditions that must hold for the mathematical method in question to have utility, we can not only better understand and explain the failings of the latter methods in the hands of mainstream economists, but also recognize that such methods are unlikely very often to provide insight no matter what substantive economic theories are used in their construction". (Lawson 2009a, p. 126).

Unfortunately, this is not the place and time to continue exploring these interesting views in any further detail. The point I want to make at this stage, however, is a crucial one. On one hand, the socio-economic context might be more complex than Cochrane's *central pricing formula* and analytical cases can hope to reveal. Both Dupré and Lawson, albeit from different perspectives, point towards this possibility. There might, as a consequence, be some compelling arguments in favour of abandoning the "constant conjunction" view that many financial economists, including Cochrane, hold for a "realist" point of view. On the other hand, empirical optimism cannot in the case of Cochrane, stem from doing more of the same. Because such optimism is only warranted if Cochrane can show that his modelling efforts have improved. So far, however, this track-record is limited, to say the least. This result should play well into Lawson's court and argue for Cochrane to move away from his ontological and methodological approach.

The *second* topic I alluded to above besides "trying harder" within the contemporary orthodox economic paradigm is related to the "complexity" of the socio-economic world view. I showed earlier in *Chapter Three section, 1.3.1*, how Buchanan and Hendry, but also Solow earlier in this chapter, argued for a simplification strategy when facing complexities. By now we also know that Dupré points towards the difficulties in doing science in such a world. Here, I want to address another view that pulls in Dupré direction. It is that of Cartwright. She also has a vision of the real world as we encounter. It is a "dappled world" she says, and claims:

> "This book supposes that, as appearances suggest, we live in a dappled world, a world rich in different things, with different natures, behaving in different ways (...). For all we know, most of what occurs in nature occur by hap, subject to no law at all. What happens is more like an outcome of negotiations between domains than the logical consequence of a system of order." (Cartwright 1999, p.1).

She speaks of complexities, events and domains. Financial economists, I have argued, think along the same lines. Their strategy is to separate out the economic "domain" and explore the consequences of the representative investor's character and directed behaviour. Within the confines of the financial market structure and with the support of financial assets, a partial equilibrium situation can be established. Although financial economists in their research effort do not underestimate the challenges arising from the complexities of the underlying real situations, they seem to believe "trying a bit harder" can help them understand and "control" such environments. And control leads to policy advice. As we have seen, Cochrane holds such a view. But what are the consequences?

Cartwright gives advice: "My belief in the dappled world is based in large part of the failures of these two disciplines [*physics and economics*] to succeed in these aspirations. The disorder in nature is apparent." (Cartwright 1999, p. 1). Cochrane's strategy of simplification with respect to the investor, the markets and assets, seek to impose the order that Cartwright claims is not obvious. Dupré lends support by claiming that:

> "... the suggestion that the failures of economics derive not merely from excessively simplistic assumptions, crude theories of human nature, and so on, but rather from a fundamental mismatch between the kinds of phenomena with which economics is concerned, and widely held conception of what it is for an investigation of any realm of phenomena to be genuinely scientific." (Dupré 1993, p. 2).

So far, Cochrane is the first to admit that his microeconomic based approach to asset pricing has not delivered on its promise to give knowledge for use. While he leaves ontology and methodology unquestioned, fellow economists and philosophers alike urge him to rethink. Can it be that Cartwright and Dupré are right? And if yes, how can Cochrane proceed?

4.3 Point-forecasts or claims about tendencies?

Cochrane's research programme thrives on the idea that observable asset prices and their macroeconomic counterpart, i.e. consumption data co-vary. This covariance can be taken as a manifestation of some underlying socio-economic processes. Detecting such processes may, therefore, uncover the thus far hidden truth regarding the relationship between asset prices and investors' behaviour. For this purpose, Cochrane develops a theoretical framework of ideal and fictional individuals, markets and assets in a separate realm. This theorizing and their concepts rest on unrealistic assumptions. They are made explicit in applied mathematical models. Deductive mathematical reasoning is thus used as the language to establish internally consistent models. These models are thus extensions of the theoretical framework – as Daniel M. Hausman maintains. (see *Chapter Four, section 3.2)*. But at the same time, the consumption based asset pricing model is also meant to capture the perceived fundamental, non-random causes that act on people's decisions and, ultimately, assets prices. As such, the models are also set up to account for real situations – as Milton Friedman states (see *section 1.2.4*).

The idealized and fictional consumption based asset pricing model is, therefore, considered resembling the real situations in some significant ways. This importance is captured in the fundamental principles, i.e. triad of sentiments, i.e. rationality, self-interest, risk aversion, plus, new, habit-persistency. From the triad plus habit proposition, the belt of auxiliary assumptions guides the theory towards very specific analytical cases that, unfortunately, do not make accurate predictions concerning actual values in the real situations they are asked to address. Apparently, stock prices, excess returns, and equity risk premia are influenced by other causal factors as well. John Stuart Mill called them the “disturbing causes”:

> “When the principles of Political Economy are to be applied to a particular case, it is necessary to take into account all the individual circumstances of that case; not only examining to which of the sets of circumstances contemplated by the abstract science the circumstances of the case in question correspond, but likewise what other circumstances may exist in that case... (...) These [*latter*] circumstances have been called *disturbing causes*.” (Mill 1874, E5, V58).

The socio-economic sphere is indeed “dappled”, “complex” and prone to be “disturbed” by a multitude of interacting causes. On these points, I side with Cartwright, Dupré and Mill. Cochrane’s option to continue along his methodological path of deductive mathematical reasoning and “trying a bit harder”, therefore, seems to have come to an end. On this, I agree with Lawson. It is questionable, therefore, how helpful a research programme is when it relies on isolated, idealized and fictional elements and deductive reasoning when its aim is to target predictive accuracy in a specific designated area. Stakeholders should at least consider this question when seeking knowledge for use in, for example, a policy setting context.

In this section, I continue discussing how Cochrane’s research programme can be interpreted in light of its lack of statistical success at the level of specific analytical cases, such as the one seeking to explain the equity risk premium puzzle – or stock market prices and their excess returns over the risk free interest rate for that matter. I am now pointing towards the second option that I brought up at the end of *section 4.1*. There, I asserted that model-based claims should possibly not be viewed as point-forecasts of asset price behaviour. Can it be, I ask here, that point-forecasting stock prices, excess returns, risk premia etc. are overly ambitious? If yes, what is the alternative? I will start answering this question with a few arguments rendered in John Sutton’s fine little book *Marshall’s Tendencies* (2000), and put them in the context of Cochrane’s asset pricing model. The philosophical discussion will, again, be informed by Cartwright’s suggestions towards the end of this section.

In Sutton's discussion of Alfred Marshall's analysis of *tendencies*, he makes references to the natural phenomena of tides observable in some coastal areas:

> "The tides are affected by two different influences. The primary influence lies in the gravitational pull of the moon and the sun, and this contribution can be modelled with great accuracy, but the tides are also affected by meteorological factors, and these are notoriously difficult to predict." (Sutton 2000, p. 5).

Now, Sutton goes on to argue that a similar set-up has been used by economists to explain economic phenomena. I would add that financial economists such as Cochrane as well as Fama and French are no exception. First, economists seek to capture the primary causes of observed phenomena and make them "explicit" in a model-based theoretical framework. When predictions fail to confirm the workings of the empirical situations, they, next, add in secondary influences whenever necessary to improve the predictions.

Tides, it turned out, were easier to forecast than the behaviour of economic agents. In both cases, however, the grand idea was to develop a complete and deterministic model-based upon a few justifiable fundamental principles plus some auxiliary influences. Sutton reminds us that when using probabilistic models, we would often refer to the systematic part of an equation, on one hand, and its error term on the other. While the systematic part as captured in the fundamental principles was considered to be truthful representations of the real situations, the error term would address the non-systematic influences. With hindsight, this framework is not particularly impressive. Yet, it carries a strong ontological commitment towards the world being an ordered place that can be explained in a simplistic way. And not only that. The belief, which is still widely held, and also shared by Cochrane, is that fundamental, core, principles and auxiliary, belt, assumptions exist, can be identified, and measured. We certainly encountered a good example of this attitude in Cochrane when he added the habit-persistency argument to the power utility function.

I would not argue against the possibility that such ideal assumptions reflect real situations and mirror properties and relationships. My scepticism is directed towards the identification and measurement aspects. Consider again the analytical case of risk correction that Cochrane argues is a main component of investors' fear when buying stocks (see *Chapter Four, section 3*). Risk aversion is certainly something to which we can relate. Financial economists, however, have chosen to consider it as a main causal factor behind investor's behaviour. But how can it be identified, measured and transported across various situations and through times? What about the entirely opposite sentiment of greed? How is greed related to risk aversion? What are their relative strengths in influencing behaviour, and, ultimately, stock prices? So far we have no conclusive answers to these fundamental questions.

As we were exposed to in the previous section, Lawson suggests that "trying harder" within the confined field of so-called "orthodox economics" cannot be the right approach to answer the above questions. We might, therefore, need to lower our expectations with respect to coming up with a "complete" model of asset prices. Cochrane's claim that his version of the CCAPM can, "in principle give a complete answer to all the questions of the theory of valuation" might, as a consequence, be overstretched – in particular in light of the predictive failures so far. While awaiting the results of such "orthodox economics" and "empirical optimism", we might ask them, in the mean-time, to consider reviewing their model-based claims. We might even ask whether they consider these claims to be of the "right kind", i.e. such that give knowledge for use by the various stakeholders.

Yet, while scientific progress in tides forecasting has been steady, little progress has been made in the field of economics. Can it be, Sutton asks, that: "...the most we can expect of economic analysis is that it captures the "tendencies" induced by changes in this or that factor?" (Sutton 2000, p. 4). This question points towards the subtitle to his book, i.e. "what can economists know?" In some areas of mathematical deductive modelling, Sutton argues, a lot can be learned. He points, for example, towards Black and Scholes "option pricing model". [61]

[61] This model does not involve any theory of the investor, and it does not need any of the fundamental core principles such as those in use by the CCAPM to back out a particular asset price. In fact the model calculates the option price *given* the price and price volatility of the nearby assets, i.e. the underlying security, and other given variables such a risk free interest rate and investment horizon. My point,

In other areas of economics and finance, less can be learned from such standard models because the underlying reality does not give away its secrets to the mathematical structure imposed upon it. In such situations, an alternative approach is required. Sutton argues, therefore, in defence of a "pluralistic approach". It implies putting less emphasis on the standard paradigm of orthodox economics, such as rationality, when analysing social phenomena. In some ways, the pluralistic approach echoes Lawson, who argues that empirical regularities in the economic-sphere are too few and too far apart to be of help in developing knowledge for use. What economists can hope to know is, therefore, more limited than what Cochrane aims for.

But Cochrane seems to prefer working within the deductive tradition by focusing on a "one analytical tool", keeping "analysis simple and elegant" applying "simple and compelling logic". Nonetheless, as we saw earlier in *section 2.5, footnote 57*, some moderation has crept into his thinking. Those statements are certainly interesting in the context of our discussion. Here is a broader representation of Cochrane's more recent views.

Referring to a debate which reflects on the merits of unconditional and conditional forecasting in economics and finance, Cochrane argues pro "hedgehogs" and against 'foxes": [62]

> "Milton Friedman was a hedgehog. And he got the big picture of cause and effects right in a way that foxes around him completely missed. Take just one example, his 1968 American Economic Association presidential speech, in which he said that continued inflation would not bring unemployment down, but would lead to stagflation. He used simple, compelling logic, from one intellectual foundation. He ignored big computer models, statistical correlations, and all the

therefore, is that very little can be learned about the derived option price because we are not given access to why, for example, the stock price behaves as it does.

[62] Cochrane says that unconditional forecasting is using "historical correlations to guess what comes next with no need of structural understanding" while conditional forecasting predicts "the answers to questions such as "if we pass a trillion dollar stimulus, how much more GDP will we get next year?", and "if we tax the rich, how much less will they work?". Cochrane is advocating the latter, conditional, forecasting strategy, because "here we are trying to predict the effects of a policy, how much will the future change if a policy is enacted".

> muddle around him. And he was right. (...) Good hedgehogs stick to a few core principles because they know that nobody really knows detailed answers. Principles matter. They produce wiser conditional forecasts. (...) [*while foxes used*] "...a wide assortment of analytical tools, sought out information from diverse sources, were comfortable with complexity and uncertainty, and were much less sure of themselves...they frequently shifted intellectual gears (...) People who use a wide range of analytical tools, mixed economics, political, sociological, psychological, Marxist-radical and other perspectives end up hopelessly muddled." (Cochrane 2011a).

This statement challenges some of Cochrane's earlier references to what he presented as the classical cases in finance and how they can be approaches in the context of the analytically convenient special cases and their empirically useful representations. In those cases brought to us in a fox-like manner, I found how a myriad of auxiliary, belt, assumptions were tagged onto the fundamental, core, principles in order to derive, mathematically, the outcomes. Now we learn that Cochrane prefers the hedgehog's big idea of sticking with a "few core principles" rather than the foxes' "hopelessly muddled" strategy.

I think Cochrane is wise enough to choose his "horses for courses" and that both approaches are, in different situations and for different purposes, warranted. My focus is, therefore, on a related topic. I ask what he can learn from being a hedgehog focusing on the "big picture" of "core principles". My point is that he can apply them to identify *tendencies* in the real situations. *Tendencies*, in this respect, can be related to his statement that "economics is pretty good at such structural forecasting. (...) At least we know the signs and general effects. Assigning numbers is a lot harder". But how can he use "the general effects"? I suggest that Cochrane can see them as claims regarding *tendencies* – as Sutton discusses in the context of Marshall. Such *tendency* claims are not applied to find out what happens in very particular circumstances because of Mill's disturbing causes. But they will get the "sign" right. Let me address my suggestions in the following paragraphs of this section.

We have been exposed to claims about *tendencies* earlier in my thesis in the context of Maeki, Mill, Cartwright and Lawson. Take, for example, Mill:

> "Doubtless, a man often asserts of an entire class what is only true of part of it; but his error generally consists, not in making too wide an assertion, but in making the wrong kind of assertion: he predicated an actual result, when he should have predicated a tendency to the result – a power acting with a certain intensity in that direction." (Mill 1874, E5, V75).

In the previous *section 4.2*, Mill reminds us about the "great complexity" found in the Political Economy and that their underlying processes are "concealed from observations". Cartwright, Lawson, but also Dupré, we recall, painted a similar picture of the modern socio-economic world. But Mill's main concern in the above extract is what we can hope to find in such complex situations and what kind of inferences we can make. His answer is that we should concentrate on identifying and extracting "powers" from the real situations, and from there, use them to make claims about *tendencies*.

I also sense that such *tendency* claims might be portable across a wider range of situations. Can it be that Cochrane is well advised to reconsider his research project in light of Sutton's and here, Mill's suggestions? Instead of insisting on particular point-forecasts in a designated area, should Cochrane rather say that the CCAPM research programme aims at making claims about *tendencies* in the real situations? Can it be that the triad of temperaments previously used in the original CCAPM plus the habit-persistence in the new CCAPM points him in exactly that direction? Can it be, should Cochrane choose to pursue this suggestion, that such effort remains within the research programme's heuristic and, in addition, leads to theoretical and empirical progress? Let me explore tendencies in more detail before I seek answers to those questions.

What makes a *tendency* claim? Cartwright gives guidance:

> "I suggest that it [*the "law" of electron-electron repulsion*] says that electrons – because they are electrons – have the capacity of the given strength to repel other electrons, where for nice situations we have some rules for how to calculate the results that occur when this capacity operates jointly with others, and where in messier situations we are entitled at least to claim (...) the electron might cause a second to move away. What better alternatives are available?" (Cartwright 2008, p. 135).

Cartwright here points towards the "capacity" that are inherent in the electron to produce certain effects. [63] The electron carries this effect-generating ability across various situations, i.e. both within "nice" and "messier" situations. They are, therefore, not context dependent, but can be seen as something enduring throughout. In the former nice setting, the precise effects of such capacities can be calculated. In the latter messy setting, this is not easily done. Yet, in both situations the electron still has the "capacity of the given strength". However, there are situations in which a capacity exists but is counteracted, i.e. it may completely fail to manifest itself because it is suppressed by other causal factors at work in that particular situation. But, at least in one particular way this is not a problem, Cartwright assures us:

> "To assert (...) that aspirins relieve headaches is to claim that aspirins, by virtue of being aspirins, have the capacity to make headache disappear. (...) Once the capacity exhibits itself, its existence can no longer be doubted." (Cartwright 1989, p. 136).

Capacities, therefore, are real:

> "Capacities are at work in nature, if harnessed properly they can be used to produce regular patterns of events. (...) What makes things happen in nature is the operation of capacities." (Cartwright 1989, p. 36).

Tendencies, as we have seen by Mill, are also real. They can be captured, and their existence in different situations can be tested. Cartwright now suggests that such *tendencies* can be found applying a so-called "Galilean experiment":

> "Galileo's experiments aimed to establish what I have been calling a tendency claim. They were not designed to tell us how any particular falling body will move in the vicinity of the earth; nor to establish a regularity about how bodies of a certain kind will move. Rather, the experiments were designed to find out what contribution the motion due to the pull of the earth will make, with the

[63] Cartwright's use of the term "capacities" is almost identical to Mill's use of "tendencies". (Cartwright 1989, p.224). I take this at face value and will use the terms interchangeably.

> assumption that the contribution is stable across all the different kinds of situations falling bodies will go into." (Cartwright 2007, p. 224).

Cartwright's suggestion is important for at least two reasons. *First*, she informs about the purpose of the Galilean experiment. It was neither used to make suggestions with respect to a particular case, nor aimed at making a regularity claim. The experiment, and this is the important *second* point, established a *tendency* claim, i.e. it found out what the stable contributions from one single cause, in this case the pull of the earth, is on the falling body. Galileo conducted the experiment by eliminating, as far as possible, all other disturbing causes so that he could measure the effect of gravitational pull in isolation.

Now, can we find traces of capacities or *tendencies* in Cochrane's writing? I think the answer is "yes, but". Again consider the fundamental, core, principle that portrays the representative investor as rational, self-interested, risk averse and habit-persistent. Mill tells us, and I agree, that such character traits are engraved in the "whole man". He/she thus "carries" the "triad plus habit" with him/her as he/she proceeds from one choice situation to another – be it to the stock exchange or to the grocery store. Some of these situations might be as Cartwright suggests, "nicer" than others. I suggest that such "nice" situations are few and far apart in the financial market situations given inherent complexities as described by Buchanan's "interdependencies" and Hendry's "evolutions" in *Chapter Three, section 1.3.1*. Since economic choice situations may be considered to be "messier", it is important as Cartwright suggests, to isolate situations of particular interest and figure out the result when only one or a few causes are allowed to be present.

In the context of Cochrane's consumption based asset pricing model, Cochrane has not isolated a single cause, but wrapped three of them together, and then added a fourth. I referred to them earlier as the "triad of temperaments plus habit". He could claim that financial market phenomena are governed by causal factors with stable capacities. Let me, therefore, explore what the consequences for Cochrane's research programme are, if we think of Cochrane's fundamental, core, principles as being able to reveal capacities or *tendencies* of these four factors wrapped together in the real situations in the way Mill and Cartwright propose.

First, I have, so far, given the analytically convenient special cases and their empirical usefulness a lot of attention. These cases carry both fundamental, core, principles and auxiliary, belt, assumptions within them. They are thus established using neo-classical economic theory, but they are also enriched with assumptions borrowed from outside the pure financial market context, and even from every-day concepts. Furthermore, in the real situations towards which specific claims are made, there are many unaccounted for "disturbing causes" that also influence the particular outcome. As a consequence, the model-based claims are not representations of the real situations. They make claims about the model-world. This can be taken as a good reason for re-directing the discussion away from point-forecasts for a real situation setting towards making *tendency* claims about them. It can be argued that such claims are more relevant for the stakeholders than what is today's failed practice, i.e. point-forecasts.

For example, let us assume, for now, that analytical cases and their predictions predominantly are carried by the fundamental, core, principles and that the triad plus habit have the capacity to generate effects, i.e. movements in stock prices, stock market returns, equity risk premia, etc. One can then make statements such as "whenever the triad plus habit operate unimpeded, stock prices will rise". Consider next the risk aversion's role in this statement. Cochrane claims it is an inherent stable human capacity in choice making situations with uncertain outcomes. [64] Risk aversion can have the capacity, in other words, to pull or push stock prices, stock returns or equity risk premia up or down across a wide range of situations, in different countries, and during different time periods. This capacity, however, does not say anything regarding the actual behaviour of stock prices, stock returns or equity premia because of the influence of "disturbing causes". In this sense, the triad plus habit never operate unimpeded. But, as Cochrane argues, getting the "sign" right, i.e. the directional pull or push, is easier than

[64] Julian Reiss makes a similar case with respect to the growth of money in an economy. An increase in the supply of money thus has the capacity to raise the general level of prices. (Reiss 2008).

finding the exact "number" associated with its strength. In fact, this outcome was achieved with respect to the risk aversion parameter in the context of the equity risk premium puzzle (see *Chapter Two, section 6.2*). The risk aversion value is expected to have realization values between 0 and 2, but the predictions point towards a much higher number.

Nonetheless, Cochrane could argue that risk aversion is present in the investor's decision making processes but not to the degree that conforms to the real situations he targets because they are "dappled" and "messy". This argues for the acceptance of making tendency claims in a financial market context. Such claims do not increase the statistical success of the consumption based capital asset pricing effort. It merely acknowledges the complexity of the subject matter and adjusts the expectations towards a more realistic view. Additionally, it would enhance the potential of "knowledge for use" that I see as a crucial outcome of this research programme.

Second, following Lawson, who I introduced earlier in *section 4.2*, the acceptance of tendency claims establishes an alternative way of seeing the asset pricing research programme than what Cochrane has portrayed. Cochrane, or to an even higher degree, Eugene F. Fama and Kenneth R. French, have namely so far been searching for the "constant conjunction" or the "regularities" in the data. The classic topic of "risk correction", in which an investor requires a mark-up, i.e. a risk premium for entering into a risky investment, is a case in point. We recall Cochrane's statement "an asset whose payoff covaries positively with the discount factor has its price raised and vice versa" (see *Chapter Four, section 2.1*). A reference to data-covariance is a clear indication of his "constant conjunction" view. However, I have already objected to this orthodoxy. In this context, Mill resonates well: What Cochrane should have predicted is not "an actual result" but "a tendency to the result". However, we can also use the constant conjunction view positively and claim, as Cartwright does, that regularities in data might originate from underlying causal capacities. It is thus the task of the scientific project to uncover such capacities. If this search is successful, the consumption based asset pricing programme could claim continued theoretical and empirical progression. As a consequence, embracing tendency claims would not hinder the positive development of the research programme. In fact, Cartwright claims: "...knowledge of capacities is more basic in that it is more embracing and more widely useful than knowledge about regularities." (Cartwright 1999, p. 77).

Third, we are told that *tendency* claims are not made regarding particular outcomes but related to what a model's main causes tend to generate as effects. Should the triad plus habit be able to support claims concerning *tendencies* in the real financial market situations, such claims should also be detectable in a range of situations. *Tendency* claims, therefore, should be horizontally portable from one situation to another (see *Chapter Four, section 3.1.5*). Cartwright points out: "I take it that a causal structure is a specific arrangement of features of the world - causes - that act together to produce different effects. We are supposed to imagine experimenting on the various causes in the structure to see how a given variation in a particular cause affects the effect. What do capacities do beyond that? They articulate what the given cause contributes across all possible causal structures, where this will in general be different from the effects produced in any one causal structure by varying the cause." (Cartwright 2008, p. 135). With this intervention, Cartwright argues that capacities are a more fundamental category than causes. They exercise their presence across "all possible causal structures". This can be taken to support the view that investors are, for example, rational, self-interested and risk averse across all possible structures. However, their intensity may vary and even be completely counteracted by other causes.

Fourth, if the triad plus habit principle can be used for making *tendency* claims about real situations across time and places, then the point-forecast focus of the programme can be toned down. This ontological and methodological innovation to the consumption based capital asset pricing endeavour, I believe, is well within the boundaries of a Lakatosian research programme. It uses positive heuristic to encounter the lack of success when point-forecasting and supports the programme's theoretical and empirical progress. With this I do not intend to assert, as I referred to above, that the effort of improving point forecasting should be halted. As I believe in the importance of predictions as a litmus test for measuring empirical progress, this should still be a main focus of Cochrane's research programme. As a consequence, predictive quality and tendency claims may go hand in hand. When, for example, risk aversion turned out to be a positive, albeit too large, the direction has the right sign. What I want to see is a "confidence interval" around this parameter value that would alert its users about "danger ahead" when applied. In this sense, theoretical and empirical progress is still the goal and, as we have seen in the triad plus habit innovation, achievable.

Can we wholeheartedly embrace the concept of *tendency* claim? A few words of caution are appropriate. Earlier, I showed how Cartwright advocates the virtues of the "Galilean experiment" in which only a few dominant causes are isolated in order to analyse their effects in a particular situation. "Details of the situation" and the "background factors", Cartwright suggests, should be eliminated as much as possible. Only then is the experiment "shielded", and its effects can be traced back to the isolated causes. If we have good reason to think those causes *have* stable tendencies (as with gravity, and as is presupposed for the triad plus habit in Cochrane's use of his models) we will be enabled to identify what the canonical effect or contribution is of those causal factors with stable tendencies. If this can be established, the findings can be made, horizontally, portable to other situations, as claims about contributions that will occur. Cochrane's version of the CCAPM research programme has not entered that league yet. He has so far focused upon specific and granular analytical convenient cases. His auxiliary belt, therefore, carries the "details of the situations". Without these details, there is little to do for the triad plus habit proposition. As a consequence, it is almost impossible to find out what happens when the triad and habit proposition acts on its own. In fact, triad plus habit do not produce anything if left alone.

Next, we do not yet have a full understanding of how to identify the contributions from the "triad of temperaments plus habit", the auxiliary, belt, assumptions, and the disturbing causes which operate in the real situations that the model seeks to explain and forecast. In other words, if it is the web of causes, i.e. core and belt, that produce the model's result, which difference, then, does the triad plus habit make? To answer this question, we need to say, first, that such financial market capacities exist, i.e. make an epistemic statement, and, second, have a method that can identify, extract and allocate these capacities to their respective causal factors. Financial economists can, of course, commit to the first requirement, but will be at loss in giving complete answers to the second. Financial market situations are too complex, unstable, and interconnected. Creating a clean experimental zone in which causal contributions can be allocated to principles, assumptions, and the disturbing causes have not been achieved.

Additionally, I have often highlighted Cochrane's preference for the "theory heavy" approach taken to understand asset prices and their behaviour. Fama and French's "factor-modelling" stand in contrast to this neo-classical methodology. Can it be, I ask, that the triad plus habit principles stand in the way of further progress? Can we use them to learn about financial markets so to make *tendency* claims, or even point-forecasts? It is certainly the case that Cochrane "works with theory" from an a priori starting point and then derives consequences that he believes to be useful for empirical representation. But is a theory-heavy approach a good starting point for establishing knowledge for use in relation to activities in complex financial situations? Lawson's "trying harder" analogy comes to mind. And if Lawson is right, is Cochrane's orthodox methodology, at all, able to explain the phenomena and establish claims about *tendencies*? Maybe Cochrane should give even more weight to the findings of the factor-based, relative research effort than he initially envisaged.

And, as a final word of caution, I wonder how we can be sure that the financial market situations and their manifestations, i.e. prices, excess returns, equity risk premia, carry in them processes that can be captured and used by financial economists to render knowledge for use by their stakeholders. It is certainly true that their methods, be it visible in structural equations or time series analysis or the H-D approach, so far, have not been able to establish acceptable point-forecasts. Do we have any reasons to believe that such or other innovative methods can extract causal factors with stable capacities? My answer here is tentative; not being able to establish *tendencies* is not evidence good enough that they do not exist, and that they cannot be discovered. But such a discussion might take us into the field of metaphysics that Giere (2008) warns against.

In sum, I argue that Cochrane's current methodological approach is possibly too orthodox and might not be appropriate for what he wants to explain. When encountering anomalies, Cochrane swings between *stories*, theories, models and tests and indicates a high degree of flexibility with respect to reverse-engineering at a deep level inside his applied mathematical model. In this respect, he uses the opportunities Lakatos has offered in the "protective belt" to their fullest in order to back out highly specific point-forecasts. Cochrane thus portrays himself as a "fox". Yet, more focus on ontology and methodology along the lines that Mill, Sutton and Lawson just suggested might be more beneficial, i.e. become a true "hedgehog" using the triad of temperaments plus habit as the few and main "principles". Where Cochrane seeks unique outcomes and even precise point-forecast concerning stock prices, expected excess returns and equity risk premia, he might be better advised to treat his model-based predictions as claims about *tendencies* instead. Nevertheless, a shift in focus would let Cochrane continue using "predictive success" as a litmus test when evaluating the outcome of his asset pricing research programme – drawing on the advice given by the positive and negative heuristic. And it will not hinder the theoretical progression of his distinct research programme. The programme can thus progress both theoretically and empirically. What this shift in focus brings is, however, not an improved predictability but a humble delivery of knowledge for use.

5. Statistical philosophy - a way out?

So far we have found that Cochrane is adamant with respect to the importance of using the consumption based asset pricing approach in developing a better understanding of financial market activities and establishing links upwards to macro-events and downwards to investors' behaviour. We are told investors' first-order condition for savings and portfolio choice is the "correct starting point" in an exercise aimed at providing knowledge for use. Earlier I argued that the assumptions are simplified, ideal, fictional and unrealistic. They are used to establish an isolated, non-complex, socio-economic model world. It cannot be expected, therefore, that such theories and their models represent the real situations well. Nonetheless, the fundamental, core, principles and auxiliary, belt, assumption form the foundation for the "litmus test" when measuring predictive success, i.e. scientific progress. As we recall, this aspiration found its outlet in what Cochrane referred to as reverse-engineering at a deep technical level inside the utility function while still keeping the character of the rational, self-interested and risk averse investor in-tact and unchallenged. This approach, I commented, is well within the heuristics of the consumption based asset pricing programme.

However, we sensed Cochrane's preference not to focus too intensively on the statistical quality of the predictions when extending the analytical cases towards the real situations. This is understandable. Both his original and the new habit-persistent CCAPM are rejected due to the lack of predictive success in the pursuit of explanation in the specific case of the stock prices, excess returns and the equity risk premium puzzle. But if the litmus test is not the sole criteria what else should we focus on? I already pointed towards three areas of considerations; More realistic assumptions can be added, the socio-economic reality might be too complex for what his asset pricing model seeks to capture, and *tendency* claims might be a better alternative than point-forecasts when explaining real situations. As far as I can judge, Cochrane does not consider these suggestions. Instead, he presents his own solution. In the next sections, I analyse Cochrane's stimulating suggestions.

5.1 De-emphasizing predictability

In the middle of his book on *Asset Prising* (2005), Cochrane positions a paragraph denoted "Statistical Philosophy". It seems somewhat misplaced but gives valuable insights to Cochrane's thinking around the topic of theory and model assessment. Below, a long quote sets the scene:

> "Statistical testing is one of many questions we ask in evaluating theories, and usually not the most important one. (...) Think of the kind of questions people ask when presented with a theory and accompanying empirical work. They usually start by thinking hard about the theory itself. What is the central part of the model or explanation? Is it internally consistent? Do the

> assumptions make sense? Then, when we get to the empirical work, how were the numbers produced? Are the data definitions sensible? Are the concepts in the data decent proxies for the concepts in the model? (...) Are the model predictions robust to the inevitable simplifications? (...) How much fishing around for functional forms, data definitions, proxies, and innumerable other specification issues, did the authors do in order to produce good results? Finally, someone in the back of the room might raise his hand and ask, "if the data were generated by a draw of i.i.d. normal random variables over and over again, how often would you come up with a number this big or bigger?" This is an interesting and important check on the overall believability of the result. But it is not necessarily the first check, and certainly not the last and decisive check. Many models are kept that have economically interesting but statistically rejectable results, and many more models are quickly forgotten that have strong statistics but just do not tell as clean a story." (Cochrane 2005, p. 305).

From this extract, we understand that many questions may be asked towards achieving a better understanding of what a theory and its models are, and what they are used for. The questions can be allocated to two different categories. In the first category, we find questions related to the theory itself while the second accommodate those related to the empirical tests and achieved results.

Cochrane thus wants us to ask whether the mathematical structure and the *story* are consistent and make intuitive sense. This topic was also addressed by Gibbard and Varian (1978) as I pointed out in *Chapter Four, section 1.2.2*. Furthermore, Cochrane wants to be challenged on how he operationalizes and measures concepts used in asset pricing research. I have discussed the main topics earlier in my thesis. I found that Cochrane's applied mathematical model, i.e. the "M" in CCAPM and the more detailed and specific analytical cases are well supported by a set of assumptions, ranging across a few fundamental, core, principles to more numerous auxiliary, belt, assumptions. I now turn to the latter part of the long Cochrane quote that I introduced.

At the very end of that insert, Cochrane sums it all up by de-emphasizing the value of empirical tests. Statistical results should not be the "last and decisive" check. He tells us that history is full of examples where statistically rejected models continue to "live-on". We recall, for example, that Sharpe's single-factor CAPM was superseded by Fama and

French's multi-factor "style" models. The reason for this, Cochrane claims, was that they presented the readers with a more "interesting" and "clean story" (see *Chapter Two, section 1.3*). This statement certainly reduces the relative importance of the widely used litmus test of predictability relative to the other above mentioned criteria. Cochrane thus suggests that it is possible to achieve scientific progress by not only relying on the quantitative results of statistical accuracy as measured by standard statistical tests, but also from reviewing the results against the back-drop of qualitative criteria.

Cochrane even claims:

> "The classical theory of hypothesis testing, its Bayesian alternative, or the underlying hypothesis-testing view of the philosophy of science are miserable descriptions of the way science in general and economics in particular proceeds..." (Cochrane 2005, p. 305).

He doubles up:

> "Fifty years ago, the reigning philosophy of science focused on the idea that scientists provide rejectable hypothesis. This idea runs through philosophical writings exemplified by Popper (1959), classical statistical decision theory, and mirrored in economics by Friedman (1953)." (Cochrane 2005, p. 306).

Cochrane's own idea of scientific progress, he admits, is closer to those of Kuhn (1970) and McCloskey (1998). The advantage of their work, Cochrane claims, is that these authors circle in on what genuinely goes on in the economic "laboratory"; much rhetoric, a lot of convincing and little focus on "the largest t-statistics".

Cochrane here highlights a possible disconnect between what the scientific method "prescribes", and what financial economists truly do back home. He certainly seems to advocate less focus on the value of statistical measures. Given what we so far have read with respect to his asset pricing research approach this is somewhat puzzling. It certainly does not square up to his euphoric statements I alluded to earlier of how asset pricing research efforts are on the verge of achieving "predictive successes".

De-emphasizing predictions do not go down well in some quarters. We recall that Rosenberg (1994) was unyielding with respect to this defining characteristic of a progressive science:

> "For the purpose of this book, I stipulate the following implication of the economists' commitment to an empirical epistemology; a scientific discipline should be expected to show a long-term pattern of improvements in the proportion of correct predictions and their precision." (Rosenberg 1994, p. 18).

We recall that Rosenberg does not believe that this "long-term improvement" has been evidenced in the part of economics that he reviews, i.e. micro-economics. The predictive ability of economics has been too weak. But Rosenberg is in particular intrigued by the apparent immunity of economics against persistent empirical evidence of rejections. Rosenberg argues that this failure can be traced back to the choice theory as found inside the standard microeconomic based utility theory. From his point of view, this choice theory is nothing more than advanced "folk psychology", and this is certainly neither a necessary nor sufficient basis for any science. Following this rather negative assessment, what is Rosenberg's suggestion to economists:

> "If we are to apply, test and improve the explanations we make with the hypothesis that agents engage in rational choice, we need to measure the "initial conditions" to which we apply this hypothesis. Especially, if we want to improve our predictions, we need to improve our measurements of the states of the agent to which we apply the theory in order to secure predictions about behaviour." (Rosenberg 1994, p. 124).

Rosenberg continues:

> "We cannot expect the theory's predictions and explanations of choices of individuals to exceed the precision and accuracy of the common sense explanations and predictions with which we have all been familiar since prehistory." (Rosenberg 1994, p. 129).

The core of the argument thus goes towards improving our understanding of choice, and the conditions under which they are formed and taken. If this cannot be achieved, scientific explorations on the basis of this orthodoxy cannot be expected to render a better outcome than what any informed layman would be able to suggest.

In this context, I think Cochrane could formulate a good reply to Rosenberg's first suggestion. He could argue that we have augmented our understanding of the "initial choice" conditions because of the inclusion of the habit-persistency argument related to "keeping up with the Joneses" in the investor's utility function. The adoption of this external benchmark intuitively makes sense. Nonetheless, it is more difficult to address Rosenberg's suggestion that such enhancement might be classified as advanced "folk psychology" and nothing more. Rosenberg is not overly explicit in drawing the line between the "psychology" and more scientifically founded elaborations. But it could be that he believes that a rational choice model is not in any ways superior to or even necessary as long as we use some "common sense and understanding". In the end, Rosenberg argues, predictive success is the ultimate benchmark for scientific progress. However, this advice takes us towards accepting Friedman's instrumentalism – a view I oppose on grounds given above in *section 4.1*. In the following, I do not intend to address the relatively rich literature which has formed on instrumentalism beyond what I have already mentioned there.

Rosenberg finds that economics cannot be characterized as an empirical science because such sciences thrive on predictive success based upon theoretical progress. Towards the end of his book *Economics – Mathematical Politics Or Science Of Diminishing Returns* (1994), Rosenberg concludes that economics is rather a form of mathematical politics. Space does not permit a proper analysis, so we have to leave it as suggested.

Hausman (1992) and Lawson (2009), also point towards the lack of predictive success within the standard neo-classical paradigm of economics. Hausman, however, and in contrast to Rosenberg, sees the merits of continuing with a set of a priori fundamental principles that have been established inductively as articulated in most micro-economic supported theories of the consumer. When their predictions fail, Hausman suggests in the spirit of Mill, to ask economists to try again within the same framework. Lawson, we know by now, resists this view. He argues for a shift away from the orthodox neo-classical microeconomic principles that hitherto have, unsuccessfully, been applied to the complex socio-economic topics that do not easily surrender to the efforts made by the profession. Hausman, like Maeki, following Mill and Cartwright, but in contrast to Rosenberg, argues that economics is an empirical science – but inexact. Hausman, furthermore, is of the opinion that economists use models for conceptual explorations from which claims can be made. I have argued earlier that Cochrane's asset pricing research programme goes further than mere explorations (*Chapter Four, section 3.2*)

What do Rosenberg and Hausman's suggestions mean for Cochrane's research effort? Cochrane certainly could argue that the asset pricing research programme is a branch of the scientific project aimed at understanding human action in a societal context. His only support for this argument, so far, has been predictive success. It is clear that Cochrane has not provided sufficient evidence that would allow him to defend this proposition. Hence, as long as this support is lacking, it is difficult to accept Cochrane's claim that the consumption based asset pricing research programme is part of the "scientific project".

If Cochrane reformulates the success criteria to be "scientific progress" rather than "predictive success", I think he stands a better chance of attracting listeners. He could, for example, point to developments from single- to multi-factor pricing models and to the integration of habits into the standard CCAPM. Nonetheless, this shift in emphasis from predictive success to scientific progress would be opposed by the Lakatosian view of measuring a programmes' progress both on theoretical and empirical merits. The point to stress here is the way this progress has been achieved – namely through predictive failures. In this sense, progressive sciences thrive on mistakes as Mill, Lakatos and Hausman suggest it should.

In sum, I think Cochrane suggestions of including the external benchmark of habitual consumption has improved our understanding of investor's choice situations or "initial conditions” as Rosenberg calls them. Nonetheless, despite this theoretical progress, there is only modest predictive progress to confirm that the investor carries habitual persistence across time and states in a way that makes the argument “visible” in the asset prices. Nonetheless, I think we can qualify Cochrane’s effort as a Lakatosian progress.

5.2 Interesting and clean stories?

Nonetheless, let me return to the broader question related to the evaluation of theories. I do not intend to discuss whether Cochrane’s list of assessment criteria given in the long extract above is complete and accurate but rather turn to his last assertion that models may continue to live on despite having been statistically rejected. The intriguing part of the claim is that the survivors rather tell an “interesting” and “clean story” than showing “statistical significance” through high “t-statistics”. Cochrane takes the lead when claiming:

> “..., I can think of no case in which the application of a clever statistical model to wring the last ounce of efficiency out of a data set, changing t-statistics from 1.5 to 2.5, substantially changed the way people think about an issue.” (Cochrane 2005, pp. 304).

Additionally, Cochrane tells us that Fama and French’s model has replaced Sharpe’s CAPM. Why? He claims that the innovative character of the multi-factor model changed the way people think about asset pricing. Intuitively it makes sense that many factors influence asset price level and behaviour, and that these factors, possibly, originate in the properties of a company, i.e. their “size” and “valuation” as Fama and French suggest. The two authors’ effort and explanations were, as Cochrane tells us, more “persuasive”, “interesting”, “sensible” and “coherent” to the discerning reader. As a consequence, Cochrane explains, a “cleaner” *story* emerged. Cochrane even goes as far as to claim that: “...these papers *[referring to Fama and French research]* made clear what stylized and robust facts in the data drive the results, and why those facts are economically sensible.” (Cochrane 2005, p. 304). Cochrane concludes: “It seems that “it takes a model to beat a model,” not a rejection.” (Cochrane 2005, p. 304).

I have highlighted some of the words that Cochrane uses to create a second metric, besides “predictive success”, to be used when evaluating model-based claims. Nonetheless, developing “persuasive” and “interesting” *stories* to back up the mathematical structure of the asset pricing model, is not always a good thing. Cochrane reminds us: “In general, empirical success varies inversely with theoretical purity.” (Cochrane 1999a, p. 40). This statement clearly draws our attention towards a trade-off. However, Cochrane fails to tell us where the borders lie between “empirical success” and the “purity of stories”. This was also a topic earlier in *Chapter Four, section 4.4.1* where I discussed Cochrane’s *stories* in some detail. Striking the right balance, therefore, becomes challenging as it might be based upon a pure subjective judgement. My concerns are, as a consequence, also related to model selection. It is not obvious how two theories and their models can be compared along the criteria of “persuasiveness”, “sensibility”, and “interesting”. And from that judge which model is the “cleanest”. It almost sounds like asking a panel of experts to decide on the basis of some quantitative and qualitative score-cards. Model selection is surely different. Or is it? Let us continue our explorations.

Cochrane seems to be willing to consider story-telling as a basis for model competition. *First,* he gives credit to Mehra and Prescott who presented us with the equity risk premium puzzle: “In finance as elsewhere, identifying, marketing and packaging the insight, and leaving a structure that others can play with, are justly important contributions.” (Cochrane 2006, p. 24). I think the most significant part of this statement is certainly the latter, i.e. leaving a legacy that contributes to bringing the whole financial market research new impetus. Yet, I cannot resist pointing towards the first part in the context of my discussion here. Cochrane even emphasises: “...a researcher who wants his ideas to be convincing, as well as right, should do well to study how ideas have in the past convinced people,..” (Cochrane 2005, p. 306). I think this could rather be taken as practical advice to his upcoming PhD students than giving clear directions with respect to model selection. Nonetheless, if Cochrane wants to see both theoretical and empirical progress in the consumption based research programme, and use “predictive success” as a "litmus test", I think he is well advised to go back to the quantifiable evidence rather than consulting expert score cards.

In the end, I think it is sensible to evaluate a theory and its models along the lines Cochrane initially suggests, i.e. statistical tests of model-based claims. However, as we have seen, Cochrane lets such criteria fade as increasingly more evidence is collected against accepting the claims emitted from the original and new, habit-persistency enhanced versions of the CCAPM. Despite the legitimate reverse-engineering effort, Cochrane drifts away from answering the statistical challenges up front. Instead, "interesting" and "clean stories" are introduced as new criteria for measuring "scientific progress".

Cochrane's suggestion to complement the quantitative model choice measures with other more qualitative and subjective criteria must be rejected on methodological grounds. The discussion on *tendencies* might give Cochrane a pretext to argue that although the CCAPM fails both general and the specific equity risk premium puzzle tests, it is right in claiming, first, that it has captured *tendencies* that are active in real situations, and, secondly, that the "cleaner" the *stories* are the better they get. However, Cartwright warns us about "messy" situations. So how can clean *stories* be told about messy situations? Needless to say, Cochrane's efforts to justify a continued use of consumption based asset pricing model is a far cry from what we see in the scientific practices of other disciplines.

In the next section, I draw main conclusions.

6. Main conclusions

First; the consumption based asset pricing model, i.e. the model "M" in CCAPM, is an applied mathematical model. It has been given a dual role; it is used for theorizing, i.e. "conceptual explorations" and for econometric analysis, i.e. fitting theories to the data.

Second; in its first role, the model "M" is developed in a "process of isolation" which establishes "simplified", "idealized" and even "fictional" versions of the investor, the financial markets and the financial assets. This a priori starting point is used to deduce more granular models referred to as "analytically convenient special cases".

Third; in its second role, the model "M" extends its analytically convenient special cases towards the real situations. I draw on the much debated "equity risk premium puzzle" to demonstrate the model's inability to develop what is referred to as "empirically useful representations". Given this inadequacy, Cochrane suggests incorporating "habit persistency" as a new argument into the model's mathematical utility function.

Fourth; I argue that the consumption based capital asset pricing effort, in Imre Lakatos' sense, is a "research programme" that uses a few fundamental, "hard core", principles, a large flexible set of auxiliary, "protective belt", assumptions, methodological decision rules in the form of "positive and negative heuristic", and an established form of assessing whether the research programme is "progressive" or "degenerating". Cochrane's "habit-persistency" argument is thus well within the heuristic of the programme, it modestly improves the programme's predictability and contributes to its progression.

Fifth; although the analytical convenient special cases are to some extent de-idealized and de-fictionalized versions of their a priori starting point, they remain tools for theorizing and find few methodologically sound and valid bridges to the real situations they target. As a consequence, I do not expect the CCAPM research programme to progress by the construction of even more granular, lower-level models as they are too dependent upon the auxiliary, belt assumptions. Hence, these cases lack horizontal portability to situations different from those they are meant to represent.

Sixth; given the research programme's modest level of epistemic value, it's advocates are well advised to move away from the model-based "point-forecasts" approach towards one that makes "tendency claims" with respect to empirical situations. This re-direction replaces Cochrane's own suggestion to reduce the importance of standard statistical tests when evaluating the model-based claims, and, in addition, offer a sounder foundation for emitting knowledge for use to the various stakeholders.

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