Methodological Issues in Neoclassical and Behavioral Economics

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Abstract

This work describes how epistemological and methodological stances have been applied to economics; in particular the thesis' subject is the approach to two research programs, the neoclassical economics and the behavioral economics.
Neoclassical economics is, nowadays, the dominating paradigm. The work depicts the evolution of its methodology, starting with Mill's narrow view of economics, passing through the economic approach to human behavior inspired by Friedman and terminating with the meaning of economic models.
On the contrary, behavioral economics is a recently born paradigm whose original aim was to increase the realism in economic investigations. Despite behavioral economics' original essence, an increasing number of its proselytes is turning to mainstream. This apparent betrayal has its root in a methodology proposed in the neoclassical framework, namely Friedman's instrumentalism. Methodological backgrounds underneath the history of utility function accompany the description of behavioral economics' development.

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Chapter 1

Introduction

This thesis is presented for a Master of Science in management engineering. Yet, it deals with topics, namely epistemology and methodology of economics, that are generally held to be quite far from engineering's subjects. How to justify this apparently inconsistent choice? In order to disentangle this crucial question, it is necessary to outline the essence of management engineering. The cornerstone idea of the discipline, whose traces can be found in any of its subjects, from bachelor to master, is that engineer has to face really complex problems for which, almost usually, optimal solutions can't be found. In this landscape populated by problems that are computationally and conceptually demanding, the approaching to the problem has the prominent role. Given the fact that optimal solutions are the exception, rather than the rule, different approaches yield different sub-optimal solutions that present trade-off among themselves. This idea is very common in Operational Research, in particular in the field of heuristics. In it, there is a wide family of procedures that can be used to solve intractable problems and the ability of the solvers lie in the choice of procedures that better fit the practical situation. In this approach-dependent framework, focusing just on solutions would be a clear mistake, as the procedure is completely determined by the approach chosen. The presence of trade-offs among solutions and the approach-dependent feature underline another important peculiarity of the issues that management engineers usually have to address: problems cannot be considered on their own. Rather than a static vision, where the problem stands in the focal point, it should be better to conceive a dynamic vision in which solver, problem and context are engaged in a continuous exchange of information that eventually leads to a sufficiently good solution. According to those considerations, it seems that the anytime a problem arise, effort should be focused on problem's approach.

In this work the problem is the dismal science, that is economics. It should be quite evident that economic system is the root of many problems and that its nature is intrinsically problematic. This thesis, consistently with the teaching of management engineering, instead of focusing on solutions and their effects (namely public expenditure, austerity, central bank operations), focuses on the economics' approaches. The fact that those approaches are located in the field of philosophy is just incidental.

The aim of the work is to describe the methodologies of two economic schools: neoclassical and behavioral. The neoclassical economics is the mainstream; academic courses deal with it and economic policies are taken consistently with its

principles. Behavioral economics is a relatively new paradigm; born with the aim of increasing the realism of neoclassical assumptions by directly studying agents' behaviour, nowadays part of this program has been absorbed by the mainstream, while the remaining share works on a new paradigm.

Chapter 1 introduces three philosophical concepts that are typical in epistemology and methodology. The content of this chapter is quite important, as it has strong and interesting connections with the topics presented throughout the thesis. Chapter 2 is dedicated to neoclassical methodology. Following a chronological path, it ranges from John Stuart Mill to the meaning of models in economics, passing through Friedman. It is interesting to notice that, even if the neoclassical paradigm is a well unified program, its methodological foundations changed dramatically from Mill to Friedman. Chapter 3 deals with behavioral economics. It begins with an historical overview that might shed lights on the relationship between economics and psychology. Then, the main discoveries of behavioral economics are depicted. Finally, it points out the disputes that nowadays this new research program is facing; on one side there is the possibility to be absorbed by the mainstream, on the other side, a revolution is needed. Chapter 5 concludes.

Chapter 2

Three Philosophical Pillars

In this chapter, three main philosophical issues will be introduced: the induction problem, the Duhem-Quine thesis and the *ceteris paribus* clause. They deserve a detailed and separate section because they will constitute the focus point of the methodology of economics' analysis.

The philosophical problems that are going to be disentangled do not belong to the same category. The induction problem's scope is actually the philosophy of knowledge. The Duhem-Quine thesis is concerned with the verifiability theory. And finally, *ceteris paribus* clauses characterize almost all scientific statements.

The objective of the chapter is to deal with those three pillars in a neutral fashion; a part for an example regarding the Duhem-Quine thesis, topics will be described without any economics' references.

2.1 The Induction Problem

The induction problem is one of the most difficult problem that philosophy has faced. The first author that formalized it was David Hume. As it will be stated in this section, the consequence of the induction problem is quite radical: our scientific knowledge has got a rational base? The question is quite relevant because our society strongly rely on science. It's a commonly accepted belief that scientific disciplines and science in general are the most rational human activities. Nonetheless Hume and, later, Bertrand Russell argued that the induction principle cannot be logically accepted without having previously assumed the same induction principle. This circular reason, that proves the impossibility to logically verify the induction principle, is the most significant result of the problem. The induction problem that has just been introduced is the logical version of the issue. David Hume and Bertrand Russell are the philosophers that have studied it. There is also a practical version of the problem. The latter has been analysed by John Stuart Mill. First the practical problem will be described and then the logical version.

2.1.1 The Practical Induction Problem

John Stuart Mill was an empiricist. The empiricism main idea is actually quite simple: the good science should be based on experience. The problem, as Mill argues, is that social phenomena are complex, that is there are a lot of effects for which the social scientist is not able to derive in a easy way causes. Furthermore, this difficulty

is increased by the fact that in social sciences, experiments cannot be run:

> In these sciences we therefore study nature in a situation of great disadvantage; being forced to the limited number of experiments that take place (if so we can say) spontaneously, without any preparation or direction from our part; in circumstances also of great complexity and never perfectly Note; and with most of the procedures hidden from our observation. The consequence of this inevitable defect in the elements of induction is that we can rarely discern what Bacon designated [...] as *experimentum crucis*[Mill, 1836]

The induction problem described by Mill boils down to the fact that humans are small and have an imperfect and partial information of the procedures of social phenomena. Thus, trying to acquire scientific knowledge in social sciences just by looking at the effects is not a viable method:

> Being therefore vain to hope to reach the truth, both in Political Economy and in any other sector of social science, as long as we look at the facts in concrete, covered with all the complexity of which nature has surrounded them, and we try to draw a general reading with a process of induction from a comparison of details, there is no other method than the a priori one, or of abstract speculation [Mill, 1836]

It's worth to notice that Mill does not uphold that is logically impossible to base the scientific knowledge on induction. Mill is arguing that social sciences and social phenomena have some peculiar characteristics that rule out the possibility to apply the induction method. If social scientists had a perfect information of the hidden social mechanisms, they would be able to look at the effects and derive in a straightforward way the causes. If the social phenomena could be investigated by an *experimentum crucis*, the induction method would be suitable also for social sciences. That's why the induction problem introduced by Mill is a practical problem.

2.1.2 The Logical Induction Problem

Mill's induction problem is actually a light problem: induction doesn't work well in the social sciences because they study phenomena whose characteristics are not in compliance with the induction method. The logical version of the induction issue is more radical.

Let's start from a well known and not so original example. The problem is to provide an answer to the following question: "Will the sun rise tomorrow morning?". From past observations it is known that sun has always risen. Furthermore, by studying related phenomena, physicians have derived some laws that state that the Earth is rotating around its axis and thanks to this rotation every morning the sun rises. Notice that the reason behind this knowledge is build according to this over-simplified schema:

The laws of motion and gravity worked this morning
The laws of motion and gravity worked yesterday
The laws of motion and gravity worked the day before yesterday

...
The laws of motion and gravity worked worked in the 5^{th} of July 1976
...
$\Downarrow$
The laws of motion and gravity will work also tomorrow morning

The starting point of the logical problem of induction can be introdued citing both Hume and Russell:

> Since it seems that the transition from an impression, present to memory or to the senses, to the idea of an object, which we call cause or effect, is based on past experience and on the memory of their constant conjunction, one wonders if the experience it produces this idea through the intellect or through imagination [Hume, 1740]

> But the real question is: does any number of cases of compliance with a law in the past give us proof that that law will be fulfilled in the future? [Russell, 1912]

In order to address the problem, a definition of the induction principle should be provided. The induction principle can be defined by two points:

1. the greater the number of cases in which a thing of type A is associated with a thing of type B, the more probable it is that A is always associated with B;
2. under the same circumstances, a sufficient number of cases of association of A with B will almost be the certainty that A is always associated with B, and will cause this general law to approach the truth unlimitedly [Russell, 1912]

Given the definition of the induction principle, the next step is to provide an argument based on experience that is able to prove or refute the induction principle. It will be shown that the argument cannot be provided as it does not exist:

- **Impossibility to refute**. The definition of the induction principle contains the notion of probability. According to that notion, the occurrence of a phenomenon with very low probability does not contradict the law expressed in probability terms.
- **Impossibility to prove**. The experience can only verify that the past phenomena are in compliance with the law. Since the future is the set of phenomena for which we do not have experience, it is impossible to compare a future phenomenon with the law.

David Hume and Bertrand Russell provide a solution to the the logical version of the induction problem:

> Our previous method of reasoning easily convinces us that there cannot be sufficient demonstrative arguments to prove that *these cases of which we have not had any experience resemble those of which we have had it.* [Hume, 1740]

> The general principles of science, such as trust in the rule of law, and the belief that every fact should have a cause depend directly on the principle of induction [...]. In all those general principles, men believe because they have seen

> countless examples of their validity and no proof of their falsity. But this does not provide any proof of their validity in the future, if the inductive principle is not considered valid. [Russell, 1912]

Those conclusions are indeed quite radical. Scientists of any fields of the knowledge might find laws that describe real phenomena, but in the end nothing ensures that those phenomena will happen also in the future. It seems that the scientific activity is based on an irrational cornerstone. Indeed, this is the conclusion provided by Hume:

> If therefore we call habit that which proceeds from an antecedent repetition, without any new reasoning or inference, we can establish as a certain truth that every belief, which follows a present impression, has in this its only origin. [Hume, 1740]

It is evident that habit does not constitute a rational pillar to the scientific activity. There is a chance to follow the destiny of Bertrand's turkey.

2.2 The Duhem-Quine thesis

The Duhem-Quine thesis is an argument that has been introduced by Pierre Duhem in 1906 and then it was further developed by Quine. This argument can be used to criticize both the Popper's approach to epistemology and the Neopositivism. In particular, the former will be explored in the detail when the application of Popper's method to economics will be introduced.

The thesis deals with the existence of what Bacon refers to *experimentum crucis*. The aim of this experiment is to validate or refute a theory. Just by looking at science's history, it is really not common that a single experiment is able to refute completely a theory. But there are not just historical reason to doubt the concept of *experimentum crucis*; there are also logical reasons, and the Duhem-Quine thesis is based on those ones. The result is that a theory can't be tested on its own, but it is always tested together with auxiliary hypothesis. The Duhem-Quine thesis splits in two problem: the one related to the stochastic hypothesis and the other one related to the auxiliary hypothesis.

- **Stochastic hypothesis problem**: suppose that an hypothesis is formulated to describe a physical phenomenon. The hypothesis is in the form of a mathematical equation such as

  $$r = a_1 Y + a_2 X$$

  If this "theoretical" equation is tested, it will be for sure rejected: any observation that do not exactly lie in that plane, would be a proof to refute the hypothesis. This methodology is not at all wise; let's consider, for instance, observation about the freezing temperature of the water. It would be very unlikely to get always 0 Celsius degree. Actually, all measurements would be around zero, but it can be the case that none of them is exactly zero. Coming back to the previous hypothesis, if the aim is to verify it, a clever strategy is to include in the equation an error term. The equation eventually becomes

  $$r = a_1 Y + a_2 X + \epsilon$$

Now, two problems arise: how large/small ϵ should be and which hypothesis are under examination. The second problem is the one related to the Duhem-Quine thesis. As soon as an error term is included in an hypothesis to run the validation phase, the verification phase actually involves two hypothesis: the starting one and the error term. If the verification test's result is not consistent with the theory, should the experimenter refute the starting hypothesis or ascribe the discrepancy to the error term? There is no logical mechanism that is able to indicate which is the right choice.

- **Auxiliary hypothesis problem**: The starting point of the auxiliary hypothesis problem is the well-known logical argument called *modus tollendo tollens*. The schema of the *modus tollens* is:

$$\alpha \rightarrow \beta, \neg\beta \vdash \neg\alpha$$

The logical expression simply states that, if the consequent of a conditional is denied, then the antecedent is denied as well.
The problem of course does not lie in the *modus tollens*, that is a verified and quite simple logical structure. Rather, the root of the issue should be searched in some naive applications of this structure in the field of epistemology. Let's consider an historical example. In 1919 Arthur Eddington went to Africa to observe a solar eclipse. The purpose was to compare observations with the implications of both Newton's law of gravity and Einstein's law of gravity. Essentially, Eddington was in charge to validate one theory and refute the other. Once observations have been collected, it turned out they were not in compliance neither with Newton's theory, nor with Einstein's one. Here two interesting and fascinating problems can be underlined: the first one deals with the competition of two opposite theories; the second one is concerned with the possibility of the verifiability of a scientific proposition on its own. Focus is posed on the latter issue and the question is: is it possible to verify a scientific hypothesis on its own? If the aim is to collect data on solar eclipse, the experimenter has to use at least one further theory, the optical theory. A step forward can be done by saying that any time a scientific hypothesis has to be controlled, it is not controlled on its own, but together with other propositions and assumption. Following this reason, the previous logical structure should be modified when applied to the verification of scientific propositions:

$$(H_1 \wedge H_2 \wedge H_3 \wedge ...H_m) \rightarrow \beta, \neg\beta \vdash \neg(H_1 \wedge H_2 \wedge H_3 \wedge ...H_m)$$

where $H_1, H_2, H_3, ..., H_m$ are the hypothesis and statements that are involved in the verification phase. The Duhem-Quine problem is now materializing: from the *modus tollens* it is known that the antecedent is denied, but there is no logical tool that indicates which of the hypothesis and statements under investigation is false. In symbols:

$$\neg(H_1 \wedge H_2 \wedge H_3 \wedge ...H_m) = \neg H_1 \vee \neg H_2 \vee \neg H_3 \vee ...\neg H_m$$

The conclusion provided by Duhem and Quine is that

> The experiment does not designate which hypothesis should be changed, for it is the whole theoretical scaffolding used by the physicist that is called into question. [Duhem, 1906]

> The unit of measurement of empirical significance is all science in its entirety. [...] science as a whole is like a force field whose limits are experience. A disagreement with the experience on the periphery causes a rearrangement within the field.[van Orman Quine, 1976]

It is worth to provide an example taken from the economic theory (the following example is taken from [Sawyer et al., 1997]).

The objective is to test the Positive Duration Dependence theory; according to it, unemployed individuals are more likely to accept wage offers the longer they have been unemployed. One possible formalization of the theory maintain that the reservation wage of unemployed individuals declines during the job search period, thus increasing the range of wage accepted by the same unemployed. This model assumes that:

1. Job offers are distributed according to a Poisson distribution with expected value λ_t
2. The distribution of wage offers is known
3. The cost of search C_t is non-zero

Given the previous assumption, the theory can be expressed by the following analytical formula:

$$W_t = -C_t + \frac{\lambda_t}{r} \int_{W_t}^{\infty} (w - W_t) dF_e(w)$$

Leaving aside any theoretical feature of the theory, at this stage testing phase's problems are underlined; the theory it is not tested on its own, but with other auxiliary hypothesis, namely the distribution of job offers, the distribution of wage offers and the cost of job search.

2.3 *Ceteris Paribus* Clause

Ceteris Paribus is a latin expression whose translation is "all the other things remain constant". Almost every scientific statement is qualified by this clause and this is evident from this example (the example is taken from [Barrotta and Raffaelli, 1998]).

Let's suppose the motion of the pendulum has to be analysed. In theoretical physics there is a law that states that pendulum's period is described by the following equation:

$$Period = 2\pi\sqrt{l/g}$$

It is possible to express the law in a formal way by using the following expression: "every pendulum has a period of $2\pi\sqrt{l/g}$ ".

The particular statement concerning the pendulum's period can be generalized in a logical sequence of the form: "Every object that has the property F [is a pendulum] has the property G [has a period equals ...]". Now one may ask that any proposition belonging to a scientific theory has to be verified. It is straightforward

that if the testing phase involves the previous logical statement, the theory won't pass the validation phase in a large number of cases. This is because the proposition does not say anything about the rest of the world. Suppose, for instance that someone is applying a force to the pendulum by pushing it, or that a metallic pendulum is placed in an electric-magnetic field. For sure the period won't be described by the equation provided by the theory, nonetheless it is not upheld that the theory is false. If the aim is to verify a particular scientific proposition, something has to be added to the general law. So that the verifiable proposition is "*Ceteris Paribus*, every object that is a pendulum has the period equals ...". This is a general argument, but it is worth to focus a little bit to the peculiar characteristics that this clause has when it is involved in the physics (and in natural sciences in general). When it comes to verify the law of the pendulum's period, the proposition characterized by the *ceteris paribus* clause has to be used. If in the validation environment the "other things" are not constant, one must admit that the clause is violated. But it is not the end of the story. The experimenter can study in detail the scenario and eventually should be able to understand which are the things that are affecting the period of the pendulum. It will turn out that the effects that the "other things" may have on the pendulum are actually described by other physical laws. As before, if a metallic pendulum is placed in an electric-magnetic field, than the experimenter should take the electric and magnetic force into account. Not only the physics provides theories depicting how other things may affect the motion of the pendulum, but it provides also a rule to combine the effects and, finally, obtain the right equation of the period. Physics therefore allows the existence of a **closing deductive clause**. This expression means basically that the *ceteris paribus* clause can always be substituted by a proposition provided by the physics that, combined with the starting proposition (in this example, the law of pendulum's period), makes the verifiable proposition true. In the following chapter of this thesis, it will be upheld that the closing deductive clause is not available in economics.

It is worth to notice, very briefly, that the philosophical stance arguing that any proposition belonging to a scientific theory has to be verified, is called **Neopositivism**. This work will come back to Neopositivism and to the Hypothetico-deductive method in the following chapter.

Chapter 3

The Method of Neoclassical Economics

Neoclassical economics is the mainstream approach to economics. Then, an obvious result is that the epistemology of neoclassical economics is the orthodox epistemology. This chapter analyses how the methodology of this school has changed over years, underlying the strength points and the drawbacks of this evolution. Some contributions introduced in the following have been developed from inside the economic discipline, for instance the method proposed by Mill or Friedman's instrumentalism. On the other hand, some arguments are just economic applications of methodologies developed by philosophers of science.

The chapter deals mainly with three issues. Can economics be considered an hard science? Which is the nature of economic phenomena? Can economics' propositions be tested?

If one just look at the surfaces of those issues, she can concludes that they are too theoretical. But this is a very naive conclusion. On the contrary, they have outstanding and impressive practical implications. From 2007 on, newspapers and mass media continuously report that governments and central banks undertake economic policies to boost the economic system. Those policies are, in turn, based on some models; and that's where epistemology comes into the game. Understanding whether models are significant and whether they are able to grasp relevant economic interactions are crucial questions.

3.1 Mill and the Introspective-Deductive Method

Mill introduces his idea of methodology of economics in [Mill, 1836]. From the essay's title, "On the definition and method of political economy", it is possible to understand one important and innovative feature of Mill's thought; the description of economics' method follows the definition of the discipline. According to Mill it is impossible to depict a method if it is not clear which is the subject matter.

> The previous attempt to formulate a definition of the most exact science of the commonly accepted one can be considered of little use; [...] We are different, and for this reason: that the problem of the definition of a science is inseparably connected to that of the philosophical method of science, that is, of the nature of the procedure with which the investigations and truths must be conducted.

[Mill, 1836]

The structure of this sub-section follows this remarkable idea; first of all, Mill's defintion of economics is provided, then the methodology is described. The section ends with the brief description of an evolution of Mill'method proposed by Hausman [Hausman, 1992]

3.1.1 Mill's Definition of Economics

At the begging of the essay Mill confutes two well-accepted (at that time) definition of economics:

1. **"The economics is the science that teach how to increase the wealth of a Nation"**: Mill criticizes this definition by arguing that it makes economics not a science, but an art. It is obvious that, while the aim of an art is to provide imperative sentences ("Do that, and do not do that"), science's objective is to formulate descriptive sentences.

2. **"Political economics' scope is the laws that regulate the production, the distribution and the consumption of wealth"**: Mill admits that this defition can be useful for teaching purposes, but it is too wide to be a rigorous definition. Defined in that way, economics will include all natural sciences' laws; indeed, if one looks carefully of the procedures involved, for instance, in the production of goods, their description relies on dynamics, thermodynamics, chemistry and so on.

Mill builds his economics' definition on the distinction between moral (social) sciences and natural sciences: the topic of moral sciences is the human mind, while the topic of natural sciences is all the rest except the human mind.

> The laws of the production of objects that constitute wealth are the subject of study both of political economy and of almost all other natural sciences. However those laws which are purely laws of matter, belong exclusively to natural science. What are laws of the human mind, and no other, belong to the political economy that ultimately sums up the result of both together. [Mill, 1836]

Mill upholds that a rigorous definition of economics has to be searched in man's behaviour. But human kind, as it's characterized by intelligence and by a moral nature, is actually the topic of all the moral sciences. It's possible to analyse this side of human kind by considering a single individual, or by taking into account the sentiments that arise when two or more individuals come into a relationship, or, finally by focusing of the phenomena related to the society. Political economics' subject should be searched in this last category, but there is a problem: this category is again too wide. "The science of social economy embraces every part of human nature, to the extent that it influences the conduct or condition of the man of society"[Mill, 1836]. A scope restriction is needed:

> What is commonly meant by political economy is not the science of speculative politics (social economy), but a branch of this science. It does not treat the

> complex of nature as modified by the social state, nor the entire conduct of man in society. It deals with him only as a subject who wishes to possess wealth, and who is capable of judging the comparative effectiveness of the means. [Mill, 1836]

The previous citation underlines the human behaviour in society is complex and its explanation is the object of different and separate moral sciences. Political economics approaches this problem from a very narrow perspective; it considers the human action as it was determined just by the desire of acquire more wealth, "not that some economist has ever been so foolish as to assume that men are really made that way" [Mill, 1836].

Mill maintains that this is just the way sciences evolves. When a complex effect (in this case, the social life) depends by many causes, it is a wise choice to consider separately each cause and investigate which would be the effect if just that cause was in action. Then the results are combined together and complex effect is explained. Consider, for instance a physical body on which two forces are applied; the resultant force can be determined by applying the parallelogram law. This example however, sheds some light on peculiar problems that social sciences face when two or more "social forces" are combined together to get the resultant. The first problem deals with the existence of a law that takes the place of the parallelogram law. In moral sciences there is no law that describes how social causes interact with themselves. A qualitative description can be performed, but it is at least debatable that an exact result can be found. The second problem is concerned with the formalization of the social forces that are interacting. In physics, the parallelogram law works because the forces involved in the phenomenon are described by rigorous laws. For instance, the gravitational force applied to a body can be combined with the Coulomb' law if the body is placed in an electric field. Both the gravitational and the Coulomb's law are well-formalized by physics. But when it comes to describe a social phenomenon, there might be some causes for which a formalization is not available.

3.1.2 *A Priori* and *A Posteriori*

The economics' definition provided by Mill is "the science that traces the laws of those phenomena of society that arise from the combined operations of men for the production of wealth, since these phenomena are not modified by the pursuit of some other purpose" [Mill, 1836]. Social phenomena are complex, their effects depend to a multitude of complex causes. Political economics considers just one of those causes: the desire to acquire more wealth. All the other causes belong to different and separate social sciences. To describe the whole phenomenon, in the final stage, causes should be combined together.

Essentially Mill is upholding that is impossible to derive causes by looking at the effects. They are too complex, there are a lot of influences coming from separate disciplines. Furthermore in economics, *experimentum crucis* is not applicable. This is just the practical problem of induction: the social world is too complex for the scientists. If it was simpler, it would be possible to investigate it just by looking directly to the effects.

The practical problem of induction precludes some methods to the social scientist. *A posteriori* method is not applicable to find economics' law. The only remaining path is the *a priori* method. Mill provides a definitions for those two

research methods:

- ***A Posteriori***: "We mean that which requires, as the basis of its conclusions, not merely experience, but specific experience" [Mill, 1836]
- ***A Priori***: "We mean what is commonly understood, that is the reasoning that comes from the position of a hypothesis" [Mill, 1836]

As stated before, *a posteriori* method is not reliable since the specific experience is too complex to derive stable conclusions: "However, it was vain to hope to reach the truth, both in political economy and in any other sector of social science, as long as we look at the facts in practice, covered with the complexity of which nature has surrounded them, and we try to draw a general law with an induction process from a comparison of details, there is no other method than that *a priori*" [Mill, 1836]. From *a priori* method's definition, once an hypothesis is available, a deductive reasoning can be applied to discover economics law. Now, the issue is to explain how to find that hypothesis. The question is crucial because, from deductive reasoning's properties, it is known that this approach can't add anything to the premises. It's not stated that deductive conclusions are obvious (and actually they are not, just look in an advanced calculus textbook), but it is a law of deductive reasoning that conclusions are included in premises. Hence, the position and the justification of the hypothesis is a central issue for Mill's methodology. Mill addresses the issue in the following way:

> These causes are the laws of human nature and external circumstances capable of spurring human will to action. The desires of man and the nature of the conduct to which they push him fall within the field of observation. We can also look at the objects that arouse these desires. Everyone can gather most of the elements of this knowledge within themselves. [Mill, 1836]

Introspection is the method through which economist can derive causes of economic phenomena. Introspection is an experimental method by which, according to Mill, social scientists can test their assumptions. Where to find an evidence that more goods are better than less goods? Inside men's minds; but since all men are equals, if an economist look inside her mind by applying introspection, she will be able to find general hypothesis. Notice that Mill is providing economics with a method to test assumptions.

Once hypothesis have been derived, deduction reasoning is applied to find economics' laws. Are those laws directly verifiable? Political economics' definition should be reminded; the discipline focuses just on the effects that follow the desire to acquire more wealth, not in the ignorance of other human attitudes, but just because the scope of economics (and of any other social science) should be narrow. If effects predicted by economics' laws are compared with phenomena that actually take place in the real world, it will turn out those laws are failing to provide right predictions. Mill maintains that economics' laws are still true, but perturbations are acting in the real social environment. This is the *ceteris paribus* problem described in Chapter 1.

> The a posteriori verification of the hypothesis itself, that is to say examining whether the facts in each case are in agreement with it, is not part of the tasks

of science, but of the application of science. [Mill, 1836].

> The discrepancy between our predictions and the real fact is often the only circumstance that can draw our attention to some important disruptive cause that had eluded us. [Mill, 1836]

In those citations Mill is saying that whenever there is a discrepancy between science's predictions and experience, it is the evidence that some perturbations have not been taken into account. Economics' laws are characterized by *ceteris paribus* clauses, so it is natural that their application to real world will give a negative response; indeed, the clause contains description of other social phenomena that should be combined with the economics' law. When a law's prediction fails to meet the experience, it should not be said that there is a mistake in the pure science, but that the mistake is in the science application. Social scientist fails to include some perturbation that is relevant in the given real situation, nonetheless the economics' law is still true. And the its truth is supported by introspection and deductive reasoning. That's why the title of this subsection is "introspective-deductive method"; according to Mill, the only way to be sure that an economic law is true, is both to apply introspection and to avoid mistakes in the deductive reasoning.

The methodology proposed by Mill can be summarized in three stages:

1. Introspection provides the economist with the starting hypothesis
2. Deductive reasoning is applied in order to obtain laws from hypothesis
3. During the application phase, it is verified if all the perturbations have been taken into account

This methodology can be classified as dogmatic. The only way that researchers have to test laws is introspection. Any discrepancy with experience should be ascribed to some perturbations.

Mill is aware of the fact that sometimes economists are tempted to affirm that they laws are general. To prevent this behaviour he introduces the concept of *tendency*. "He asserted a real result, when he should have affirmed only a tendency towards that result, a force operating with a certain intensity in that direction" [Mill, 1836]. Following [Guala, 2006], "according to Mill, if X tends to cause Y, nonetheless X won't ever be able to make Y happens, because there can always be some perturbation that does not allow Y to happen". But with tendency laws, the issue of the parallelogram law in economics comes back. As before, in any point in space it is possible to know the contribution of each force on a physical body. But how to do that in economics and in social sciences, in general? "The disturbing causes have their laws, as their causes are disturbed; and from the laws of disturbing causes, we can predict a priori the nature and extent of the disturbance [...]. The effect of special causes must then be added to or subtracted from the effect of the general ones" [Mill, 1836]. Here Mill does not provide any arguments neither to support that linearity (additivity) works in economics, nor that all the other perturbations can be formalized.

Mill's work introduces the first methodology of economics. In this very first stage too, economics is facing the pratical problem of induction and the *ceteris paribus* issue. As described above, Mill solves the induction problem by formulating the *a*

priori method based on induction, and the *ceteris paribus* problem by separating the pure economics from the applied economics.

3.1.3 Practical Worries

Economics' laws tell just part of the story. If they are applied to the real world, they will fail to predict experience. The reason is that social phenomena are complex. Scientists have divided the social knowledge in narrow disciplines, each of them approaching the problem with a very limited perspective. At the same time, "practical philosophers" are interested in taking decisions to govern the society. But how they should act? A scientist that is just an economist and that has not other knowledge about other social sciences "must be content with not taking any part in practical politics, having no opinion, or supporting it with extreme modesty, over the applications that should be made of its doctrines to the existing circumstances" [Mill, 1836]. This result follows directly from the idea of Mill that social phenomena are complex and they constitute the subject of different and separate disciplines. Those that are in charge to take decisions should be aware of all human attitudes, and not just focus on the desire to acquire more wealth. They have to know the results of the social sciences and, according to some combination of those, apply results to the real world:

> The method of the practical philosopher therefore consists of two procedures: one analytical and the other synthetic. Egi must analyze the existing condition of society in its elements, not losing sight of anyone: After referring to the experience of the individual to learn the law of each of these elements, [...] there is still a synthesis operation: put together all these elements and, from what they are separately, gather what would be the effect of all the causes operating at the same time. [Mill, 1836]

This citation introduces what Mill would have called later *Etology*. This discipline, in Mill's mind, collects together all the results coming from the social sciences. This idea was then retaken by Vilfredo Pareto in his "Trattato di Sociologia Generale". As will be shown later in the section dealing with Friedman and instrumentalism, Etology is completely different from what is now called *Economics Imperialism*. Very briefly, Mill is well aware of the limited scope of economics and, thus, is willing to integrate it with other social sciences. On the other hand, imperialists apply the limited economic assumption of the maximizing agent to all the other social sciences (and in some cases also to biology).

3.1.4 Hausman and the Inexact-Deductive Method

The main problem of Mill's methodology is that it is too dogmatic. The experience has an influence just on the applied science, while the pure science is based on the (fragile) pillar of introspection. It's reasonable that in some circumstances a discrepancy between the theory and experience induces a modification of the same theory.

As described in the section about the Duhem-Quine thesis, any time a theory characterized by a *ceteris paribus* clause is tested, actually two proposition are being tested: the theory itself and the clause. In economics, the *ceteris paribus* clauses are vague. This feature explains why the methodology proposed by Mill is appealing

to economists; when an experience is not in compliance with the prediction of the theory, ascribe the mistake to the *ceteris paribus* clause, as it is dramatically less reliable than the hypothesis found with introspection. Hausman admits that a false conclusion is the result of uncertain premises, but at the same time upholds that in some cases the whole structure of the theory should be revised.

Hausman describes some methodological rules to understand when a *ceteris paribus* clause is performing a proper role in a theory.

- **Reliability**: in statistical terms, a law characterized by a *ceteris paribus* clause, has to describe a phenomenon that occurs with high frequency.
- **Improvable**: if economists are able to make the *ceteris paribus* clause less vague, the law should become more reliable than before.
- **Excusable**: the clause has to contain just peripheral or secondary influences.

In such a framework, economics' laws become inexact; "Because economic theory includes only the most important causes and necessarily ignores minor causes, its claims [...] are inexact" [Hausman, 2008]. Notice that inexactness "raise serious questions, because if [economic laws] are interpreted as universal generalizations, they are false; and philosophy of science has traditionally supposed that science is devoted to the discovery of genuine laws - that is, true universal generalizations" [Hausman, 2008].

The main drawback of this methodology lies in its name; "inexact" and "deductive" can't be put together in the same sentence. A deduction is valid or not valid, those are the only two possible outcomes of a deductive reasoning.

3.2 Friedman and Instrumentalism

The methodologies described so far share a common idea: starting from empirically verified assumptions, deductive reasoning is applied to discover the logical consequences of the hypothesis. The focus is on the realism of assumptions; if assumptions describe what really goes on in economic decisions, if economists can apply some test to verify economic assumptions, then economic laws are reliable. Mill's introspection is devised to fulfil this role, as it is nothing but a validation method.

The realism of assumptions was the main economic challenge in 1930s and 1940s. Economists tried to implement some method to test the hypothesis of the neoclassical economics. They ask households and entrepreneurs to answer some surveys about the economic decisions they take. Eventually it turned out that entrepreneurs, instead of setting the price to maximize the profit by taking into account complex cost's curves, just add a margin percentage on top of direct and indirect costs. At that time, this result was regarded as the proof of the neoclassical economics inconsistency.

Friedman's methodological essay has to be interpreted in that setting. The thesis upheld by Friedman is that "the ultimate goal of a positive science is to be found in the development of a theory or hypothesis that generates valid and meaningful predictions about phenomena not yet observed" [Friedman, 1953]. Realism of assumptions is not an index of a theory usefulness. Science is just concerned with

predictions; theories that fail to provide correct predictions, are useless and should be abandoned.

After having described Friedman's methodology, this chapter presents the consequences of it and the economic imperialism; finally, the main drawbacks of instrumentalism are introduced and discussed.

3.2.1 The *As If* Methodology

Let's suppose that a theory describing how leaves place in tree's branches has to be developed. Two scientists are asked to study this problem. The former is a realist, namely she wants to understand the real processes engaged in leaves' placement. The latter is an instrumentalist, that is, she is agnostic about the biological phenomena involved in the problem and her only objective is to come out with a theory that is able to predict how leaves place in tree's branches.

It is easy to see that the main issue faced by the realist is the practical induction problem. Biological processes can be very complex to be described and their combination can be difficult to be understood; furthermore some disturbing and not systematic cause may affect the phenomenon, for instance the fact that the tree is placed in a wood, so also other trees' "behaviour" has to be considered. Finally the ingenue realist , whose objective is to develop a theory based on fully real assumptions, is not able to do anything because the phenomena are in general too complex. This is the first issue underlined by Friedman: absolute realistic assumptions are impossible to be obtained.

Now let's focus on the work of the agnostic scientist. She is not interested in biological processes and maybe she does not know anything about botany. The instrumentalist just observes the phenomenon and her aim is to develop a useful rule, a reliable tool, that gives right predictions about the phenomenon. Notice that rules are different from laws. The latter can be true or false; the law according to which "all swans are white" is not true because in Australia a black swan has been discovered. On the other hand the rule that says "If you meet a swan, you are allowed to infer that it is white", it is neither true nor false, but it can be more or less useful; for instance in Australia this rule is useless, because its outcome is likely to be falsified by the experience, but again it is not false. Coming back to the instrumentalist scientist, her aim is to find a useful rule, a rule whose outcome is consistent with experience.

> Considered as a set of substantial hypotheses, the theory must be judged according to its predictive capacity with respect to the class of phenomena which it is intended to "explain". Only proof of facts can show whether it is "right" or "wrong" [...]. The only relevant verification of the validity of a hypothesis consists in dealing with its predictions with experience. [Friedman, 1953]

In the previous citation there are some interesting points that have to be pointed out. The first one is the well-known Friedman methodological idea that hypothesis have to be verified with respect to their predictive power. The second interesting point is words in double-quotes ("explain", "right", "wrong"). An instrumentalist theory does not "explain" anything. It does not start by some well-verified assumptions to arrive to a consolidated conclusion. An instrumentalist theory just provides

predictions, and to do so it uses rules, that can be more or less useful, but neither "right" nor "wrong".

In the pursue of developing a rule, the agnostic scientist faces one crucial issue. She may guesses, for instance, that leaves maximize the amount of light received from the sun; but at the same time she might find another assumption whose predictions are in line with leaves' behaviour. In general, it is possible to find an infinite number of hypothesis whose predictions are consistent with the phenomenon:

> Thus understood, the validity of a hypothesis does not in itself constitute a sufficient criterion for choosing between alternative hypotheses. The number of observed facts is necessarily finite; those of the possible hypotheses, infinite. [Friedman, 1953]

Friedman has in mind a precise methodological rule that helps economists to choose the right hypothesis:

> The choice between equally compatible alternative hypotheses must be somewhat arbitrary, although it is unanimously admitted that one must base oneself above all on considerations dictated by the criteria of simplicity and fruitfulness. [Friedman, 1953]

> In fact it will be possible to ascertain that truly important and significant hypotheses have as "assumptions" of extremely inexact representations of reality and that, generally, the more significant a theory is, the more unrealistic are (in this sense) the assumptions, for a very reason simple: a hypothesis is important if it "explains very little". [Friedman, 1953]

First of all, Friedman is not stating that the more unrealistic the assumptions, the more powerful the theory. Friedman is arguing that powerful and reliable theory are based on unrealistic hypothesis because, despite their simplicity, they are able to produce fruitful predictions. So, the assumption that leaves maximize the amount of light received from the sun is simpler that some complicated theories belonging to the botany or to biology, but at the same time it can be used as a reliable rule to predict leaves' position.

The link with neoclassical economics is actually straightforward. Neoclassical economics deals with the existence of a general equilibrium of full employment. In this setting, ask entrepreneurs if they set price of the goods they sell in such a way to maximize profit is useless. The important point is that they behave as if they maximize profit. If their behaviour was not consistent with this assumption, they wouldn't be able to survive competition. Notice that the profit maximization assumption is a simple hypothesis whose consequences are in line with the existence of a general equilibrium of full employment. This is indeed a "good assumption" for the neoclassical economic theory. If this hypothesis was used to build a theory of how, in reality, entrepreneurs set prices, this would be a bad hypothesis. But the set of phenomena the assumption is concerned with, are not the real behaviour of entrepreneurs, but the general equilibrium existence.

To sum up, the methodology proposed by Friedman is based on three main concepts:

- An absolute realism is impossible to be obtained
- Assumptions realism is not an index of a good theory
- Theories should be judged only with respect to their predictions

3.2.2 Economic Imperialism or Mill's Defeat

According to instrumentalism, science should provide reliable and fruitful rules that allow to predict with a good precision phenomena that are taken into account by the same science. Realism of assumptions does not matter at all. In order to achieve realism, one has to add useless complications to assumptions and, in the end, the resulting theory is not useful.

In the economic framework, this reason translates into the fact that asking if agents are rational (here rationality stands for a maximization or minimization behaviour) is not important. Actually it might be well the case that agents do not try to maximize their utility.

According to instrumentalism, economic theory is not based on real economic assumptions. If the same theoretical framework offers good predictions in other fields than economics, why do not use it? This is the starting question of economic imperialism. Imperialists are perfectly aware of the fact that in politics, in human relationships and in social life in general, people do not maximize anything. This feature is perfectly consistent with instrumentalism. Furthermore, they would say that even in economic decisions, people do not maximize their utility; and it is definitely not important, because a theory should not be assessed by looking at the realism of its assumption, as long as it can offer good predictions.

Mill's methodological stance is completely on the opposite side with respect to the imperialists' one. Mill, who is a realist, upholds that human behaviour is complex, determined by a multitude of causes. The only way to understand it is to divide this field in different and separate disciplines, each of them having a narrow scope, investigating a limited human attitude and approaching the problem from a narrow perspective. Then, once all the social (moral, in Mill's words) sciences have found their relevant causes, they should be combined together in an upper-level science called *Etology*, that eventually is able to describe in detail the effect of the combination of causes.

On the other hand instrumentalists completely neglect causes and, consistently with their methodological idea, are not interested in realism of assumptions. They just the utility maximization assumption, and apply it to the whole social life.

A part from general issues faced by the instrumentalism, that will be described in the following subsection, economic imperialism raises two specific problems:

- **Utility**: as stated before, imperialists "explains" human behaviour in social life by assuming that agents maximize their utility. The problem lies in the utility's definition. In general instrumentalists do not collect empirical evidence about human behaviour and then translate that evidence in an utility function. They begin their research by assuming an utility function and they show that it is consistent with empirical data. This procedure is methodologically unacceptable because it is always possible to find some mathematical functions consistent with data. It is true that assumptions do not have any

importance in such a framework, but a science based on this methodological procedure is unfalsifiable. [Hodgson et al., 2012]

- **Normative Issue**: from a normative point of view, assuming that people always maximize their utility, that mainly deals with self-interest, is not neutral. First of all, it seems that all the human activity can be reduced to self-interest. In Mill's thought, men are complex, they are guided by different causes, the social life is regarded as a very complex entity, and there is no attempt to reduce it. According to instrumentalism, social sciences deal with a one-sided man. Sentiments, inclinations, passions and all the other aspects that characterize the human nature are completely and intentionally neglected because it is stated that men behave as if they only maximize utility. Furthermore, there are theories describing that theories focused on self-interest behaviour make people become more favourable toward self-interest ([Marwell and Ames, 1981]).

3.2.3 What Is Wrong with Instrumentalism

In this section, three main problems concerning instrumentalism are described. They are introduced in increasing importance order.

1. **Historical Consideration**: instrumentalism has been used in science for scientific theories with a remarkable predictive power and, at the same time, with complex assumptions to be understood. For instance, Andreas Hosemann, in XVI century, interpreted the Newton's theory of gravity in an instrumentalist fashion. At that time, scientists faced two main issues with the theory of Newton: it wasn't accepted by the Catholic Church, and it was based on the assumption of "force over a distance". This latter concept wasn't clear in XVI century, nonetheless the gravity theory became a good tool to predict astronomical phenomena. Another example, is the instrumentalist interpretation of the quantum mechanics. This physical theory is the pillar of many applications as it gives reliable predictions, but it is based on difficult assumptions. In both the examples described before, there are some real facts that are foreseen by theories. In the case of Newton's theory of gravity, planets' revolution and solar system structure are contained in the theory. It is easy to understand that those are real facts: by means of a telescope one can verify that planets move on a elliptical trajectory around the sun.
   The neoclassical economics focuses on the general equilibrium of full employment. This result is obtained by applying some mathematical-deductive reasoning to the assumptions of theory. But is the general equilibrium of full employment a real fact which reliable predictions can be based on?
   From an historical point of view, instrumentalism has been used in epistemology when high-predictive theories was based on complex assumptions. The neoclassical economics does not have that high predictive feature and on the other side its assumptions are quite simple.
   Furthermore, instrumentalism in general is not consistent with the way according to which natural science's theories evolve. Even though predictions of the Newton's theories of gravity can be still used nowadays because they are quite precise, it is known that the theory is false and has been replaced by the Einstein's theory of gravity.

2. **Practical Applications**: Friedman's instrumentalism is based on three points [Guala, 2006]:

   (a) A good theory offers reliable predictions of the phenomena is concerning with.

   (b) Theories should be judged just according to the predictions they give.

   (c) Any other criterion, included the realism of assumptions, is useless.

   In simple terms the process can be described as follows; a social scientist observes a phenomenon, she devises some unrealistic and possibly simple assumptions, she applies a deductive reasoning to those assumptions and she gets a prediction in line with the phenomenon. As usual the phenomenon is not explained; instrumentalism is not aimed at explanation. Since instrumentalist theories do not explain anything, any causal relationship is, by definition, neglected. This consideration shows the biggest problem of instrumentalism. If those theories do not have any knowledge about causal relationships, how can they be useful in practical applications? Instrumentalist theories are at most good tools to predict phenomena in the current state of the world. But as soon as something in the world changes, they become useless rules as they completely miss the causes-effects relationships.

3.3 Popper, Kuhn and Lakatos

This section deals with three of the most famous and influential philosophers of the XX century. Essentially, every work that regards the methodology of science or epistemology, should contain a reference to those masters. Before the detailed description of their philosophical project and structure, some warnings should be pointed out to prevent easy misunderstandings and not-targeted critiques.

First of all, the biggest share of their methodologies is concerned with natural sciences. The only author that extensively included economics, and social sciences in general, in his project was Popper. Thus, any economic application of Kuhn's or Lakatos' philosophical stance is the result of work carried out by economists. As it will be shown in the following, it will be impossible to trace a precise relationship between Kuhn's methodology and economics.

Secondly, they belong to three different thought school. Popper is a rationalist that completely rejects the use of history in epistemology. The aim of his philosophy is to solve at the same time both the demarcation problem and the induction problem, by building up a methodology that enables scientists to justify and control their theories by using deductive reasoning and by looking at natural phenomena. Kuhn underlines the beneficial results that can be reached by taking the history of science into account. The historical analysis of science could shade light on the methods that scientists apply in their day-to-day operations. However, according to Kuhn, reason it's impossible to explain the most relevant scientific happenings, the scientific revolutions; there are other causes, outside the rational scope, that make scientists change paradigms. Kuhn is an elitist, in the sense he justifies the scientific progress by saying that scientific revolutions are solved by the scientific community and thus, by the science elite. Lakatos is a rationalist that uses history to show that there are rational principles that lead science toward progress and truth. His aim to

apply crucial modifications to Popper methodology in order to save the rationality of science. To summarize what has been just said bout Popper, kuhn and Lakatos, it is possible to provide an easy example. The three philosophers are asked this question: "Why Newton's solar system is better that Ptolemy's one?". Popper would answer that Ptolemy's solar system has been falsified by experience and that to save it, scientists introduced *ad hoc* hypothesis (it will be shown that this same problem applies to Newton's gravity). Kuhn would reply that the proof of the progress from Ptolemy to Newton was the fact that physicians on their own decided to shift to one paradigm to another. Lakatos would say that geocentric system was a regressive programme while heliocentric programme was progressive in the sense that allowed to predict new facts.

In the end, despite the fact that Popper's, Kuhn's and Lakatos' methodological proposals are general, this chapter contains just their direct application to neoclassical economics. For instance, following a Lakatosian terminology, there exists a Marxian research programme in economics, but it is not the subject of the following description.

3.3.1 Popper on Demarcation and Induction

The aim of Popper's methodological project is to solve both the demarcation problem and the inductive problem. The latter has been explained in the first chapter of this work. In particular, Popper is concerned with Hume's formulation of the inductive problem, that is the logical inductive problem. Popper recognizes the existence of the issue, but in the end he provides an opposite solution with respect to the one provided by Hume. The demarcation problem is concerned with the possibility of drawing a precise borderline that separates science from pseudosciences. Popper is well aware of the fact that a solution of the demarcation problem has got critical and practical applications. For instance, if the solution of this problem implies that traditional medicine is science while homeopathy is pseudoscience, the society should boost investments on traditional medicine and completely neglect homeopathy.

Popper was able to find an answer to the demarcation problem when he was seventeen (1919). When he was young, he was fascinated by three theories: Freud and Adler's psychology , socialism and Einstein's relativity. He noticed that while Freud's psychology and socialism were able to fit all phenomena, Einstein's relativity clearly states that the practical consequences of Newton's gravity were false. Furthermore the theory provided by Einstein could have been falsified by the experience by performing experiments (notice that in 1919 Eddington actually performed those experiments about gravitational deflection) while, for the internal logic of psychology and socialism, those theories were unfalsifiable. From those considerations directly follows the solution of the demarcation problem: scientific theory are characterized by a logical structure that allow experience to falsify the same theory, or even simpler, scientific theory must be falsifiable:

> As demarcation criterion, it hasn't to be considered the verifiability of a system, but its falsifiability. [Popper, 1963]

Given the solution of the demarcation problem provided by Popper, it should be shown how he disentangles the inductive issue. Popper noticed that science uses

propositions characterized by general terms, for instance "All swans are white". The fact that the previous sentence is characterized by the term "all" implies a strong asymmetry. The essence of this asymmetry is that one single experience can falsify the scientific law, but even a huge number of positive experiences are not able to verify it. But this is indeed the essence of falsifiability. A scientific theory has not to be verified once for ever as it is impossible. It is just required that it is corroborated, in the sense that so far its implications are in line with the experience. Popper states that the science simply neglects the inductive problem. Scientific theories are corroborated conjectures that so far has performed well in tests with natural phenomena. The asymmetry that characterizes scientific propositions implies that they won't be definitely verified and that's because they are scientific propositions. Scientific theories are not the product of inductive inferences, but they are just conjectures that have to be tested with respect to natural phenomena.

> The best that we can say about an hypothesis is that so far it has been able to show its value, and that it has had more success than other hypothesis, though in principle it can't be neither justified nor verified, neither it can be shown that it is probable. [Popper, 1963]

3.3.2 Popper on Discovery and Inductivism

Popper states that theories' falsifiability is the solution of both the demarcation problem and the induction problem. Furthermore, Popper criticizes the inductive reasoning by saying that its consequences are infeasible. The argument upheld by Popper regards the context of discovery. Popper precisely separates the context of discovery from the context of justifiability. According to the inductive reasoning, scientists observes a quite large number of similar phenomena and, in the end, they come up with a law that describes the natural world. This procedure is concerned with both the context of the discovery and the context of justifiability. The inductive inference is the base of the discovery while the inductive logic is the base of the justification. Popper argues that the expression "similar phenomena" is senseless if it is not preceded by a common theoretical ground. Thus, any observation is full of theory. Popper simply states that to notice that there is a repetition of events, that two or more phenomena are alike, the scientists that are looking at them have to accept a common theoretical base. The inductive stance according to which phenomena are neutral is unacceptable. Furthermore, the difficulties of proving the inductive justification without referring to a cyclic reasoning suggests that also the inductive justifiability is critical.

Hence Popper separates the context of discovery, from the context of justifiability. The former is concerned with the individual psychology, while the latter is the subject of his methodology.

It is worth to dwell on this distinction. For what concerns the justifiability, Popper argues that as soon as a theory is not in compliance with the experience, it should be abandoned. Here the challenge is always between one theory and the experience; the result can be either "accept the theory" or "refute the theory". All the problems and the issues of this methodology will be pointed out in the following. As an anticipation, it can be said that this version of falsificationism, that it is named logic falsificationism, is not acceptable.

The way Popper shapes discovery is more interesting. Neutral experiences does not exist and any observation of the natural world implies a theoretical background. The mind sheds light on particular phenomena, while it is impossible that neutral experiences are impressed in the mind as the writings of chokes on blackboards. The reactions that scientists have when they observe the world are *a priori* but they are not valid *a priori*. If the conjecture proposed to explain some empirical recurrence is falsified by the experience, scientists should redirect the light shed by their mind according to a trial and error process that in the end will allow the scientific community to discover corroborated conjectures. In this context also metaphysics assumes a new position. In contrast with neopositivism, that searches a method to separate sense sentences from no-sense sentences, Popper clarifies that his demarcation principle has just the aim to separate scientific propositions from pseudoscience. Hence, according to Popper, metaphysics is not senseless, but it is just non-scientific. Popper upholds that metaphysical ideas have great importance in scientific progress and in the lead of scientific discovery.

3.3.3 Popper and Social Sciences

Popper spent most of his academic career at the London School of Economics so it is quite natural that he suggested a methodology also for social sciences. Some authors ([Barrotta and Raffaelli, 1998], [Guala, 2006], [Motterlini, 1997]) argues that his idea of social sciences and economics in particular has been influenced by F. Von Hayek, economist that belongs to the Austrian school. As it is well known, the philosophical background of that economic school is methodological individualism and subjectivism (in seek on precision, authors like Merger and Von Mises upholds that economic agent behaves in a subjective fashion, but economics is able to reach objective conclusion. This tension is solved by referring to Aristotelianism and apriorism. Since those two philosophical positions are no more acceptable, Austrian economics is turning to hermeneutics. Unfortunately, as pointed out by [Barrotta and Raffaelli, 1998], also this new flow has got internal contradictions).

According to Popper social sciences should investigate non intentional side effects that characterized human behaviour. This is not an original idea; Adam Smith's *invisible hand* is nothing but this. In line with ideas that have been probably suggested by Von Hayek, Popper social sciences should not investigate phenomena regarding nations, societies or, in general, groups of individuals, but the focal point of social sciences should be always the individual. In order to provide conjectures that describe individual's behaviour, social scientists have to develop models that takes into account the situation in which individuals act. This is the situational logic, that implies that models deal with typical situations in which typical agents act. In addition to this situational logic, Popper states that a very light rationality principle has to be inserted in the model to make it "alive". According to this rationality principle, individuals behave consistently with the situation and with their ideas. This rationality principle is light in the sense that there is not a direct reference to utility maximization or rationality of expectations.

Now the problem is that, soon or later, the model should be tested. An empirical verification is required and the logical falsificationism shows its problematic face. According to the way models are built, any empirical test includes the description of the situation and the rationality principle. In the case of a falsifying empirical

response, the social scientist does not know which of the two ingredients is critical. This is actually the Duhem-Quine thesis described in the first chapter of this thesis. Social scientists need an extra-methodological decision to understand which element has to be modified:

> If a theory has been controlled and has been falsified, then we have to decide which of its parts has to to be judged responsible of the failure. My thesis is the following: decide to consider responsible not the rationality principle, but the rest of the theory, that is to say, the model of the situation, is a good methodological politics. [Guala, 2006] (that cites [Popper, Models, Instruments and Truth])

This methodological politics is in contradiction with the logical falsificationism. It is not possible to falsify a theory just by using the *modus tollens* of the deductive logic. Some extra methodological decisions (Duhem's *bon sensi*) has to be taken. The position on which Popper lies, for what concern social sciences, collides with the falsificationism of natural sciences proposed by Popper himself.

3.3.4 Criticalities of Popper's Methodology

Popper's falsificationism is to some extent, the product of XX century's history. This philosophical position is actually an educated reply to the dismal philosophies and politics that dominated Europe in those years. The final goal of all Popper's work to promote democracy and it encourages the respect paid to other people opinions. In that sense, Popper's philosophy constitutes a great project. On the other hand, the methodology proposed by the Austrian philosopher faces serious critiques that it has not been able to overcome. In the following to critiques are reported, the former is the Duhem-Quine thesis, and the latter is an historical consideration. Those issues clarify Kuhn's and Lakatos' proposals.

- **The Duhem-Quine thesis.** As described in the first chapter of this work, the argument upheld by the French physician and by the American philosopher states that it is impossible to test an insulated hypothesis with the natural world. Any hypothesis is tested with other auxiliary hypothesis and in case of a negative response, scientists do not have any logical indication about which hypothesis failed to materialize. Popper mistakenly proposed the logical falsificationism that does not take this critique into account. It is possible to develop a methodological falsificationism, by introducing some extra-logical rules that scientists should follow. But falsificationism was born with the aim of making those controversial and not precise extra-logical rule useless. In that sense, the method presented by Popper for social sciences is in contradiction with the original objective of his project.

- **Historical Consideration.** Falsificationism is also falsified, or unacceptable, from an historical point of view. By looking at the history of sciences, it can be noticed that scientists do not behave as Popper suggests. And this is not because scientists are trying to cheat, but because the methodology of falsificationism is too strict. Consider, for instance, the discovery of Neptune. In 1781 F.W. Herschel discovers a new planet in the solar system, Uranus. Bouvard, a French physician, computes the orbit of the new planet according to Newton's

gravitational law. Unfortunately, Bouvard's computations fail the empirical test; Uranus's orbit, for some reasons, does not follow the predictions implied by Newton's system. According to falsificationism, Newton's gravity should be refuted as soon as the results of the empirical tests come out. But this is not the right strategy to follow and scientist, at that time, didn't act according to Popper's methodology. They assume the existence of a new planet, Neptune, that, with its mass, alters Uranus' orbit. In 1845 Adams computes the orbit of Neptune and its mass, but the scientific community completely neglect this discovery. The following year, in 1846, Le Verrier shows some results similar to the one presented by Adams. Le Verrier persuades the scientific community to empirically search the new planet. Telescopes are pointed in the region of the space where Neptune should be according to Le Verrier's predictions. This time too, Neptune is not observed. Despite this new failure, Newton's gravitational law is still considered valid. Some months after, Berlin observatory, after some lucky circumstances, is able to identify the new planet, Neptune. This example clearly shows that falsificationism is neither a good representation of scientific history, nor an efficient methodology. It is at least debatable that refuting a stable and reliable theory, like Newton's gravity was at that time, is a good methodology.

Popper's philosophical stance was able neither to overcome nor to solve those issues. Kuhn and Lakatos, in the following years, have presented other methodologies that, to some extent, are able to circumvent those problems. Kuhn introduced a little bit of irrationality in its historical analysis, while Lakatos tried to combine Popper's falsificationism with historicism.

3.3.5 Kuhn and the Concept of Paradigm

Kuhn in [Kuhn, 1962] attempts to destroy an anti-historical idea according to which scientific progress is just the accumulation of separate and single empirical discoveries or theoretical inventions. Kuhn's objective is to use an historical approach to understand or at least describe the path that science has walked from the origin. By looking at history, Kunh notices that ancient theories about the world that now are considered naive and not at all scientific, had a scientific role at their time. So, if science is considered from an historical viewpoint, a philosopher should admit that the human activity that is called science has contained beliefs that are not consistent with the ones that nowadays are considered scientific. Kuhn is well aware of the fact that methodology on its own is not able to provide answers to all the issues concerning epistemology. Some extra-methodological aspects (like social, political or even individual facts) are able to influence the way according to which scientific communities look and investigate the natural world. Furthermore, history of science, as Kuhn states, suggests that theoretical inventions (deprived of their instantaneous peculiarity commonly accepted) have led the science toward scientific revolutions, that are critical and fundamental transformations of the world in which the scientific work is carried out. The historical analysis that constitutes the pillar of Kuhn's methodology, allow the American philosopher to solve the second issue faced by Popper's falsificationism; history teaches that the fight between different sections of the scientific community is the only historical process that has as a result the abandon of one theory and the acceptance of another one. According to this

view, it is possible to understand why physicians didn't refute Newton's gravity at the time of Neptune's discovery. No other efficient scientific theory was available at that time. As it will be shown later, this neglecting process is also beneficial for the progress of normal science.

Kuhn introduces the concept of paradigm, a scientific practice, that includes models, laws, applications and instruments, that is considered to be valid. Paradigms provide models that can initialize scientific research tradition characterized by an internal consistency. A period in which the research is carried out without the lead of a paradigm can exist and it is called pre-paradigmatic period. This pre-paradigmatic period is highly inefficient and characterized by some random researches. The reason is that, without an accepted paradigm any empirical facts can influence scientific developing. Soon or later a paradigm emerges and the period of normal science begins. The effects of the shift from a pre-paradigmatic period to a paradigmatic research are:

- There is a new and stricter definition of the research field: researches that do not comply with the paradigm are forced to abandon the field and can be considered to be out of the science.
- It becomes useless to show that one single discovery has as its base the principles stated in the paradigm. This fact will boost the efficiency of the normal science.

Even if it is sure that a paradigm soon or later will emerge, it is not so easy to understand why one paradigm can defeat the others. Somehow, it has to be the most promising among the others included in the challenge.

Generally, the paradigm that is able to emerge is characterized by a limited scope and precision. Nevertheless, it emerges because it is able to solve some problems that scientists consider important and urgent. But, according to Kuhn, that solution cannot be complete. The overlapping between paradigm and nature is not complete. Some grey spaces are left. From those grey spaces, normal science will pick puzzles to solve and, at the same time, they will be the origin of anomalies and crisis that eventually will lead to scientific revolutions. Thus a paradigm is a reliable promise that problems still unsolved, will be eventually disentangled.

3.3.6 Normal Science

Kuhn's informal definition of paradigm is a reliable promise that unsolved problems will be, eventually, solved. The entity that is in charge of fulfil this promise is the normal science. The object of this type of science is to enlarge and deepen the knowledge of the facts considered scientific by the paradigm. Normal science forces the nature to fit in the narrow and predetermined categories provided by the paradigm. Kuhn clearly states that the aim of normal science is neither the discovery of completely new phenomena, nor the developing of new theoretical structures. This basic type of science faces just three classes of problems: determination of relevant facts (relevant according to the paradigm), the comparison between empirical phenomena and theories and, finally, the improvement of the theory. This implies that applications of normal science, both theoretical and empirical, improve the scope and the precision of the paradigm that constitutes its background. Scientists spend most of their time facing puzzles whose solutions give them scientific authority. The

scientific puzzle is characterized by a certain solution and its value is measured by the difficulty of that solution rather than the intrinsic usefulness of the solution. In their quest for the solution, scientists apply rules that determine feasible results and certified logical passages. Those rules have the shape of assumptions derived from the paradigm.

Kuhn specifies that rules, paradigms and normal science are three different entities. Once a paradigm has been accepted, it induces a period of normal science. However, a paradigm not necessarily determines a set of rules. A commonly shared paradigm does not imply a commonly shared set of rules. Scientists, in their research activity, assumes as starting point the models that constitute the essence of the paradigm. Scientific community is so sure about those models that there they are accepted with a large dose of dogmatism.

3.3.7 Anomalies, Crisis and Scientific Revolutions

As stated before, Kuhn conceives normal science as the cumulative activity whose objective is to make more precise predictions and to enlarge the scope of the models contained in the paradigm by solving puzzles. Nevertheless, history of sciences shows that scientific revolutions have occurred. Anomalies and crisis are the roots of this revolutionary happenings.

The discovery begins with the observation of an anomaly, that is the proof that nature has violated the predictions implied by the paradigm. The discovery is not a simple act that coincides with the sentence "see something new". The discovery is a complex phenomenon that entails both that the scientist is aware the something new exists and that she knows what it is. Anomalies play a key role in the path toward novelties. Kuhn emphasizes the dichotomy of normal science; even if this activity has not the objective of discover novelties, it is so efficient in doing so. Furthermore, without a dominating paradigm, anomalies and eventually novelties, cannot be noticed. Kuhn states that anomalies on their own are not able to force a change of the paradigm.

Light change of paradigm, destructive and constructive at the same time, are the results of empirical discoveries that were not predicted by the paradigm. Heavier change of paradigm are theoretical inventions. A new theory implies a critical modification or a fundamental destruction of the preceding theory. When the new theory emerges, the old one is clouded by an increasing uncertainty due to its incapacity of solving relevant puzzles. Kuhn states at this stage the old paradigm undergoes huge and complex modifications that are not entirely accepted by the scientific community. The example provided by Kuhn is the state of the Ptolemy's system just before the Copernican revolution. Kuhn is aware that the failures shown by the normal science is not the only proof associated with a crisis (there can be underlined political, social and economic issues as well), but it is essentially the most important. A new theory becomes a serious rival of the dominating paradigm just in the case that the normal science is no more able to solve puzzles. In this case the normal science is in a crisis period.

Crisis are a necessary step for the new theory to emerge. The historical analysis performed by Kuhn pointed out scientists' behaviour in response of critical periods. Their confidence on the dominating paradigm decreases, but they do not instantaneously abandon it. As stated before, Kuhn upholds that just a challenge between

two paradigms is able to determine the refusal of one of the two. Once a paradigm has reached a dominating position, it is declared invalid just in the case that another paradigm is ready to replace it. The comparison is never of the form Theory-Nature, as Popper suggests, but it is Theory 1-Theory 2-Nature. When a community decides to abandon a paradigm, it decides at the same time to accept a new one. Simple refute a paradigm, without the consequential acceptance, is the decision of exit from the scientific field. Kuhn provides plenty of historical examples in which scientists, when face an anomaly, introduces *ad hoc* hypothesis in order to eliminate the discrepancy between the paradigm and the nature. Anomalies can induce a period of crisis or can worsen an existing critical period, but they are not able of their own to falsify a paradigm. Kuhn underlines this aspect of his methodology by saying that anomalies can be seen at the same time as the root of crisis and as issues that normal sciences will eventually solve. When an anomaly is able to survive to a huge number of attacks made by the normal science, when that anomaly is perceived as something more critical by the community and when there are at least one other theory ready to replace the dominating one, the shift from the normal science to the extraordinary science takes place.

Crisis has two effect:

- Paradigm loses focus and normal science's rule become less strict. Furthermore, controversial *ad hoc* hypothesis are introduced.
- Every crisis end with a new paradigm that is ready to challenge the dominating one.

The possible transition from one paradigm to another is not at all a cumulative phenomenon. It implies the building of new pillars able to sustain the research field.

A scientific revolution is a non cumulative event in which an old paradigm is replaced by a new one. The two paradigms involved in this revolutionary activity are completely different and not at all consistent with each other. Kuhn tries to explain how a fight between paradigms is performed. In this description it is possible to find the proof of Kuhn's belief that methodology and scientific rationality on their own are not able to explain entirely the history of sciences. The scientific parties involved in a revolution have to use persuasive arguments to win the competition. Since the fight involves two paradigms that are completely different for what concerns permissible scientific phenomena, models, empirical techniques, the challenge cannot be solved just by using logical or empirical arguments. This is because any defensive topic has necessarily been originated by the same paradigm. Since it is impossible to find a superior or neutral empirical base, and since argument are characterized by a clear circularity, the fight of the two opposing paradigms mostly takes place in the field of persuasion. This implies that methodological and logical considerations are useful during the normal science period. When a scientific revolution occurs, the differences of the two paradigms are so evident and unsolvable that is impossible to address the conflict with rational means.

Kuhn shows the influence of the Gestalt, a psychological theory, when he describes how the scientific activity changes after a revolution. When a paradigm changes, the world around scientists changes as well. Led by a new paradigm, scientists investigate the world with new instruments and approach it from different directions. At the same time, Kuhn underlines that one of the most interesting lecture told by history is that scientists, after a revolution, are able to distinguish new

scientific entities even when they use the empirical techniques of the old paradigm. The world in which scientists work is not determined once for ever by the nature on one side and by the science's nature on the other. It is rather the product of the interaction between the nature on one side and the dominating research tradition on the other. That's why when a revolution has been carried out, the world in which scientists are involved changes. Kuhn stresses out the fact that it is not just the interpretation of empirical phenomena that varies after a paradigmatic shift. Any interpretation is always preceded by a paradigm and a change in interpretation is just the superficial effect of a shift in paradigms.

Kuhn in the last chapters of [Kuhn, 1962] shows the reason why it is commonly upheld that science is a cumulative activity. Scientists and common people acquire information about science through an authoritarian source that for practical reasons, hide the existence and the meaning of scientific revolutions. This authoritarian source consists of scientific books, divulgation books and philosophical stances shaped consistently with the dominating paradigm. In particular, scientific books refers to the part of the past scientific tradition that can be easily included in the new dominating paradigm. The activity of those past scientists is presented as perfectly consistent with the fundamentals of the dominating theory. But of course this consistency is obtained by means of distortions and alterations. Anyway, the main result of this activity is to build a linear and cumulative picture of the science.

3.3.8 The Meaning of Progress

As described before, scientists involved in paradigm shift seem to be lead by a persuasive force rather than logical or methodological considerations. The difference between two conflicting paradigms is so deep that any topic in favour of one paradigm has got its root in the paradigm itself, leading to a cyclical argument. Different paradigm differs for what concerns the problems that science should include and address, and most important for the term they use. Sometime for chronological reason, two different paradigm use the same terms but with different meanings, or with completely different relationships with the remaining theoretical structure. In economics this lexical issue is quite evident. Adam Smith's *invisible hand* has got a completely different meaning with respect to the one presented in the marginalist and neoclassical economics. Sometimes it seems that neoclassical textbooks try to introduce invisible hand as a foundation of their theory. It must be clear that the neoclassical tradition is completely different from the classical economics. For instance, those two traditions uphold two opposite value theories. Classical economics deals with the theory of value-labour, while neoclassical economics is concerned with the theory of value-utility. The distributional (in terms of wealth) consequences of those two theories are radically different, actually so different that any reference to the invisible hand in neoclassical textbooks are at least naive.

Given that scientific revolution can be poorly explained just by referring to logical and methodological tools, Kuhn asks whether the path on which science is walking leads to progress. In the introduction of this chapter, Kuhn has been defined as an elitist. Thus, according to Kuhn the only position that ensures that science is pointing toward progress (even if a re-defintion of progress is needed) is that scientists are free to choose which paradigm has to be developed during a scientific revolution. Kuhn's elitism should be considered in those terms. Scientific community

is an elite that if let free to choose their rules, will lead science toward progress.

3.3.9 Lakatos' MSRP

Lakatos was born in Hungary in 1922. He witnessed all the Hungarian political affairs of the XX century, from the Nazi occupation to the Russian regime. He left Hungary for political reasons and he attended Popper's lectures on methodology at the London School of Economics. His philosophical background allowed him to combine historicism with Popper's *naive falsificationism*, to produce a new methodology called *sophisticated falsificationism*.

Lakatos is a rationalist; he believes that science has to be separated from pseudoscience by applying some universal criteria. Following those criteria is possible to investigate and categorize any product of the human knowledge. He shares this goal with Popper; nevertheless the Hungarian philosophers is well aware of the fact that the naive falsificationism is unacceptable due to both methodological reasons (the Duhem-Quine thesis) and historical reasons. On the other hand, Kuhn's methodological project points out some peculiarities of scientists' behaviour by investigating the history of science, but is, from Lakatos' viewpoint, unacceptable as well. As reported before, Kuhn argues that scientific revolutions are, in the end, a matter of persuasion, trust and beliefs. Those irrational entities collide in a destructive fashion with Lakatos' rationalism. However, Lakatos is able to find a synthesis of historicism and rationalism, the rational heuristics, that is a set of instructions leading the scientific research. Influenced by Kuhn's concept of paradigm, Lakatos introduces the concept of research programme. Research programmes consist of:

1. **Hard Core**. It is a set of fundamental assumptions or basic theoretical decisions commonly shared by the scientific community. Those decisions are declared unfalsifiable by the scientific community. As long as the research programme dominates the scientific landscape, no scientist would investigate the theoretical entities that constitute the hard core of the programme.

2. **Metaphysics**. Research programme's metaphysics, that can be investigated and criticized, is expressed by means of heuristic rules. In particular, those rules can be split in two categories; the negative heuristic, that tells scientists which research path to avoid, and the positive heuristic, that leads scientists in the discovery of new facts. The positive heuristics and the metaphysics in general, underline the continuity of research programmes. Heuristics are then a critique of Popper's idea about the context of discovery. According to Popper, discovery, that can be guided by metaphysics as well, is the subject of individual psychology. On the contrary, the rational peculiarity of Lakatos' heuristics suggests that also discovery is a process characterized by rationality; "The profane sees the birth of physical theories in the same way that the child sees the birth of the chicken. He believes that this fairy to whom he attributes the name of science has touched the forehead of a man of genius with his magic wand and consequently the theory, living and complete, was immediately manifested, in the same way that Pallas Athena emerges armed from the forehead of Zeus. He thinks that it was enough for Newton to see an apple fall in a meadow because, suddenly, the effects of falling bodies, the movements of the Earth, the Moon, the planets and their satellites, the comet

journeys, the flow and the the ocean's reflections could be summarized and classified in this unique proposition: any two bodies attract in proportion to the product of their masses and inversely to the square of their distances" [Duhem, 1906].

3. **Protective Belt**. Any research programme has to face plenty of anomalies. Scientists should hence develop a protective belt consisting of assumptions, that is able to absorb anomalies. Those low level assumptions can be modified and adapted so that anomalies can be explained or neglected by the research programme.

Lakatos' methodology gives a very high degree of freedom to the theoretical scientist. She can follow the positive heuristics to develop the research programme and in case or negative empirical responses, she is allowed to modify the protective belt.

Despite some big and evident differences with respect to Kuhn's stance, Lakatos agrees with his American colleague for what concern the impossibility for an experiment to be instantaneously crucial. An empirical test on its own it is not able to falsify a theory. There is always a challenge between two theories and the natural world.

> A series of theories is theoretically progressive [...] if each new theory has some excess empirical content over its predecessor, that is it predicts some novel, hitherto unexpected fact [...]. A theoretically progressive series of theories is also empirically progressive if some of this empirical content is also corroborated, that is if each theory leads us to the actual discovery of some new fact. Finally, let us call a series of theories progressive is it is both theoretically and empirically progressive, and degenerating if it is not. We accept research programme as scientific only if they are at least theoretically progressive; if they are not we reject them as pseudoscientific [Lakatos, 1976]

The previous citation introduces also the demarcation criterion held by Lakatos. The degenerating research programmes should be categorize as pseudoscientific. This categorization is non-instantaneous. Lakatos proposes that for what concern theoretical novelties, mathematical sophistication should be taken into account. On the other hand, empirical progress should be evaluated by means of standard empirical tools.

Lakatos, with his methodology of scientific research programmes, explains why the naive falsificationism doesn't work and, at the same time, provide a new version that can challenge in a more reliable way the problems faced by Popper's methodology. Let's provide an example that clarifies how MSRP faces both the Duhem-Quine thesis and the historical phenomena collected in the history of science. The example (taken from [Motterlini, 2000]) deals again with the discovery of Neptune. Since this historical example has been already introduced, it is possible to skip directly to the logical implications and consequences outlined by Lakatos.

Let's call T Newton's gravitational law, A a set of assumptions, A' a new set of assumptions including Neptune, O the Uranus' orbit without the influence of Neptune and O' the Uranus orbit with the influence of Neptune. Physicians have two options:

$$T \wedge A \rightarrow O, \neg O \vdash \neg T$$

$$T \wedge A \rightarrow O, \neg O \vdash \neg A$$

The two options raise from the Duhem-Quine thesis. When a system of hypothesis is tested (and it is impossible to test one single hypothesis), in case of a negative response deductive logic doesn't provide any advice on which hypothesis is false. Physicians at that time didn't accept the first strategy and embraced the second one. By modifying the set of assumptions, including the new planet Neptune, they were able to explain the orbit of Uranus.

$$T \wedge A' \rightarrow O', O' \vdash T \wedge A'$$

In the end, that was the right strategy as Neptune was eventually discovered.

But let's consider another example in which theory has been abandoned. Mercury's perihelion was one of the problem that Newton's gravitational system wasn't able to solve. This problem was recognized in the early stages of the Newtonian physics, nevertheless it didn't prevent the theory to be developed. Einstein's relativity was able to explain it in some *non-intentional* way. Let' call N the Newtonian gravity, E the general relativity, A a set of assumptions, A' a set of complicated and *ad hoc* assumptions with respect to N, M Mercury's perihelion and M' the perihelion predicted by N.

As usual scientists have two options:

$$N \wedge A \rightarrow M', \neg M' \vdash \neg N$$

$$N \wedge A \rightarrow M', \neg M' \vdash \neg A$$

Let's suppose that they follow the previously right strategy and they modify the set of assumptions:

$$N \wedge A' \rightarrow M, M \vdash N \wedge A'$$

Even if A' is completely *ad hoc* with respect to N, N is not refuted. Notice that also in the Uranus' example, before the discovery of Neptune, the new set of assumptions was *ad hoc* with respect to Newtonian system. In this case, the anomaly is quit neglected. The falsification of N is impossible as no other theory is available so far. Then, a new theory E is developed. E is developed with other purposes than to explain M. Nevertheless, when E is combined A, it predicts *non intentionally* M:

$$E \wedge A \rightarrow M, M \vdash E \wedge A$$

This last example shows one of the most important characteristics of Lakatos' MSRP. Rationality is not instantaneous. The anomaly of Mercury's perihelion was considered just an annoying phenomenon before Einstein. Once the general relativity has been developed, that anomaly becomes one of the most important counter-fact of the whole physics' history.

> [Rationality] acts much more slowly than most people are willing to believe and still fallibly. Minerva's noctule only takes flight over sunset. [Lakatos, 1976]

Another important aspect of Lakatos' project concerns new facts. According to the examples described before, in some cases modifications of assumptions lead to the discovery of new facts, while in other cases assumptions are introduced just to

explain anomalies. Those research programmes that are able to discover new facts are called progressive, while the ones that do not have this peculiarity are called regressive. It is interesting to underline that Lakatos, challenging Feyerabend, have never clarify his normative position toward progressive and regressive programmes. Sometimes he argues that a plurality of research traditions is beneficial for the scientific progress as a whole. In other circumstances, he upholds that financial and human resources should be directed just toward progressive programmes, determining a natural death of regressive traditions. This apparent contradiction has got its root in Lakatos' background; on one side we have the liberal Popper's falsificationism, and on the other we have the authoritarian historicism.

Leaving aside those political and social issues, the problem about the definition of new facts should be introduced. History of science has been characterized by two different approaches toward new facts. The fist one, called temporal approach, states that a theory to be progressive have to discovery facts that were completely forbidden before; in that sense the results of good predictions are phenomenon which scientists are not aware of. The second approach is the Bayesian school. According to this position, the temporal characteristics of a phenomenon should not be considered when its novelty is discussed. Lakatos notices that the temporal approach do not explain rationally some historical events in the history of science. In order to understand whether a theory is able to predict new facts in a genuine fashion, the way in which that theory has been developed should be taken into account. Following this reasoning a fact is new with respect to an hypothesis if it has not been used to build the hypothesis itself.

3.3.10 MSRP and Economics

Economics welcomes Lakatosian methodology in 1970s. Economists felt methodology scientific research programmes' suitable for their discipline essentially for two reasons; first of all, Lakatos give a remarkable degree of freedom to the theoretical economist with respect to empirical controls. Secondly, because the demarcation principle held by Lakatos seems to give economics the credentials that are typical to hard sciences. In the following the most famous and rigorous Lakatosian application to economics is described. Later on, the drawbacks of such approach are discussed.

Weintraub in [Weintraub, 1993] tries to describe and justify the evolution of the neo-Walrasian economics using Lakatos' criteria. According to Weintraub, this research programme can be analysed as follows:

1. **Hard Core**.
    - there exist economic agents;
    - agents have preferences over outcomes;
    - agents independently optimize subject to constraint;
    - choices are made in interrelated markets;
    - agent have full relevant knowledge;
    - observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium states.
2. **Positive Heuristics**.

- go forth and construct theories in which economic agents optimize;
- construct theories that make predictions about changes in equilibrium states.

3. **Negative Heuristics**.

- not construct theories in which irrational behaviour plays any roles;
- not construct theories in which equilibrium has no meaning;
- not to test the hard core propositions.

The justification of the neo-Walrasian programme follows its description. Weintraub holds that the series of theories included in that research tradition should be tested according to two different criteria, accordingly to Lakatos. "Theories in the protective belt of the program, theories developed out of the hard core by the heuristics, are appraised by the method that is appropriate for any empirical science". On the other hand, "the work in the hard core of the program is controlled consistently with the valuation rules applied to the mathematical theories" [Weintraub, 1993]. To sum up, theories of the hard core, the ones that are cannot be falsified by any empirical observations for a methodological decision, have to be tested by measuring the impulse given to mathematics. Theories included in the protective belt have to be controlled by empirical means.

This project has got good result in describing the progress of the neo-Walrasian economics. Nevertheless, it seems to miss a clear argument in favour of the progressiveness of that programme. "[...] the fact is that there is a profound debate about the success of the theories in the "protective belt" of neo-Walrasian economics. It is easy to pile up Nobel laureates on either side of the question whether economics has met the test of empirical progress. [...] Moreover, the parties to this dispute do not share a common criterion for predictive power, or empirical confirmation, because there is none" [Rosenberg, 1986]. The problem of the Lakatosian methodology, not just in economics, but in general in sciences, is that it refers to vague terms; "[...] the notion of "empirical content" of "novel fact", of "corroboration" are among the most vexing in the philosophy of science" [Rosenberg, 1986]. As Rosemberg points out, if a clear definition of those term were available, there would be no need of liberal methodology such the one proposed by Lakatos. It seems that Lakatosian project can offer fruitful description of the history of economics, especially underlining the relationship between metaphysics and choice of political economics ([Motterlini, 2000]). On the contrary, in the context of appraisal, MSRP offers little result because it is based upon terms that are complex to define in a commonly accepted way.

It is worth to investigate the link between Lakatos and Friedman's instrumentalism. As described earlier, economists that apply the F-twist (Friedman's methodology) are tempted to develop rules or instruments that can be used to predict and described human's behaviour also outside the scope of economics. This approach is called imperialism. The temptation of saying that the neoclassical economics is progressive because, due to imperialists' effort, it predicts and explains novel facts is appealing. Nevertheless, it is wrong. There are two reasons for that; first of all, economic imperialism does not explain anything. The object of imperialism is not the explanation of phenomena, but the developing of rules to predict empirical facts.

Secondly this position is not consistent with Lakatos' stance. Lakatos introduces the conjecture inductive principle as metaphysical principle to be sure that the progress of the science tends to plausibleness. In an instrumentalist context, plausibleness does not and cannot play any roles; it is simply useless.

3.4 Economic Models

Economic theorists spend most of their time in developing and analysing models. The aim of this section is both to describe the reason why economists tend to prefer them and to distinguish model from other entities such as theories. In economic textbooks, there are plenty of references to theories: consumer theory, firm theory and etc. It will be shown that those theoretical results are indeed models.

The central question of the section is: "how model can be useful to understand some characteristics of the real world?". Scholars involved in methodological issues about neoclassical economics tried to provide different answers to that question. However, their results have to rely on some non-logical (outside logics' scope) arguments.

3.4.1 The Hypothetico-Deductive Method

The chapter "Three Philosophical Pillars" has shown why the relationship between system of laws and natural science' phenomena can be expressed in a deductive form. Even if those laws are characterized by a *ceteris paribus* clause, the nature of those sciences allows theorists to formulate a closing deductive clause. This clause is a proposition provided by the particular natural science that, combined with the starting law, makes it valid. Validity has to be interpret in the context of the deductive logic, that is if the the hypothesis of the law is true and if the closing deductive clause is true, then the consequences of the law are true.

Let's see, by means of an example (taken from [Barrotta and Raffaelli, 1998]), if the relationship between economic phenomena and economics is deductive. The example deals with the revealed preference theory introduced by Samuelson. The theory assumes consumers' coherence by postulating the weak preference axiom: if it's revealed that x is preferred to y, it cannot be the case that y is preferred to x. It seems that Samuelson is able to get rid of all psychological assumptions about utility, since his theory is based on observable data. But is this axiom verifiable? Logically, its verification would require an infinite number of individual choices between two goods. Thus, from a logical point of view the weak preference axiom is not verifiable. However, it may seem too ungenerous to ask for the verifiability of an axiom. A more feasible request would investigate the verifiability of the entire revealed preference theory. Notice that, in a verification context, the axiom should be qualified with a *ceteris paribus* clause, otherwise it would be false, as it ise very common that consumers change preferences. Hence the testable axiom is "*Ceteris paribus* if it is revealed that x is preferred to y, it cannot be the case that y is preferred to x". The clause contains information about the choice process that runs in consumers' mind. If their preferences are steady, the verification of the axiom can take place. But the problem is that the experimenter has to assume that preferences are constant if she wants to test the axiom. *Ceteris paribus* clause contains information about the psychological mechanisms that induce the consumer to change preferences. A

well-confirmed psychological theory that described those mechanisms is not available hence the clause is inevitably vague, that is it cannot be substitute by a genuine closing deductive clause containing a law that, combined with the weak preference axiom, makes the theory verifiable. In practical terms, this reasoning implies that one can assume the validity of the axiom's premises (if it is revealed that x is preferred to y), and the non validity of the consequence (it cannot be the case that y is preferred to x), because any deviation from the axiom can be targeted to the *ceteris paribus* clause.

3.4.2 Meaning of Models

The lack of a deductive link between economic phenomena and economic laws introduces a fundamental distinction: the difference between theories and models. Even though those two knowledge entities are sometimes used in an interchangeable way, they have two different objectives.

Scientists sometimes observe some well known phenomenon and they try to find an explanation for it. By combining some law that already exists they can state "the given phenomenon occurs because ...". Following this reason, theories are involved with the question "why?". Applications of natural sciences are a typical example of theories. On the other hand, models have a different role. They are suitable to answer the question "under which conditions?" thus, they can be thought, as a starting point, as conceptual exploration. Conceptual exploration means that the theorist builds a model world provided with an engine (typical some mathematical and logical instrument) and the consequences of this model world can shed some light on the real world.

The fundamental problem is that economic models use counterfactual assumptions, hence it is not straightforward how those models help economists understand the economic system. Counterfactual are those assumptions that are evidently not true in the real world. Neoclassical economics makes an extensive use of counterfactual assumptions.

In the following are presented two attempts to understand the link between counterfactual economic models and real world. It is worth to remind that the central statement of this section is:

> [The] starting point is that model-building in economics has serious intent only if it is ultimately directed towards telling us something about the real world. [Sugden, 2000]

In conclusion of this subsection, some words should be spent about the critiques advanced against neoclassical economics. From a normative point of view, it can be maintained that every model have to be built on real assumptions. From this viewpoint, anytime a counterfactual hypothesis is removed from a neoclassical model, this operation should be regarded as a good procedure. On the other hand it is not justifiable the critique according to which neoclassical models are not at all explicative because they rely on counterfactual assumptions. Those critiques miss completely the point. It seems that the fundamental problem of this economic school is not the extensive use of counterfactuals, but the fact that, even though all the results about the link between real world and models are vague, the majority of

the neoclassical supporters strongly maintains that economics is an hard science. It is evident that this problem boils down to the definition of economics, but this is not a news at all. John Stuart Mill expressed the clear relation between economics' definition and its methodology.

3.4.3 Approximations and Caricatures

Allan Gibbard and Hal Varian in their influential and inspiring article try to disentangle the models' issue. They explicitly state that the objective of the paper is to investigate the meaning of the model in an economic environment. Thus they pose the relevant question: "In what ways can a model help in understanding a situation in the world when its assumption, as applied to that situation, are false?" [Gibbard and Varian, 1978].

Before dig into that question, the two scholars define a model as a story with a specified mathematical and axiomatic structure. Those models, according to the authors are quite conceptual, as they involved general entities. The interesting passage happens when models are applied to a situation, that is when those entities assume the shape of real entities (for instance, the consumer theory is the general model, and its application to the tobacco consumers it's the applied model).

Given the previous models' taxonomy, it is maintained that models help economist understand the world through two distinct paths, the first one is approximation, the second one is caricature.

> [...] we shall distinguish between models that are "approximation" and models that are "caricatures". The former are models that aim to describe reality, albeit in an approximative way. Caricatures, on the other hand, seek to "give an impression" of some aspect 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]

It is worth to describe how approximation and caricatures can be linked to real world according to Gibbard and Varian:

- **Approximation.** The theorist hypothesizes that model's assumptions are quite close to the real world. In formal terms she set a parameter δ that measures the approximation of the assumptions. The general model, according to mathematics and logics, ensures that if the assumption were true in the real world, then the conclusion would be also true. Since the assumptions are sufficiently close to the truth, then also the conclusion should be sufficiently close, to a degree of ϵ to the real world. For sure the critical passage is the hypothesis according to which sufficiently close assumptions give sufficiently close conclusions. Gibbard and Varian are aware of that issue and thus they write: "For this no argument within the model can be given; it is rather a hypothesis, for and against which evidence might be given" [Gibbard and Varian, 1978]. Somehow, it is a matter of taste. If theorist feels that assumptions are sufficiently close, she can rely on model's results. A little dose of subjectivism is reintroduced.

- **Caricatures.** Economists sometimes tend to "exaggerate or isolate some feature of reality". They do that because reality may be so difficult even to

approximate. It is much simpler to focus on just one of its aspects, or to make one peculiarity the most prevailing one. In such theoretical circumstances, robustness, according to authors, plays a key role. "When a theorist applies a model that caricatures a situation, one hypothesis he may entertain is this: the conclusion of the applied model roughly depict some feature of the situation, and that is because (1) the assumptions of the model caricature features of the situation, and (2) the conclusions are robust under changes in caricature. A principal way of testing this hypothesis may be to try out models with disparate caricatures of the same complex aspect of reality" [Gibbard and Varian, 1978]. In this attempt to explain the link between real world and model, it seems that robustness may induce theorists to feel comfortable with that particular problem, but there is no clear explanation of how this robustness constitutes a link with the real world. In particular Sugden holds that "Gibbard and Varian have disappointingly little to say about how a casual model explains an aspect of the real world [...]" [Sugden, 2000]. Sugden himself, proposes a new way to explore models' world.

3.4.4 Credibility or the Induction Revival

Sugden proposes a different method for evaluating the usefulness of models in grasping aspects of the real world, despite their counterfactual assumptions. He combines two features that together might make clear the link between economics and models. On one side, Sugden reintroduces induction, on the other side, credibility would increase induction reliability. Let's see in detail how Sugden's proposal works.

Inductive reasoning is presented with a standard indcution principle: a general set S of phenomena is given, and it consists of two subset, c and r. The former is the subset of counterfactual assumptions or phenomena that economist can investigate with models; the latter is the subset of real phenomena. By plugging c into a model, and by performing deductive reasoning, some regularity X is found. Induction may allow the theorist to infer that X would work also for the remaining subset of real phenomena r. The merge of c and r in the same subset is justified only if c and r are, somehow, similar. The concept of similarity in philosophy is one of the most controversial of the entire subject, and Sugden is aware of that: "Many of the philosophical puzzles surrounding induction stem from the difficulty of justifying any criterion of similarity. [...] what is important is this: if we are to make inductive inferences from the world of a model to the real world, we must recognize some significant similarity between those two worlds" [Sugden, 2000]. Since Sugden is not willing to face the similarity problem, he introduces another concept, credibility. If the world built in model is credible, than theorists may have more confidence on the induction that allows them to link the model world with the real world.

> I want to suggest that we can have more confidence in [inductive inferences], the greater the extent to which we can understand the relevant model as a description of how the world could be [Sugden, 2000]

Sugden is not able to say what constitutes credibility in economic modelling, but he suggests that this principle has got some interesting similarities with the credibility of novels. The author states that a novel's success depends on the writer's

capacity to build a credible screenplay in which the reader can recognize some typical situation of the real world. If the novel depicts some very implausible scenario, it is likely that it can't achieve success.

Sugden proposal is interesting, as it upholds a vision of economic modelling close to an artistic work. Criticize this stance by saying that induction is unjustified miss the crucial point. "Everyone makes inductive inferences, but no one has really succeeded in justifying them. Thus, it should not be surprising if economists leave gaps in their explicit reasoning at those places where inductive inferences are required, and rely on their readers using their own intuition to cross those gaps" [Sugden, 2000]. It seems that one problem that Sugden's proposal suffers lies in the definition of the set S. It consists of two different subsets, the one of the counterfactual phenomena of models, and the one of the real world phenomena. It may be maintained that at some point in time similarity or credibility allows economists to merge those two subsets together. But, while the set of counterfactual hypothesis is petrified in economic models, the other subset changes continuously. No argument is advanced in favour of the stability of the credibility.

Chapter 4

The Method of Behavioral Economics

This chapter focuses on behavioral economics and its current status in the economic landscape. Its present condition is not at all non-ambiguous; many scholars notice that behavioral economics might be conceived as a failed revolution and they argue that it has been completely absorbed in the neoclassical program. The root of all issues concerning behavioral economics is its unsolved tension between descriptive purposes and normative stances. Indeed, a considerable share of behavioral researchers take for granted normative prescriptions that they themselves hold to be false for descriptive analysis.

Behavioral economics is the attempt to bring psychology into the economic analysis in order to improve the realism of assumptions. It may seem puzzling how a social science based on atomic agents could survive without references to agents' psychology, but this was the *leitmotive* in the first half of XX century, when neoclassical economics was based on a firm ground. Nevertheless, that was not always the case in economic literature; fundamental economists such as Mill, Jevons and Edgeworth didn't eschew psychological arguments in their economic findings and they made numerous references to conscious, yet unobservable, mental states like pleasure and pain.

The role of the first section of this chapter is then to describe this historical evolution. A lot of effort will be spent to describe Pareto's concept of economics as it was the first scholar that tried to bring psychology out of economics. The second section concerns with the main findings in behavioral economics. It is by no means a complete or exhaustive survey of behavioral researches, on the contrary the section mainly focuses on Tverky and Kahneman's discoveries. Later, the descriptive-normative tension is introduced. That tension is not just a methodological issue as it has far-reaching consequences in politics. Indeed, some political advices, grounded on behavioral findings, have been proposed in order to make agents' behavior consistent with the rationality norms that are commonly accept in economics. As usual, the importance of methodological and epistemological issues stand in their crucial practical consequences.

4.1 Historical Overview

4.1.1 Psychology and Early Neoclassical Economics

Classical and early neoclassical economics contain plenty of references to psychology. In these frameworks, individuals are complex and they behave differently with respect to the social situation they face. The complexity attributed to agents is consistent with Mill's methodology described in chapter 2, that at that time was broadly accepted and applied. *Analysis and Synthesis* is indeed a method to divide the social world in different sections, investigate each section with useful tools in order to extrapolate results and, finally, combine the results of all the sections to describe the complex social world. It has been shown that Mill maintains that economics should only be concerned with the acquisition of goods for the self interest in contrast with labour. Early neoclassical economics has got a similar viewpoint about the human motivation in the economic world. At that time utility was introduced and it was conceived in terms of hedonic psychology, that is terms of unobservable conscious experiences like pleasure and pain. In this new framework, agents seek to maximize and minimize pleasure and pain, that, it is useful to remind, are unobservable mental states. It might seem that unobservable entities are too fragile (from a epistemic viewpoint) to base a science on. Nevertheless, at that time it was completely legitimate to make references to something that cannot be observed. This was due, first of all, to a strong commitment toward introspection. As noticed in chapter 2, Mill's method is completely based on introspection. It seems that in those years, economists agreed upon the psychological pillars of economics, and this agreement relied on introspections. But not solely on introspection. The psychological landscape of the second half of XIX century is shaped by a program that "proposed the revolutionary thesis that mental phenomena could be measured by finding quantitative relationships between materials stimuli and mental sensations. [Bruni and Sugden, 2007]". *Psychophysics* was at that time the dominant research program in psychology. Fechner and Weber, two psychologists belonging to psychophysics program, proposed a law that links stimuli and sensation and that states that the change in magnitude of a given stimulus that produces a unit change of sensation increase with the overall stimulus:

$$S = K \log \frac{R}{R_0}$$

In the same years, early neoclassical economics, found a similar relationship between utility and consumption. Its name is **law of diminishing return** and, as all economists know, states that as consumption of a good increases, the increment in utility due to the increment in consumption decreases. It should be pointed out that early neoclassical economics arrived at that law by using introspection.

Following [Bruni and Sugden, 2007], it is possible to outline a further example of the beneficial role that psychology had in economics. Edgeworth's *law of accomodation*, according to which the utility function of consumption is affected by the consumption in the previous period, might be considered a preliminary version of *reference-dependent* theory developed by Tversky and Kahneman in the behavioral economics program.

Psychology and economics were strongly linked at their outset, and economists didn't feel embarrassed to base their findings of psychological entities. But the

cultural landscape changed and in 1910s behaviourism arose. Behaviourism, logical neo-positivism and falsificationism challenged the role of introspection and the extensive reliance on unobservable mental states. Indeed, behaviourism proselytes upheld both that any scientific method should be public, so rejecting the role of introspection, and that a descriptive science of behavior should just focus on behavior, avoiding descriptions based on unobservable mental states. Furthermore in economics, starting from 1920s, two revolutions about the utility concept took place. The first one is ordinalism, an attempt to replace the cardinal utility function of the firsts neoclassical economists with an ordinal utility functions. This approach to utility shift the focus from pleasure-pain, to preferences. The second revolution is the operationalism; according to this approach, scientific entities description is nothing but the set of operations that have to be performed in order to observe them. Since utility is not observable, this concept is removed from economics. The economic consequences of operationalism, is Samuelson's revealed preference theory, that is centered in the concept of choice. "In Hicks and Allen's (ordinalists) view the concept of preferences is primitive, whereas in Samuelson choices are the primitives. [Bruni and Guala, 2001].

But this is just part of the story. Pareto, well before the behaviourism revolution, attempted to severe all the links connecting economics and psychology. The next section is dedicated to the Paretian turn.

4.1.2 The Paretian Turn

It is generally held that Pareto was the first economist to separate economics from psychology, arguing that any economic research should be focused just on "naked facts". This might be considered a straightforward sentence, but in the reality it hides some fundamental methodological considerations, that, if not taken seriously into account, would lead to misunderstanding and misinterpretation. The first aim of this section is to show why Pareto severe connections between economics and psychology. Then the section turns to the problem of Paretian economic actions.

Pareto declares rather clearly that his target is to base economic analysis on "naked facts" or experience:

> [The theory] rests on no more than a fact of experience, that is, on the determination of the quantities of goods which constitute combinations between which the individual is indifferent. The theory of economic science thus acquires the rigor of rational mechanics; it deduces its results from experience, without bringing in any metaphysical entity [Pareto, 1906b].

Pareto doesn't deny to admit that psychology has got its own foundations, but in order to provide economics with a firmer ground it is better to rely on experience. It seems that Pareto is more confident about "naked facts" than about psychological discoveries. [Bruni, 2010] maintains the same viewpoint:

> For Pareto, basing economics and social sciences on psychology or on the fact of choice is mainly a question of the epistemological degree of confidence: the analysis of the curves of indifference is a much more secure basis, yet psychological analysis is not "non-scientific": it is only less secure and always requires the

verification of objective experience. [Bruni, 2010]

Once Pareto has declared his intention to place economics on one side and psychology on the other, he has to provide economics with the a new scope. Once psychology has been thrown out of the window, the maximization of pleasure does not have a role in economics. At this point Pareto introduces the new scope of economics:

> We will study the many logical, repeated actions which men perform to procure the things which satisfy their tastes [...]. We are concerned only with certain relations between objective facts and subjective facts, principally the tastes of man. Moreover we will simplify the problem still more by assuming that the subjective fact conforms perfectly to the objective fact. This can be done because we will consider only repeated actions to be a basis for claiming that there is a logical connection uniting such actions [Pareto, 1906b].

Thus the new scope of economics are the logical actions. In order to understand which is the meaning of logical actions in the Paretian view, a little digression will facilitate this challenge. Rather than an economist in the narrow sense, Pareto is a sociologist. His sociology deals with human actions that in general can be both logical and non-logical. The possibility to attach the logical attribute to an action definitely depend on the motivation that guide the individual in performing that given action. [Bruni, 2010] provides a taxonomy for components of a real actions:

- The logical component: based on a pure instrumental reasoning, where the means are adequate to the end, subjectively and objectively;
- The non-logical component, where the instrumentality feature is missed.

Economics deals, according to Pareto, just with logical, and thus instrumental actions that are also self-centred. This is clearly obtained from the last Pareto quotation where logical actions have the objective of satisfying agent's tastes. The distinction between logical and non-logical actions is consistent whit his methodology. Once again, the methodology applied by Pareto is the *Analysis and Synthesis*. It should be noticed that, according to this epistemological stance, Pareto reduces dramatically the set of phenomena that can be explained by economics. This problem rises some important questions about the applicability of economics and, quite surprisingly, it has been completely neglected by economists that came later Pareto.

The next section deals with some problems that arise from Pareto's economic definition, namely that economics is just concerned with naked facts that regards logical-instrumental actions repeated many times. At this stage, it is worth to introduce the *discovery preference hypothesis*, an hypothesis that some neoclassical economists use as a defence against behavioral economics. Behavioral economics states that agents systematically depart from the perfect rationality that neoclassical program attributes them. The idea behind discovered preference hypothesis is that, if agents have sufficient time to lean, through experience, they will behave in accordance to perfect rationality, and thus to their underlying preferences. There is of course a strong assonance with Pareto "repeated actions" that make "subjective fact conforms perfectly with objective fact". Discovered preferences hypothesis

might be used as a working hypothesis to run experiments that involve repeated transaction. Nevertheless it has to face two critiques:

- Many phenomena on which economics is called to provide an explanation are put outside the scope of economics as soon as the repetition feature is accepted. It might be the case that work selection, house acquisition, retirement plan selection are decisions taken just once in a lifetime. In those circumstances, agents do not have the time to learn and discover their underlying preferences. This problem fits well also in the Paretian framework, but is should be remarked that Pareto has in mind a narrower set of economic phenomena than the present economics;

- As the name of this hypothesis suggests, preferences have to be discovered. It implies that agents are endorsed with coherent preferences and market participation allow them to discover their preference set. But if preferences are constructed rather that discovered, their context-dependent feature does not ensure, and in general does not imply, the the final preference set is made up of coherent choices, namely preferences that obey to completeness and transitivity. As usual transitivity and completeness, and in general preference coherence, assume an axiomatic shape.

It has been shown that the repetition feature attached to logical actions is the root of some problems that can be attributed both to Pareto and to the discovered preferences hypothesis. The following section describes issues that are quite critical to the economic concept advocated by Pareto.

4.1.3 Non-Psychological Social Science's Troubles

The previous section provides Pareto's idea about economics. Economics is then a science concerned with self-centred, logical-instrumental repeated actions. Furthermore any economic analysis draw on "naked facts" and experiences, without any references to "metaphysical entities". This non-psychological economics can be criticized following two paths. The issues that soon will be described, have been pointed out as soon as Pareto conceptualize his idea about economics. Furthermore Pareto himself worked on one of the two problems. What is interesting here is that Pareto's followers seem to have partially misunderstood both the problems. But now it is worth to describe in detail the two issues, namely the motivation problem and the integrability problem.

- **Motivation Problem.** Maffeo Pantaleoni was the first that drew attention of this problem. The motivation issue draw on the instrumentality feature that characterizes economic actions in Pareto. At the same time, Pareto holds that economics must rely solely on experience, "naked facts". How it is possible to determine whether an action fulfils the instrumentality requirement if a psychological theory that shed lights on agent's motivation is outside the scope of economics? It seems that it is impossible to "identify the situations to which the theory applies, prior to observing the behavior that it is intended to predict [Bruni and Sugden, 2007]". No doubt, this is not an healthy methodological position. Notice that this problem does not arise as soon as economics is provided with some psychological background. For instance, hedonist economists

do not have such a problem because they characterize economic actions as the ones that maximize pleasure, and by introspection they might know which actions are inside and which outside economics.

- **Integrability Problem.** Indifference curves are a cornerstone concept in neoclassical economics. They present some important features, but, above all, they depict the consumers' consistency, that in general it is expressed by two properties, namely completeness and transitivity. The integrability problem deals with consumer's consistency: "The correct translation of the integrability problem was in terms of the consistency of consumer preferences. [Stigler, 1950]". If the problem admits a solution, then agents' behavior is consistent, otherwise the set of preferences does not present the characteristics required by the neoclassical economics.
  One possible formulation of the problem is the following: since economists can only observe, in a geometrical representation where goods lie on axis, points that represent purchased bundle at given budgetary data, is it possible to integrate those points in order to obtain a continuous and possibly well-shaped indifference function?
  It should be noticed that the integrability problem might be approached in two different ways. Following the former, the integrability problem is just a technical, or mathematical puzzle. According to the latter, it assumes a methodological shade. While Samuelson took on the integrability issue by walking down the first approach, Pareto had in mind the methodological-shaped path. Now, let's see why integrability is actually a problem, and then, once problem statement is clear, it will be shown how the two approaches shape the solution. Let's start from the two-dimensions case, in which, of course, there are only good A and good B. In such a case, the economist faces the following mathematical problem:
  $$\frac{dx_2}{dx_1} = -B(x_1, x_2)$$
  It is always possible to find a unique family of indifference curves starting from that differential equation. Furthermore, the problem admits a solution even when the consumption path is taken into account. The consumption path is nothing but the real order followed by the consumer when it moves from a point U to a point U^1 in the plane. If the consumer goes from U to U^1 and the comes back to U following a different consumption path, a cycle is obtained. By relying just on experience, nothing ensures economists that utility (or ophemility, in Pareto's terms) is conserved along the cycle, thus it is possible to find strange results such as U is preferred to U, that of course violate the transitivity property. In thermodynamics there's a similar issue. The evaluation of heat exchange of a reversible transformation, δQ_{rev}, is not an exact differential, but an integrator factor can be found, namely $1/T$. Thus, $\frac{\delta Q_{rev}}{T}$ is an exact differential and dS ca be computed. A similar procedure can be performed in economics, namely find an integrator factor that "closes" the cycle. However that operation is possible just in the two-dimensions case. When the geometrical representation is generalized, allowing to treat n-dimensions cases, no mathematical procedure is available to address the puzzle. In particular, in three-dimensions, where the discussion rotates around indifference surfaces and budget plane, if the economist rest just on naked facts and mathematics,

it is not possible to integrate points and find an indifference surface.
Notice that Pareto's methodological concerns have their roots in the "naked fact" proposition. Thus consumption orders become important and Pareto is unwilling to provide some axioms that would solve the integrability problem. How the integrability problem was solved in economic literature? References to [Samuelson, 1950] should be pointed out. First of all Samuelson states that Pareto's pain about integrability problem is due to his confusion about consumption order and integration order:

> It must be emphasized that the paths along which I as an economist scientist choose to evaluate the man's preference have absolutely nothing to do with the order in which the human guinea-pig consumes the good. [...]The comparison of A and B (and of intermediate points) is a case of comparative statics. We need not invade the provacy of the consumer's castle to concern ourselves with the minutiae of his domestic arrangements. [...] To repeat, Pareto's primary confusion results from his identifying the paths of integration chosen by economist-observer for statical comparisons with the behind-the-scene programming of pleasures by consumers [Samuelson, 1950].

Samuelson, once stated that the integrability problem does not arise in the two-dimensions case, shift his effort to the three-dimensions problem representation. He notices that his *weak axiom of consumer's behavior* cannot yield the solution to the integrability problem. Thus, he puts beside his weak axiom a strong axiom, that is provided by Houthakker. The strong axiom states that if A reveals itself to be better than B, and if B reveals itself to be better than C, ecc, then definition of revealed preference is extended and it is said that A can be defined to be revealed to be better than Z, the last in the chain. In such a case it is postulated that Z must never also be revealed to be better than A. But now the distance that separates Pareto and Samuelson is evident. Pareto wants just to rest on experience, and the integrability problem in such a case cannot be solved. Samuelson, provides his theory with two axioms whose empirical content is not obvious. Without this axiomatic structure, the integrability problem is a crucial one. Samuelson himself declares that:

> [...] every cycle must be closed, and integrability is assured by our strong axiom [Samuelson, 1950].

The integrability problem, as intended by Pareto, is definitely related to the problem of psychology in economics. Samuelson's move is legitimate, but the axiomatic structure of his theory somehow hides the integrability problem under the carpet and, probably, misunderstands the real meaning of Pareto's issue. "Pareto was aware that there is a deep problem in his project of expunging the metaphysical from economics, namely how to justify the assumption that indifference curves exist, when all we have to go on are the facts of experience" [Bruni, 2010].

This section has described two problems that economics has faced in the seek of independence from psychology. The first problem was forgotten, or completely changed. Nowadays economists do not ask themselves how to understand the motivation that belong to the economic set, but they apply a single motivation, the maximization of a function, to all the situations. This approach has been described earlier and it has been called imperialism. Samuelson solved the integrability problem by grounding his theory on axioms. It can be regarded as a legitimate move, but this approach completely miss the problematic related to the integrability problem, as Pareto conceived it. The next section deals with psychology revival in economics, and the methods that behavioral economics applies in its researches.

4.1.4 Psychology Revival

It is always difficult to indicate the precise time instant in which a research program begins. In this work, it is assumed that behavioral economics was born around the mid 1970s, when Amos Tversky and Daniel Kahneman published a series of articles which demonstrated that agents systematically depart from neoclassical rationality and that those departures were widespread and predictable. At that time, there was the conviction that firm strictures of agents' rationality were not suitable to describe economic behavior. Experiments run by the two psychologists showed that consumers' decision-making process is different from the standard constrained optimization. In order to circumvent those biases, psychology had to be brought back into the economic analysis. Conscious mental states that, during the firsts years of the XX century were systematically eschewed, now assume a prominent role in economics. Nevertheless, behavioral economists learnt the lesson about the little reliability of introspection. Indeed, they applied, and they are still using, different methods to discovery and to understand what process goes on in agents' mind when they face an economic choice. However, before the discussion about the methods of behavioral economics, it is worth to describe a work that somehow anticipated the behavioral breakthrough.

Herbert A. Simon noticed that the kind of rationality that agents should have to be in line with neoclassical strictures requires both a computation capacity and an information availability that are impossible to find among economic agents. Once he has pointed out the complexity of the problems that agents should solve according to neoclassical economics, Simon states "that there is a complete lack of evidence that, in actual human choice situations of any complexity, these computations can be, or are in fact, performed" [Simon, 1955]. He then proposed an agent with bounded rationality by introducing some computational simplification in the decision-making process.

The objective of behavioral economics, as it is generally held, is to improve the empirical content of economic assumptions in order to increase its descriptive power. By means of analysis and investigation of economic decision-making foundations, the hope is to increase the predictive power of economics. Psychology and cognitive sciences now assume a key role in the new paradigm, as they will provide researches with insights about how agents behave with respect to the economic decision that they have to undertake. Systematically and predictable departures from neoclassical rationality, open the door for conscious mental states there were forbidden in the post-war neoclassical economics. However, one of the most important shift that

characterizes behavioral economics is that introspection is no more regarded as a reliable method. Furthermore, behavioralists try to ground their findings on more secure basis. One peculiar feature of behavioral economics' methodology is its interdisciplinary approach: "Behavioral economists, just like cognitive scientists, draw on evidence of many kinds and are comfortable using differet methods to generate such evidence. In particular, unlike many postwar neoclassical economists, behavioral economists do not consider choice behavior the only kind of admissible evidence" [Angner and Loewenstein, 2007].

In the following, a short lists of the most applied method is presented.

- **Hypothetical Choices.** Hypothetical choice is the first experimental method applied by behavioralists. According to this method, subjects should imagine what their action would be in the case they face a particular decision. Then it is stated that observations collected by researches through this method are reliable. Notice that thanks to hypothetical choices, firsts behavioral economists were able to discoveries decision-making anomalies. Over time the use of hypothetical choice method has been criticized by experimental economists. The artificial environment, the lack of motivation and doubts about external validity were the main arguments advanced against hypothetical choices. Those doubts, by the way, were known also at the time of Kahneman and Tversky's outset: "The reliance on hypothetical choices raises obvious questions regarding the validity of the method and the generalizability of the results. we are keenly aware of these problems. [...] By default, the method of hypothetical choices emerges as the simplest procedure by which a large number of theoretical questions can be investigated" [Kahneman and Tversky, 1979].

- **Experiment with actual outcome.** One of the critiques against method of hypothetical choices is that agents are not motivated and they do not have incentive to act as in a market transaction. The natural consequence of the critique is the development of experiments in which agents, at the end of the procedure can, in fact, gain actual outcomes. In some cases it has been observed that departures from neoclassical rationality vanish if agents are properly motivated and if they are given a sufficient time to learn how to play in the experiment. In other circumstances, standard rationality fails to predict experiment results even after the introduction of real outcomes. However, nowadays "experiments involving real outcomes [...] are the gold standard for research in behavioral economics" [Angner and Loewenstein, 2007].

- **Field Research.** External validity and generalizability are of course important concerns in an empirical science. The laboratory's artificial environment raises the question whether results obtained in laboratory can be generalized also in the real economic environment. In order to address this problem some bahavioral economists have based their researches on field data, thus data are obtained outside laboratory in a "genuine" fashion. If field research rules out questions about external validity, it generates doubt about internal validity. Indeed it is not always simple, or possible, to interpret a correlation as a causation. For instance it is known that rain level in Scotland is positively correlated to price level in Great Britain. Nevertheless, no one thinks that this correlation can by interpret as a causal relationship that would take the

form of the following sentence: "Price level changes following the trend of rain level". The causation cannot be inferred as the two processes might be completely separated and independent (as, in reality, they are). A possible solution to the internal validity problem is the natural experiment, in which behavioralists study the effects of an exogenous shock.

- **Process Measure.** The interdisciplinary approach that characterizes behavioral economics brings this program to rely on methods quite far from economics. This is the case of process measure methods, that are measuring procedures applied in neuroscience. Typical process measure used is the functional magnetic resonance imaging that allows researches to investigate which part of the brain is activated when agent is facing an economic decision. A further example of process measure involves mices: "[...] experiment uses mice with a dopamine transporter (DAT) knockdown due to genetic engineering. [...] the knockdown mice produce only about 10 percent as much dopamine transporter as the control mice, but as a result have 70 percent more extracellular dopamine sloshing around between neurons" [Camerer, 2006].

4.2 Behavioral Economics' Results

This section focuses on the main findings of behavioral economics. It is neither an exhaustive nor a complete survey of all the researches that have been carried out in this program; rather it shows how Amos Tversky and Daniel Kahneman's discoveries of 1970s have shaped the path of behavioral economics. Having in mind this aim, the section splits in two subsections, the former refers to heuristics, and the latter concerns with prospect theory. A further caveat is that topics are described in a methodological-neutral fashion, without dwelling to much on different concepts of rationality that researches endorsed. That's why the paragraph of heuristics contains both Tversky and Gigerenzer without underlying the differences between those two scholars. Those differences are the topic of the next section that evaluates the ambiguous present state of behavioral economics.

4.2.1 Heuristics

According to the Expected Utility (EU) framework, economic agents undertake choices in uncertain environment by evaluating a function that is indeed their expected utility. This function is nothing but an expected value in which possible outcomes are weighted by the probability of their occurrence and then they are added together. EU theory assumes that agents have well-formed beliefs about the future, so that they are able to evaluate probabilities in a correct way. The work on heuristics criticizes this approach by stating that:

> [...] people rely on a limited number of heuristics principles which reduce the complex tasks of assessing probabilities, and predicting values to simpler judgemental operations. In general those heuristics are quite useful, but sometimes they lead to severe and systematic errors. [Tversky and Kahneman, 1974]

In that article Tversky and Kahneman presents three heuristics:

- **Representativeness.** When agents are asked to assess the probability that an object or event A belongs to the class B, they frequently disregard important analytical information that yields the correct probability evaluation. Agents, on the contrary, apply the representativeness heuristic, according to which that probability is assessed by taking the degree to which A resembles B into account. The typical example referes to an agent that is asked to guess the occupation of a worker whose a description of personality is given. The agent would rely much more on the description than to other important information. The representativeness heuristic lead to mistakes such as insensitivity to prior probability, insensitivity to sample size, misconception of chance, insensitivity to predictability and misconception of regression.

- **Availability.** In the assessment of the probability of, say, a car accident, agent's evaluation would be affected if a friend of her reported of a car accident. "There are situations in which people asses the frequency of a class or the probability of an event by the ease with which instances or occurrences ca be brought to mind" [Tversky and Kahneman, 1974]. Agents behave in such a way to reduce the complexity of the problem and decrease the number of operations required to come to a solution. Similarly to the case of representativeness, also availability leads to mistakes: biases due to retrievability, biases of imaginability and illusory correlation.

- **Anchoring.** In many situation agents approach the problem with a starting solution, and then they adjust it until they reach a new solution that they feel to be sufficient. It turns out that adjustments are always insufficient, and the final solution is biased toward the starting point. Notice that mistakes arise both when the agent guesses the initial solution and when the agent is provided, by an external source, with the starting point. The standard biases associated to anchoring are: insufficient adjustment, biases in the evaluation of conjunctive and disjunctive events and anchoring in the assessment of subjective probability distributions.

The way Tversky and Kahneman conceptualize heuristics has got similarities with the topic of heuristics in operational research. There, when problem are computationally demanding, that is when they are NP-complete, heuristics might be applied to arrive to a sufficiently good solution. The trade-off is evident: complex problems are solved by using rather simple rules of thumb, but the solution that is obtained is not, in general, optimal. Indeed, in Tversky and Kahneman's program, "[...] heuristics are highly economical and usually effective, but they lead to systematic and predictable errors" [Tversky and Kahneman, 1974].

It might be the case that Gigerenzer's idea of heuristics is much broader than the one that has been just described. Even if the pillar of Gigerenzer's concept is a different kind of rationality that has got consequences also in the normative field, it is possible to grasp the difference with the previous hueristics' concept by recycling the similarity with operational research. In that subject, it is well-known that heuristics in general lead to sub-optimal solutions for NP-complete problems because there are other problems, namely P problems, for which optimal algorithms are available, and their optimality has been proven. But in the economic context, how it is possible to assert that neoclassical rationality leads to optimal solutions,

while heuristics are destined to biases? Maybe people that depart from neoclassical rationality are worse-off than agents whose actions are in compliance with that framework. As it will be described in the next section, Gigerenzer rules out this possibility. Here, the focus is on the less-is-more effect:

> The goal of making judgements more accurately by ignoring information is new. It goes beyond the classical assumption that a heuristic trades off some accuracy for less effort. [...] less-is-more effect: the complex model had all the information the simple heuristic used and more, performed extensive estimations and computations, but nevertheless made more errors. [Gigerenzer and Gaissmaier, 2011]

4.2.2 Prospect Theory

Article about heuristics was concerned with how agent assess probability in uncertain context. On the contrary, prospect theory stands on an higher level, investigating the consumers' behavior, given objective probabilities: "The present discussion is restricted to prospect with so-called objective or standard probabilities" [Kahneman and Tversky, 1979]. Since prospect theory is quite agnostic about probabilities assessment ad it just focuses on the decision-making process, the theory can be considered a critique of the expected utility framework from a descriptive viewpoint.

By running experiments with the hypothetical choice method, the two scholars found a set of systematic deviations with respect to the predictions provided by the expected utility. In order to address those deviations, "prospect theory distinguishes two phases in the choice process: an early phase of editing, and a subsequent phase of evaluation" [Kahneman and Tversky, 1979]. During the editing phase, the agent approaches the prospect by simplifying some of its aspects. Thus, she organizes and reformulates information in such a way that the following operation of evaluation can deal with a simpler problem. Subroutines like coding, combination, segregation, cancellation are very common during the editing phase, and in general, they are the source of many anomalies of preferences. However, it is in the evaluation phase that most of novelties are introduced. It is proposed that, similarly to the expected utility theory, an overall value V is attached to each prospect and that, once V has been computed, the agent chooses the prospect with the highest overall value. Unlike expected utility, V doesn't contain simple objective probabilities and objective outcomes, but it deals with transformation of them. Indeed V is the weighted sum of two scales, a probability weighting function $\pi(p)$, where p is the objective probability, and a value function $v(x)$, where x is the payoff. Given a prospect with an outcome x with probability p and an outcome y with probability q, the overall value is:

$$V(x, y, p, q) = \pi(p)v(x) + \pi(q)v(y)$$

It has to be remarked that the argument of the value function is the lottery payoff. Thus, in prospect theory what matters is the change in the level of wealth measured with respect to some reference point, rather than the overall level of wealth. Kahneman and Tversky support this theoretical decision with an analogy with human perceptual apparatus:

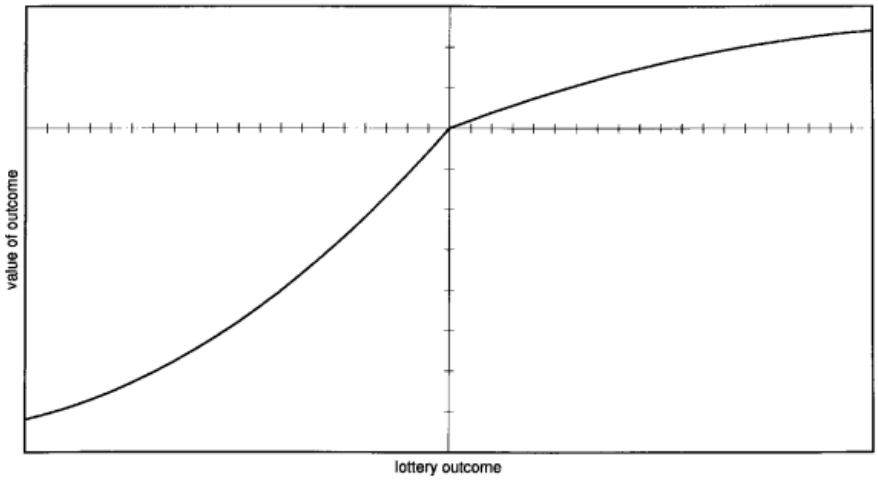

Figure 4.1: Value Function of Prospect Theory

> An essential feature of the present theory is that the carries of value are changes in wealth or welfare, rather than final states. This assumption is compatible with basic principles of perception and judgement. Our perceptual apparatus is attuned to the evaluation of changes or differences rather than to the evaluation of absolute magnitudes. When we respond to attributes such as brightness, loudness, or temperature, the past and present context of experience defines an adaptation level, or reference point, and stimuli are perceived in relation to this reference point. [Kahneman and Tversky, 1979]

The shapes of both the value function and the probability weighting function introduce some important features of the prospect theory. It is therefore worth to describe them.

Let's start with the value function (Figure 4.1). First of all, prospect theory splits the whole region in two subregions, the former referring to gains, the latter related to losses. In the gain region, the value function is concave indicating that when agents face gains, they are risk averse. On the other hand, in the losses region, the value function is convex, pointing out that losses make agents risk seekers. The origin of the axis indicates the reference point, and as deviations from this point increase, diminishing marginal sensitivity is experienced. Prospect theory assumes agents to be loss averse, and this theoretical statement is expressed in mathematical form by a non-differentiable point in the reference point. There, the slope ratio of the losses' value function with gains' value function is approximately 2. This analytical framework is able to explain some empirical regularities that are observed during experiments. First of all, the extensionality principle, according to which preferences do not vary when gambles are modified in an incosequential way, is violated. This violation is the root of the so-called framing effect. Agents' risk preference depends whether the gamble is framed as a gain or as a loss. Furthermore, the combination of loss aversion and reference dependence explains the endowment effect, a phenomenon that indicates that agents are really not unwilling to give up objects that they posses.

The probability weighting function depicts how agents interpret objective probabilities in relation to the gamble that they are playing. It should be pointed

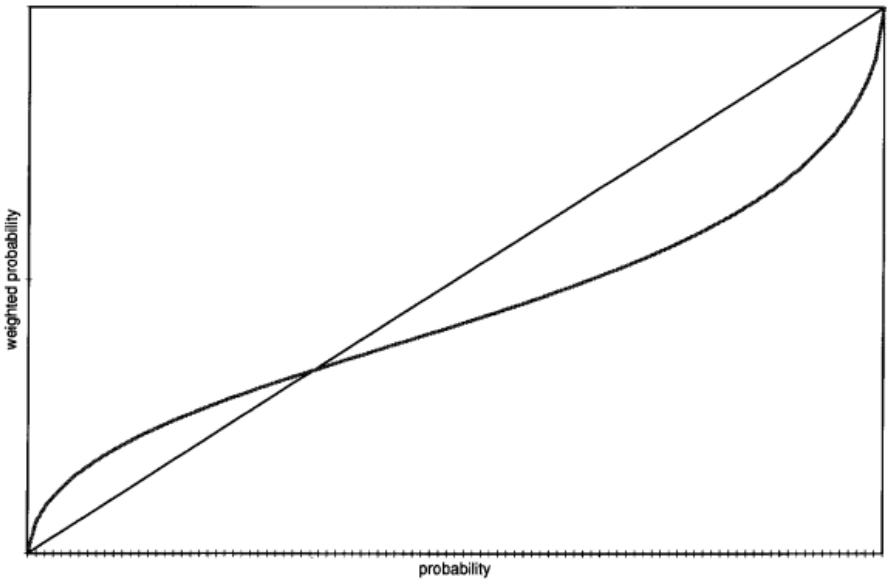

Figure 4.2: Probability Function of Prospect Theory

out that "decision weights are not probabilities: they do not obey the probability axioms, and they should not be interpreted as measures of degree or belief" [Kahneman and Tversky, 1979]. As the value function, the probability weighting function shows diminishing marginal sensitivity. The comparison between the prospect theory's function and the expected utility's one, sheds light on another important property: agents tend to overweight low probabilities and underweight high probability. Those two conflicting behaviors explain both why people are risk seeker when they face a very unlike event, and why, in general, agents opt for complete insurance.

4.2.3 A Theory of Mind

As it has been pointed out in the two previous sections, economic agents systematically depart from neoclassical rationality. Those departures are predictable and theories provided by behavioral economics account for them. The bottom line belief of "mainstream" behavioral economics is that agents are biased during decision-making process. Biases arise from both their reliance on rules of thumb, such as heuristics, and the way they manage information about gambles, like in the case of the prospect theory. The term "bias", is, of course, not neutral as it implies that there is another behavior that should be taken as standard or right.

The dichotomy between right decisions and possibly biased decisions draw on an architecture of cognition that consists of two systems. Those systems might be named System 1 and System 2, or reasoning and intuition, and they differ each other for the complexity of the operations involved and the quality of the outcome. "Reasoning is done deliberately and effortfully, but intuitive thoughts seem to come spontaneously to mind, without conscious search or computation, and without effort" [Kahneman, 2003]. Effort is, thus, the main variable that can be used to distinguish whether a mind process has been carried out by System 1 or by System 2. As in the heuristics topic, effortless intuition may be powerful, as they reduce

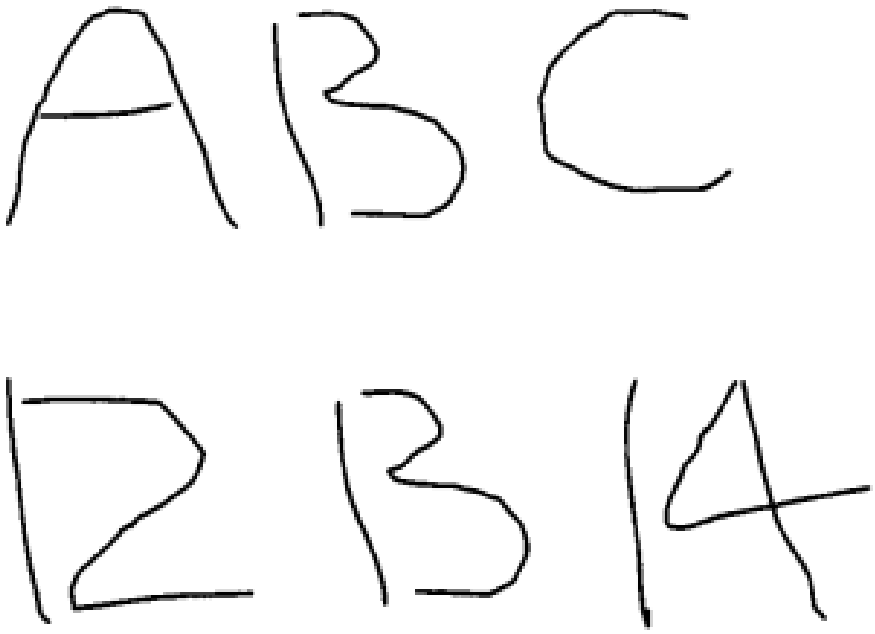

Figure 4.3: Example of Accessibility

the complexity of the problem and allows agents to grasp useful insights about the uncertain situation she's facing, but it could lead to mistakes. It turns out that intuitions are readily generated by mind whenever a problem has to be addressed. If those intuitions are incorrect they have to be changed, and this operation is performed by a self-monitoring procedure that, as it probably belongs to System 2, it is not always activated because its computational weight. But why in their evaluations, agents seem to focus on just a share of the attributes that characterize the situation? A new dimension should be introduced, and the origin of this dimension stands in the melting point of the characteristics of the object (or scenario) that has to be evaluated and agent's purpose. This dimension is called accessibility and "at one end of this dimension we find operations that have the characteristics of perception and of the intuitive System 1: they are rapid, automatic and effortless. At the other end are slow, serial and effortful operations that people need a special reason to undertake. Accessibility is a continuum, not a dichotomy, and some effortful operations demand more effort than others" [Kahneman, 2003]. Accessibility can also account for context influence in the decision-making process. As an example of accessibility and context influence, [Kahneman, 2003] can be exploited once again. In the first line of Figure 3, the second character is interpreted by everyone as a "B", while, if the second line is considered, now the second character is "13". Thus, the same character can be interpreted in two different ways according to the context in which it is inserted. Notice, furthermore, that if the two streams of characters were not displayed in the same figure, it would have been impossible simply to conceive the second character of the first line as "13".

4.3 Behavioral Economics' Disputes

This section attempts to assess the current status of behavioral economics. The task is not at all easy, as the path that the paradigm is walking down is ambiguous. On one side, behavioral economics has been absorbed in the mainstream. On the other side, some ambitious scholars hold that the real objective of the program has failed. The ambiguity that affects behavioral economics stands in the binomial descriptive-normative. While some researchers maintain that the powerful normative strictures of neoclassical economics are inadequate for descriptive purposes, other behavioral economists take the view that also normative standards should be reformulated. This tension has got far-reaching practical consequences; indeed, scholars that see agents' behavior affected by mistakes with respect to neoclassical rationality, are proposing politics to de-bias economic agents. This approach is called paternalism and it rises obvious concerns. From its very beginning, behavioral economics has been a descriptive program. The fact that a reformulation of normative stances has not taken place, can be considered a missed revolution.

4.3.1 The Descriptive Challenge

Behavioral economics' definition is the attempt to increase the realism of theoretical economic assumptions in order to improve the predictive power and reliability of economics. This operation is carried out by applying psychological findings that would help economists grasping insights about human decision-making process.

Empirical evidence resulting from experiments, has shown that the neoclassical assumptions about agents' rationality are not reliable and they fail to predict systematic economic outcomes. To accounts for those empirical mistakes, behavioral economists, most of the times, try to modify the neoclassical framework. The important point here is that the neoclassical apparatus is taken for granted, and its predictive power can be improved by slight modifications or by inserting parameters in the model. As Rabin points out, "psychological research can teach us about the true form of the function $U(x)$" [Rabin, 1998]. This quotation is a clear evidence that the utility framework, and in general the concept of decision-making as a constrained optimization problem, is retained. Psychological discoveries would allow economists to understand the real form of the utility function, that previously has been completely deduced from some economic axioms.

This subsection analyses the similarities between the mainstream approach and the behavioral approach. The benchmark is once again the prospect theory proposed by Kahneman and Tversky.

The prospect theory challenges the expected utility theory. But the problem is to understand the magnitude of that challenge. The analysis should begin with the analytical forms of the two theories:

$$U(x, p, y) = xp + y(1 - p)$$

$$V(x, p, y, q) = \pi(p)v(x) + \pi(q)v(y)$$

The first equation is the one related with expected utility, while the second one refers to prospect theory. It's not at all strange that the two equations are very similar. Indeed, the very basic assumption is that agents, when they have to face an uncertain scenario, undertake their decision by performing sort of averages. "Both

prospect theory and expected utility theory suffer from the shortcoming of assuming that risky choice always emerges from a process of weighting and averaging" [Berg and Gigerenzer, 2010].

The same similarities between neoclassical economics and behavioralists can be found by analysing Fehr's social preferences utility function ([Fehr and Schmidt, 1999]). In such a theoretical context, agents are not just concerned with their own payoff, but are also sensible with others' payoffs. That psychological findings is included in the theory by providing the utlity function with psychological parameters that can better fit the economic data. But, again, the main assumption that decision-process takes the shape of a constrained optimization problem is retained. "Instead of maximizing a neoclassical utility function that depends only on own payoffs, Fehr and Schmidt assume that people maximize a behavioral or other regarding utility function" [Berg and Gigerenzer, 2010].

For sure, the success that part of the behavioral economics has reached in economic departments can be explain by the fact that it "didn't challenge the central normative judgements of the profession. Many economists [...] felt [it] to be a quiet ally in the battle for prescriptive rationality" [Laibson and Zeckhauser, 1998]. However, that success is not able neither to erase nor to hide the tension between descriptive openness and normative dogmatism. In the last section of this chapter is shown that there is also a methodological similarity between neoclassical and behavioral economics. But now it's the time to show what are the consequences of the normative dogmatism.

4.3.2 Paternalism

The share of behavioral economics that has been absorbed by the mainstream economics, holds that the normative strictures advocated by neoclassical economists have not to be revised. On the contrary, both their experiments and their theoretical framework show that those strictures are completely inadequate for descriptive purposes, as agents systematically act differently with respect to the predictions provided by the mainstream economics. In a word, agents are biased. Those biases are described sometimes like optical illusions. Despite the fact that behavioral economics have found a lot of illusions, they are still "illusions", hence agents should be aware of them. The standard example refers to two equal lines that, due to some biases, are regarded as lines of different length. Although "in showing that human decisions contradict the predictions of expected utility theory, there is no analog to the straight lines of objectively equal length" [Berg, 2003], behavoral economists hold that departures from expected utility rationality axioms are indeed mistakes or illusions. As instance, when Kahneman evaluates the meaning of outcomes in the prospect theory (magnitude of changes) in contrast with the standard view (level of wealth), he argues that the former refers to short term, while the latter is concerned with the long-term. Furthermore, he states that:

> Which of these concepts of utility is more useful? The cultural norm of reasonable decision-making favors the long-term view over a concern with transient emotions. Indeed, the adoption of a broad perspective and a long-term view is an aspect of the meaning of rationality in everyday language. The final-states interpretation of the utility of outcomes is therefore a good fit for a rational-agent model. These considerations support the normative and prescriptive status of

> the Bernoullian definition of outcomes. [Kahneman, 2003]

Thus, behavioral economists maintain that it is possible to introduce solutions able to de-bias agents. Those solutions are included in a project that is called *paternalism*.

> In our understanding, a policy is paternalistic if it tries to influence choices in a way that will make choosers better off, as judged by themselves. [Thaler and Sunstein, 2008]

Those policies would help agents to act in a consistent way. And since consistency is a synonym of rationality in the neoclassical environment, paternalistic policies lead individuals to rationality. In such a context, training is crucial: "This perspective, implies that the full range of possible policy conclusions to be drawn from the empirical record against neoclassical behavioral assumptions consists of nothing more than a call for better training in the logic of existing models of choice" [Berg, 2003]. Policies should then promote training in the neoclassical rationality, and when this training is not possible for the nature of the transaction, a form of libertarian paternalism has to be developed. Some examples of paternalistic policies are reported below (most of the examples are taken from [Camerer, 2006]):

- **Licensing.** Driving licenses are a well-known examples of licensing. Driving is not permitted unless the driver has got a license, thus a certificate that states that he is able to act properly during the drive. The economic counter-part is the credit card license. According to this proposal, agents, in order to use a credit card, should first undertake an exam. If the exam is failed, credit card is not released.

- **Dramatizing.** As it is not always easy to foresee the consequences of current decisions, dramatizing would help agents to visualize the future results. This process is activated, of course, only when the economic decision violate neoclassical rationality that, as usual, is taken as the standard. A very common instance of the dramatizing procedure, is the invitation of testimonials to Alcoholics Anonymous. Those testimonials describe the crazy things that they do when they are drunk, so that the audience is, somehow, scared.

- **Deleting Choices.** When an agent has to undertake a decision between, say, two choices, and one of the two is considered to be erroneous, that choice can simply be eliminated. The elimination procedure should be carried out by an external entity that knows which of the choices have ill consequences.

- **Promoting Learning.** In the case of rare decisions with highly irreversible consequences, if the agents know exactly their preferences, it is likely that the occurrence of mistakes decreases. The learning period is crucial in this vision.

- **Calibration.** When the agent is about to take an evident (from the viewpoint of neoclassical rationality) incorrect decisions, an external institution has to force the agent to take the "right" decision.

Even if some of the paternalistic policies can be considered quite strong (especially calibration), it should be reminded that the biggest share of behavioral economists

in favour of paternalism, underline that it is necessary not to restrict the freedom of agents: "Libertarian paternalists want to make it easy for people to go their own way; they do not want to burden those who want to exercise their freedom" [Thaler and Sunstein, 2008].

To sum up, part of behavioral economics sees neoclassical rationality as a proper normative standard. The illusions and the mistakes that this program has discovered, should be corrected by paternalistic policies that would make agents better off. In such a view, the strength of behavioral economics stands only in the descriptive field, where neoclassical economics has been defeated by empirical evidence. The next subsection deals with the remaining part of behaviorl economics, whose idea is that the behavioral revolution took the wrong way.

4.3.3 The Missed Revolution

This subsection describes a two-pronged attack to mainstream behavioral economics, that is the part of the new program that doesn't challenge the normative status of neoclassical economics. The current status of the behavioral program is evaluated from two different perspectives. The former is a methodological approach, the latter refers to some problems that paternalism arises. Of course, the two perspectives are liked as the practical actions undertaken to solve a situation are determined by the way the problem is approached, that is the methodology applied by the problem solver. Many of the misunderstandings that cloud behavioral economics stem from the concept of rationality. While the mainstream behavioral economics draws on the neoclassical rationality for their normative prescriptions, the heterodox behavioral economics introduces a concept of rationality that take the context into account, the ecological rationality. Ecological rationality will be described at the end of the subsection.

- **Methodology.** As it has been pointed out during the description of both Prospect Theory and Fehr's Social Preferences, sometimes behavioral economics' models do not challenge the validity of the underlying neoclassical framework, but it just adds some parameters or functions that are able to include psychological processes. Prospect Theory introduces the value function and the probability weighting function, while Social Preferences add parameters to agents' utility function able to represent social concerning. Notice that, from a merely statistical point of view, the reliance of a model with n parameters is always increased when a further parameter is included in the model. Another crucial issue about this approach is that there are not parameters defined once forever. When a new sample is given, parameters are estimated over that sample, and then it is stated that the model can predict in a proper way the characteristics of the sample. It would be more methodologically correct to estimate parameters from one sample, and then apply the model to another sample: "The more challenging test of a theory is in prediction using a single set of fixed parameters" [Berg and Gigerenzer, 2010]. The fact the behavioral economics still maintains that decision making takes the shape of a constrained optimization problem (or of a weighting and averaging process) suggests that there is not a clear commitment to understand what is actually the decision-making process that takes place in agents' mind. On the contrary,

there is an hidden reliance on the As-If methodology proposed by Friedman.

> Leading models in the rise of behavioral economics rely of Friedman's as-if doctrine by putting forward more unrealistic process - that is describing behavior as the process of solving a constrained optimization problem that is more complex than the simpler neoclassical model they were meant to improve upon [Berg and Gigerenzer, 2010]

This critique is quite destructive for behavioral economics. Indeed, associate a methodology that neglect the realism of assumptions with a research program that is meant to increase the realism of economic assumptions, may create some difficulties.

- **Paternalism.** The objective of paternalistic policies, as already stated before, is to make agents' better off, as judged by themselves. This objctive, combined with the acceptance of the neoclassical rationality, yields the result that agents that do not comply with standard rationality are worse off; but "almost no empirical evidence exists documenting that individuals who deviate from economic axioms of internal consistency actually suffer any economic losses" [Berg and Gigerenzer, 2010]. Not only it might be the case that "irrational" agents are able to achieve better results than neoclassical individuals, but from a social perspective, deviations from standard rationality strictures may lead to social-improving outcomes: "[A]nomalous behavior leads to surprising social benefits. That is, systematic "mistakes" can have pro-social consequences in certain contexts. [...] Seen in this light, behavior that does not satisfy neoclassical axioms, including beliefs about the world which are incorrect, cannot be easily interpreted as aberrant, or labeled "irrational", since such behavior is (in certain decision environments) net welfare-improving relative to neoclassical rationality" [Berg, 2003]. Notice, furthermore, that behavioralists' stance is much restrictive that neoclassical. Neoclassical economics does not deal with agents that do not act consistently. On the contrary behavioral economics holds that those inconsistencies are irrational. And that's because they have a strong commitment toward neoclassical rationality. It can also be noticed that rationality axioms were introduced for descriptive purposes and not for normative suggestions. Their prescriptive features were developed by the concept of Pareto optimum.
  Further issues about paternalism rise, even if inconsistencies of choices are considered irrational, that is non-neoclassical agents actually suffer from economic losses and are worse off. The first problem can be called the "knowledge problem" ([Rizzo and Whitma, 2009]) of paternalism. For the planner it would be very difficult to determine which are the choices that are preferred by agents. And even if proper means were available, the complexity of the problem that the planner faces, would make its task quite difficult. The second problem is, on the contrary, a logical problem:

  > If an agent shows evidence of having both Preference Set X and Preference Set Y, there is no analytical basis for designating X or Y as the "true" underlying preference set of the agent. Maybe it's both; maybe it's neither.

> To choose one over the other is simply a *non sequitur* [Whitman and Rizzo, 2015]

> Notice that the *non sequitur* argument draws on a logical background. If paternalist policy aims at improving agent's welfare as judged by herself, a common judging criterion is not available, and the choice of, say X over Y simply relies on neoclassical rationality axioms.

Most of the issues outlined before originate in the concept of rationality. In neoclassical framework, rationality means consistency of choices. That consistency is described by axioms that have a non-ambiguous history. "The most important problem [...] is the complete abstraction from context" [Rizzo, 2017]. The axiomatic structures that neoclassical economics has assumed, allows it to neglect completely the context and the environment in which decision-making process are activated. A broader concept of rationality, that eventually accounts also for context, is ecological rationality. This idea has got strong relationship with the heuristics framework.

> [T]he normative framework of ecological rationality eschews universal norms that generalize across all contexts, and instead requires decision processes to match well with the environment in which they are used. Ecological rationality focuses on the question of which heuristics are adapted to which environments [Berg and Gigerenzer, 2010]

It seems that behavioral economics is in a crossroads. One road points toward neoclassical economics. There, behavioral findings are just descriptive, and the hegemons of mainstream theory is not challenged. The other road implies a reformulation of rationality that will cause also a building of a new normative structure. This will be, at the end, a true revolution.

Chapter 5

Conclusion

This thesis has depicted the methodology and the epistemology of two economic schools, the neoclassical and the behavioral. This work's purpose was to address at least two issues. First of all, the practical value of methodological and epistemological issues has been pointed out. It has been shown that the majority of economic disputes discussed nowadays, have their roots in the methodology. The way in which problems of any nature are approached, deeply influences their solution. Furthermore, the thesis tried to evaluate the status of economics, that is whether is it possible to conceive economics as an hard science. The difficulty with which economics is forced inside the narrow strictures of the methodological theories described throughout the work, might suggest that the dismal science is not an hard one.

The methodology of neoclassical economics has undergone dramatic changes. John Stuart Mill faces a social world extremely complex and economics investigates only a little share of it. According to Friedman, that complexity is not denied, but it is just avoided. In so far economic predictions are reliable, assumptions' realism is useless. The extensive use of models in neoclassical economics has encouraged the born of methodologies whose aim was to underline how a counterfactual model can shed light on the real world.

The methodology of behavioral economics splits in two conflicting sides. The first faction, that is quite close to the mainstream, aims at improving the empirical content of neoclassical economics, whose theoretical background is taken for granted. On the contrary, the second faction hopes to reformulate the concept of rationality and its objective is to construct a new theoretical building, both descriptive and normative.

No doubt, the author that most influenced the idea of who is writing is John Stuart Mill. The importance attributed to the relationship between definition of economics and its methodology might have been underestimated. Future researches on the methodology and the epistemology of economics should begin with a definition of what economics is. Obviously, the uncertain, complex and fascinating nature of economics does not ensure that the definition is definitive.

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