Metodologia de la Ciencia
Doctorado -HANDS

Resumen y Comentario del Libro:

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El libro tiene 9 capítulos. En el Capítulo 2 analiza las dos grandes corrientes en Economia: Milliana y Positivista. En el Capítulo 3 analiza , dentro de la Filosofia de la Ciencia, la linea metodológica predominante(Received View) y su derrumbamiento. En el Capítulo 4 analiza el Enfoque Naturalista (Naturalistic Turn) que trata de explicar los procesos que sigue el investigador en el descubrimiento y la justificación. En el Capítulo 5 (Sociological Turn) se explica como la actividad cientifica es un fenómeno social y como el entorno lo condiciona todo. No hay ninguna referencia a la Ideologia. En el Capítulo 6 se presta atención a temas como el Pragmatismo y otros como la epistemologia femenina. En el Capítulo 7 se analizan los desarrollos recientes en Economía en la linea poperiana, en la linea de Mill y otros. El capítulo 8 está dedicado al Giro Economicista y ,en el Capítulo 9 se presentan las conclusiones.

1. FUNDAMENTOS

1.1 METODOLOGIA ECONÓMICA. Se puede entender como **study of methods**: the practical techniques employed by succesful economists in the execution of their day-to-day proffesional activities. O bien como **rules of appraisal of economic theory:** if one wants to appraise an economic theory with respect to the scientific method, then he needs to know what the scientific method is and that specification has traditionally been the responsability of the philosophy of science.

1.2 CONTEMPORARY SCIENCE THEORY. There was a mainstream view in philosophy of science during the middle of the twentieth century- the Recieved View. Pero a partir de los años 60 y 70 esto ya no es así aunque no ha sido reemplazado por ninguna otra corriente.Para estar en la contienda hay que prestar atención a alguno de los siguientes issues: underdetermination, theory ladenness, the

social nature of science, relativism, antifoundationalism, and

naturalism. These issues and concerns constitute the **problem situation** for contemporary science theory.

Fundamentalismo Metodológico: Todos pretendemos que el conocimiento esté justificado. El Fundamentalismo Metodológico is the traditional approach to such justification. Suppose we could identify a set of basic beliefs that were directly justified- self justified and did not rely on any other beliefs for their justification- once we had such basic beliefs we could then indirectely infer the justification of other beliefs. These basic beliefs are the foundations of knowledge and the various epistemological frameworks buidt on such incorregible foundations are foundationalist approaches to knowledge. The two most influential enfoques fundamentalistas en la historia de la filosofia son : **empiricism(** where sense data serve as foundations) and **rationalism (**where reason serves as the foundation).

2. THE METHODOLOGICAL TRADITION IN ECONOMICS

2.1 The Millian Tradition

J. S. MILLL(1806-1873). "On the Definition of Political Economy"

His basic theme-economics should be and is a science but its method is not exactly the same as the method of the phisical sciences- became the dominant view for the next 100 years.

2.1.1 MILL and the Method a Priori.

Hisgreatest challenge was the reconciliation of empiricist epistemology and (Ricardian) economic theory.Mill was a radical empiricist- the only source of knowledge was sense experience; knowledge was obtained inductively; and scientifical laws were simply empirical event regularities- and yet he never surendered the Ricardian economics-an economic theory with a tight deductive structure, based on a minimal number of assumptions.

TIPOS DE CIENCIAS: Distingue entre las que pueden individualizar las causas de un fenómeno y las que no pueden individualizar. Entre las primeras, las que siguen un método a priori (economía) y las que siguen un método a posteriori (física). Entre las

segundas, las que siguen un método experimental(quimica) y las que siquen otros métodos(leyes de la mente).

The deductive character of economics derives from two main features of the discipline. **First,** because the experimental(a posteriori) method is not available in the social domain. **Second,** no interpretar esto como una desventaja pues la economia tiene unos rasgos especiales que lo justifican. The most important of these features is that the economic domain is restricted to just one particular type of fenomena: the **pursuit of wealth**.

In order to apply economics, it is necessary to verify which particular causes (and countervaling forces) are in effect. In Mill's words the a posteriori method is important in economics not as a means of discovering truth, but of verifying it in every particular case.

PERO MILL es un **empirista radical** and for him there is no such thing as a priori knowledge. Esto lo resuelve diciendo que el principio de donde se deduce todo (pursuit of wealth) se sostiene inductivamente.

Example: Falling Rate of Profit. Esta ley es verdadera. It is a true tendency law. The deduction of the law follows from the desire to pursue wealth(economics) , the laws of population theory, and the differential fertility of the soil(viewed as natural laws). En la realidad es dificil observar el funcionamiento de esta tendencia. The law is too inexact to predict (even qualitatively) what the rate of profit will be in a particular capitalist economy at any particular point in time. The practical man may desire more but such abstract tendency laws are really the best that economics has to offer. Economics is clearly a science and produces knowledge but it is a particular type of science and the type of knowledge that it produces is not the same as the knowledge available in the physical sciences.

2.1.2 The Millian Tradition in the Nineteenth Century

SENIOR argued that the science of political economy ultimately rested on four general propositions:1) that every man desires to obtain additional wealth with as little sacrifice as possible;(2) that the population is limited by the available resources; (3)that

capital enhances the productivity of labour and (4) that agriculture exhibits diminishing returns.

JOHN CAIRNES sique defendiendo el enfoque de Ricardo frente a empiristas, institucionalistas e historicistas. Pero se distingue de Mill en algunos aspectos.

CAIRNES is much more insisting about the importance of the discipline's hypothetical character. The conclusios of economic science will correspond with facts only in the absence of disturbing causes, which is, in other words, to say that they represent not positive but hypothetical truth.

Another difference is that the fact that experimentation is not available in economics , he views this as a strength and not just a dissimilarity with physics.(como Mill)." The economist starts with a knowledge of ultimate causes… He is already, at the outset of his enterprise, in the position which the physicist only attains after ages of laborios research. "**Economics is not just good science, it seems to be blessed science".**

CAIRNES also discusses the role of verification in economics. For Mill science is wholly empirical; it starts with particular facts and ends with particular facts; deduction is just a convinient middle step. Not so for Cairnes. Because we know the relevant causes with certainty, the only role for empirical evidence is in verification at the end of the process.

JOHN NEVILLE KEYNES(Scope and Method of Political economy-1917). He says that science requires general laws. But how can such general laws be obtained in an nonexperimental science such as economics? Certainly not from any a posteriori method; instead, the economist must rely on deduction from elementary principles of human nature. This a priori method will, of course, result in hypothetical science that will, because countervailing causes, be concerned with tendencies only. Empirical verificationist certainly required, but as with Cairnes, it comes in at the end and is only to determine which disturbing causes were in effect.

2.1.3. ROBBINS

"An essay on the nature and Significance of Economic Science" (1932/1952)

Robbins responded to the absence of controlled experiments by grounding economics on the indisputable facts of direct introspective experience. For Robbins the postulates of economics are the fallible implications of some long inferential train; they rest on that which is inmediate and obvious. "**they are so much the stuff of our every day experience that they have only to be stated to be recognized as obvious."**

Close to Mill pero con diferencias, especialmente in the definition of ecomics science. What makes something the subject of economic analysis is its scarcity. "**Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses".**

2.1.4 AUSTRIAN ECONOMIC METHODOLOGY

Aunque close to the Millian tradition the Austrian tradition is both **antiempiricist** (and, thus, deeply at odds with Mill) and earnestly **marginalist** in its economics (and thus equally at odds with Mill's commitment to classical economics).

The position of **CARL MENGER(1840-1921)**. Althogh Mill and Menger both end up advocating a deductive a priori approach hay la tension between an empiricist-inspired deductivism(the Millian tradition) and the openly antiempiricist of Menger.

LUDWIG VON MISES (1881-1973) represents a radical departure from the methodological mainstream. Mises called his approach to economic methology "**praxeology". The** origins of praxeology are **Kantian:** making the objective word match up to our concepts and experiential framework. For Kant, there were certain basic principles and judgements that formed the basis of our knowledge- things such as the rules of logic, the idea that every event has a cause,and the facts that objects exit- that are so fundamental to our understanding that without them no meaningful experience would be possible at all. These principles cannot come from outside, from empirical observation, but must be syntetic a priori true. For Mises, economic knowledge also has a (unique) necessary precondition- a syntetique a priori true proposition

necessary for the possibility of meaningful experience- it is that human beings act engage in **intentional or purposive behavior**.

The Misesian approach has at least three important methodological implications:

Metodological individualism: the collective has no existence and reality but in the actions of individuals.

Methodological dualism: human and social science are fundamentally different in character than the natural sciences.

Methodological a priorism: For Mises economics is not subject to tests; the fundamental presuppositions of praxeology are a priori true, and, therefore, assuming the deduction is done correctly, the conclusions of deductive arguments based on those premises are true as well. Mises would certainly agree that empirical evidence can be useful in deciding about the applicability or relevance of a certain results for a particular problem or in a specific context, but these are questions about history not about economic theory.

FRIEDRICH HAYEK(1899-1992), although sharing many of Mises's views, seems much more resigned to empirical science.The aims of a social science such as economics "is to explain the unintended results of the actions of many men". Such social science must start with human action, the subjective goal-directed action of individual agents, but it is much more. Social science must study the coordination of those individual actions into social phenomena and strutures that were not the goal of any individual agent. Hayek call this approch the "**composite"** method and attributes it originally to Menger.

2.2 VARIATIONS IN POSITIVIST THEMES

The **big three** represent the sum total of what we learned about economic methodology in graduate school.

2.2.1 HUTCHISON

Terence Hutchison was only twenty-six years old when **The Significance and Basic Postulates of Economic Theory (1938)** appeared in print.

He drew a demarcational line in the sand. Hutchison's criterion for demarcating the scientific and empirically meaningful from the non-scientific and meaningless resides in the empirical testability (potential falsifiability) of the

proposition in question. If the proposition is subject to "**intersubjective empirical test"**then it is science; if not, then it is not.

Although Hutchison admitted the usefulness of nonempirical pure theory , he did not consider the main laws of economics to be of such analytical character. **The laws of economics were testable empirical propositions.** The primary law of economic motivation- the assumption of rational ecomomic man- was not simply an a priori proposition ; it was a testable empirical proposition about human behaviour.

2.2.2. FRIEDMAN

Milton Friedman's essay on"**The Methodology of Positive Economics" (1953)** is clealy the best known work in twentieth-century economic methodology. It was a marketing masterpiece.Friedman was writing in response to, at least, three contemporary debates regarding the theoretical and empirical practices of the economics profession.

First, the debate over the appropiatness of marginal analysis in the study of labour markets and the theory of the firm. Richard Lester(1946) and others had made the case (in part based on survey data from busines managers) that firms do not actually maximize expected returns as assumed in the standard marginalist framework.

Second, was the" imperfect competition revolution"- initiated by Chamberlain(1933) and Robinson(1933)- which offered a major challenge to the assumption of perfectly competitive markets.

Third, and most relevant in light of later developments in economic theory, was the so-called measurement without theory debate between representatives of the Cowles Comission and the Chicago School of economics. The stable equilibriun of Friedman was a type of Marshallian, partial equilibriun,small-number-of-

equations micro model different from the models elaborated by the Cowles practitioners.

Friedman's main argument was that for the purpose of positive (as opposed to normative) economics, the truth of the assumptions of a theory do not matter all. The only thing that matters in deciding among varios economic theories is which one is most succesful in making empirical predictions. The theory that makes the most accurate predictions in the relevant domain is the best theory, and if it employs unrealistic assumptions this should not in any way detract from its success as a positive scientific theory. **Predicting a novel fact –evidence not yet observed- is the key determinant of a successful economic theory.** Of course if prediction is all that matter, novel or otherwise, then the realism of the assumptions is entirely irrelevant to the importance of an economic theory.

Friedman's essay has generated a massive **critical and interpretative** literature.

Alan Musgrave's (1981) point is that not all assumptions play the same role in economic theory. He divides the "assumptions" in economics into three main types: negligibility, domain and huristic.

Daniel Hausman (1992) employs the "used car argument".

1. A good used car drives reliably.
2. The only test of whether a used car is a good used car is whether it drives reliably.
3. Anything one discovers by opening the hood and checking the separate components of a used car is irrelevant to its assessment.

2.2.3. SAMUELSON

During the 1950s and 1960s, the teaching of college-level economics in the United States stabilized around two key texts: Samuelson's **Economics** (1948)at the undergraduate-introductory level and Samuelson's **Foundations of Economic Analysis** (1947) at the graduate level. With

respect to theoretical content, both texts affirm the neoclassical synthesis of Walrasian microeconomics and Keynesian macroeconomics.

Samuelson's stated economic methodology is **operationalist and descriptivist.**

The core **operationalist** idea is that a question has meaning only if there exists a set of operations that will provide a definite answer to it. The meaning of a concept or term is defined by the set of operations. Samuelson makes clear that he is exclusevely concerned with the derivation of operationally meaningful theorems. For Samuelson, a theorem is operational if it can be empirically tested.

The second feature is a descriptivist view of scientific theories. Scientific theories merely describe the empirical evidence and do not go beyond the evidence to explain any deeper, underlying, or hidden causes of the phenomena.Scientific explanations are nothing more than convenient redescriptions of the empirical evidence motivated by convenience or other pragmatic concerns. We will see in the next chapter that such descriptivism was representative of early(but not later) logical positivism.

Pone dos ejemplos: ejercicios de estática comparativa en sus libros y la teoria de la preferencia revelada. Esta última está basada en la expresión

$$\sum p_i x_i^* \leq \sum p_i x_i \Rightarrow \sum p_i^* x_i \succ \sum p_i^* x_i^*$$

Cuando los precios son p_i se compra x_i pudiendo comprar x_i^*. Por lo tanto, se prefiere el primero al segundo y esto se ha derivado operacionalmente. Cuando los precios son p_i^* se compra el segundo porque no se puede comprar el primero.

3. THE BREAKDOWN OF THE RECEIVED VIEW

The Received View was the dominant framework within Anglo-American philosophy of science during the 1950s and 1960s.

3.1. THE RECEIVED VIEW.

The Received View is most clearly identified with the logical empiricist programe in the philosophy of science and logical empiricism in turn descended from the logical positivism of the Vienna Circle.

3.1.1.LOGICAL POSITIVISM.

Historically, logical positivism began with **Moritz Schlick's** Thursday evening discussion groups in Vienna in the late 1920s. The programe combined Frege and Russell's conception of logic with the classical empiricist/positivist epistemology of Hume, Compte and Mach. Mathematical and logical propositions were **analytic**; they were true in all possible worlds . The knowledge of empirical science was **syntetic** and true only under certain empirical conditions. The idea was to start with sentences about observable empirical incorigible foundations of knowledge- then via induction, generalise these observation sentences to obtain scientific theories. Scientific theories were merely redescriptions of empirical observations expressed in terms of some phenomenal observation language, **the protocol language**. The sentences that formed the empirical basis for science were thus **protocol sentences**-sentences expressed in the phenomenal protocol language- and these protocol sentences formed the ultimate foundations for science; all

scientific discourse was either expressed in terms of these protocol sentences , or could be translated into them by so-called **correspondence rules.** This protocol language was an integral part of **the verifiability criterion of meaningfulness;** for a sentence to be meaningful it must be in principle verifiable, that is it must be possible to specify, at least in principle, the conditions under which the sentence would be true.

Not all menbers of the Vienna Circle were equally confortable with the criterion of meaningfulness. **Otto Neurath** was critical of it;at least, it seemed to exclude many of the concepts employed in the social sciences. Neurath's approach to the language of science was called **physicalism.** Two important points with respect this physicalist language: it is **broad** and it is **revisable**.

These changes coincided with **Carnap's** movement from the relatively strict **verifiability** criterion of significance to a more liberal principle of **confirmability** further undermining the empiricist foundationalism.

3.1.2.LOGICAL EMPIRICISM

Logical empiricism was the dominant approach in post-World War II Anglo-American philosophy of science. A few contributors to the programe are: **Braithwaite, Hempel and Nagel.** Three points:

1). **The gradual breakdown of the theory/ observation distinction.** The terms observational and theoretical continued to be employed of course but it became increasingly difficult to demarcate these two components of the language of science in any consistent way.

2). **Cognitive status of scientific theories**. Logical empiricism offered two fundamentally different arguments about the cognitive status of scientific theories. One response was the **instrumentalist view** of theories and the

other was the **realist view** of theories. **Instrumentalism** says that scientific theories are merely instruments for making empirical predictions. **Realism**, by contrast, argues that the theoretical terms in a scientific theory actually refer to real but unobservable entities and their properties. Instrumentalism dissolves(rather than solves) the problem of the cognitive status of scientific theories by eliminating such theories from the list of things that might posses cognitive significance, whereas realism shifts the responsability for cognitive significance onto the theoretical terms themselves.

3). **Change from an inductive to a hypothetico-deductive view of the structure of scientific theories**. According to classical empiricism and early logical positivism, scientific theories do not explain at all; the scientific domain is the domain of empirical observation and the purpose of a scientific theory is to describe those empirical observations. The commonsense view that science should explain what we observe in the world by uncovering deep, underlying, not observable, causal mechanisms,is a view that is alien to strict empiricism; **in science there are no depths; there is surface everywhere.**

The logical empiricist's answer to the problem of scientific explanation is the **deductive-nomological (or D-N) model** initially presented by **Hempel and Oppenheim in 1948**. In the original Hempel and Oppenheim paper, the general Laws as well as the initial conditions had to be true; in later versions, the restrictions on the general laws were weakend to conditions such as "confirmed", or "corroborated"or "not known to be false".

$C_1, C_2 .....C_N$
$L_1, L_2 .....L_M$ (explanans)

E (explanandun)

Where each C represents a sentence that describes an initial condition and each L represents a general law. Converting the previous economic example into this schematic form we have:

C_1 = x is a monopoly firm.

C_2 = marginal cost increased

C_3 = no other relevant variables changed

L_1 = all monopoly firms rise their price when marginal cost increases.

THEREFORE, firm x raised its price.

3.1.3 POPPERIAN FALSIFICATIONISM

Popper first presented his falsificationist approach in **Logik der Forschung** in 1934. For Popper, the logic of science is modus tollens rather than modus ponens and the empirical method of science is falsification rather than induction. Popper used the idea of falsification to establish his own demarcation criterion between science and not science. Popper view this demarcation criterion as a replacement for the logical positivist criterion of cognitive significance. **The theory that has passed the most severe tests is the most corroborated and is the most preferred theory.** For Popper, the empirical basis of science is **fallible,conventional and theory-laden**.

3.1.3.SELF-SEWN SEEDS OF DESTRUCTION

The next section will discuss two problems- underdetermination and theory ladenness- that have played an important role in undermining the hegemony of the Received View. Before turning to these definite critiques , though, it is useful to review the story thus far.

We need an objective theory-neutral protocol language that maps into sense experience in a simple and reliable way. Without this reliable rock-botton linkage between sense experience and the language of scientific theories, the entire empiricist project of knowledge as a particular kind of justified belief begins to unravel. The protocol sentence debate, the adoption of a version of a phisicalist observation language, the eventual blurring of the distinction between theory and observation among logical empiricists, and Popper's arguments about the fallibility, the conventional nature, and the theory-ladenness of the empirical basis, **all undermined this core empiricist project.**

3.2. THE ATTACK ON THE RECEIVED VIEW

The critical attack that finally precipitated the downfall of the Received View took place across a number of different fronts, and by a wide range of different critics: **Paul Feyerabend, N. R. Hanson, T. Kuhn, M Polanyi, W. V. Quine and S.Toulmin. We** will focus on just two-Quine and Kuhn- and just two core criticisms-underdetermination and theory-ladenness.

3.2.1. QUINE

Quine's "Two Dogmas of Empiricism" is one of the most cited and interrogated papers in post World War II philosophy. The paper is credited with establishing the so-called Duhem-Quine underdetermination thesis. The **Duhem-Quine underdetermination thesis** asserts that any scientific theory can be inmunized against refuting empirical evidence that is, that no test is truly definitive. The problem is that no theory is ever tested in isolation.

Example: neoclassical theory of demand . Over the last seventy years, repeated attempts have been made to test one or more of these empirical implications, and while the result of these tests have almost always been contrary to the theory of demand, the theory remains a respected(perhaps the more respected) component of microeconomic science. Instead of abandoning the theory, **Schultz** offered a number of different reasons why the empirical tests were less than perfect: reasons ranging from problems of aggregation to the reliability of the empirical data. **Deaton and Muelbauer** conclude that other problems (problems in the auxiliary hypotheses) must be responsible and not the theory itself.

3.2.2. KUHN

Thomas Kuhn's **"The Structure of Scientific Revolutions**" in 1962, not only helped close the door on the Received View, it also initiated a profound change in the relationship between the history and the philosophy of science, helped to create the field of contemporary sociology of scientific knowledge, and made "paradigm" an academic household word. Kuhn's basic approach was to examine the historical development of science. What Kuhn found, rather than a process of incremental development where scientific knowledge grew slowly though the steady accumulation of empirical evidence and inductive generalization (or the corroboration of potentially falsifiable conjectures) was that the actual development of great science had ocurred through a series of substantive revolutionary transformations where the old accepted scientific theory was totally abandoned and replaced by an entirely different

theoretical framework or paradigm. **However, his most radical discovery was how the revolutionary transformation took place**. Kuhn's examination of a number of different episodes in the history of great science suggested that scientific revolutions were not a particularly rational affair (at least not rational in the way that scientific rationality had traditionally been defined) . For Kuhn, the change to a new paradigm was a social change, a change in the dominant beliefs of the members of the relevant scientific community and as such it was not the type of change that could be explained in terms of any simple rules of proper scintific method.(positivist or falsificationist). The proper tool for understanding such change was not traditional epistemology but a version of **Gestalt phycology**.

It is important to emphasize how strongly **Kuhn's** view supports the "**theory-ladenness**" of observations. For Kuhn scientists do not just "see", they "see as" and it is the paradigm, their shared conceptual framework, that determines what is seen as what. The paradigm provides the lens, or interpretative framework, by which various aspects of the world are observed. The **critics** of Kuhn have accused him of many things, but the two most frequently cited are **idealism and relativism**. With respect to **relativism**, his denial is based on a type of pragmatic or generic naturalism; there are standards in science , but they are the standrds of actual scientists and they can only be examined historically.

3.3. FIRST ROUND RESPONSES

The next two sections will examine two sets of responses to the problems raised by Quine, Kuhn and others that attacked the Received View. The fist Section examines what I call "quasi-historical" responses (Imre Lakatos). The second Section considers various realist and empiricist responses.

3.3.1. LAKATOS

The idea was to accept most of what Kuhn said about the actual history of science but minimize the normative damage done by that acceptance. The goal was to find a new demarcation

criterion and new methodological rules that would be more consistent with the actual history of science, but could be justified in traditional ways and avoid the relativism and irrationalism often associated with Kuhn.

Lakatos's ultimate goal was to meld Popperian falsificationism and Kuhnian historicism. He wanted the best of both worlds: a normative philosophy of science that could be used to "appraise" scientific theories but also to have those norms be consistent with the best gambits from the history of science.

The first move in Lakatos's approach was to shift attention away from individual "scientific theories"to a series of theories contained in a **"scientific Research Program" (SRP). A SRP IS DEFINED AS A LOOSE ENSEMBLE OF A HARD CORE, A PROTECTIVE BELT, AND A SET OF POSITIVE AND NEGATIVE HURISTICS.**

For Lakatos a series of theories is "theoretically progressive" if each new theory "has some empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact; such a series is "empirically progressive" if " some of this excess content is also corroborated, that is, if each new theory leads us to the actual discovery of some **new fact**". A **SRP IS "PROGRESSIVE" IF IT "IS BOTH THEORETICALLY AND EMPIRICALLY PROGRESSIVE, AND DEGENERATING IF IT IS NOT".**

Destacar que todo depende del concepto de **novel fact. Th**rere are at least three points to be made regarding Lakatos's use of novelty as the sole criterion for progress in science (and since that definition of science is used to demarcate science from pseudoscience, novel facts are, for Lakatos, the sole criterion for what is and what is not science). **First**, there is not a generally accepted definition of "novel fact" even among members of the Popperian school. **Second**, es un concepto con una larga tradición más allá de Popper. And **third**, there isn't a clear consensus on the matter among contemporary commentators. Many simply conclude that novel facts do not have any special cognitive virtues.

Thus, the followers of Lakatos seem to be in the predicament of hanging both "progress" and "science" on the hook of novel

facts, when they do not have an agreement regarding the definition of novel facts.

3.3.2. REALIST AND EMPIRICIST MOVES

The authors of these responses are generally much less historical.After commenting some generic characteristics we will consider two particular versions of realism: **referential realism (R. Boyd)** and **transcendental realism (R. Bhaskar).**

If one were forced to characterize post-Enlightenment philosophy of science as one big fight between just two contestants then it would be a fight between "empiricism" and "realism". In its crudest form, **Empiricism** says that all knowledge is about observables, sense experience, and that scientific theories, if they are to be justified as knowledge, are nothing more than ways of systemattizing patterns within the observable domain. **Realist** says that scientific goes beyond, that is it trancends, the observable domain; that there really are underlying causal mechanisms or capacities that generate the empirical regularities that we observe and that it is the task of science to discover these hidden causes and capacities. The issues raised by Kuhn and Quine and others, appear to leave the realist world of hidden causal mechanims entirely intact.

Boyd's argument relies on the fact that science is both theory-dependent and instrumentally successful. The intuition behind the argument is the same as the so-called **no-miracles argument f**or scientific realism. The "no-miracles argument" is basically that the success of science would be simply miraculous if scientific theories were not true.

BHASKAR says that empiricist epistemology inspires an implicit ontology of empirical realism, which makes the objects of scientific investigation the same as the objects of experience.Since those things that can be observed, the objects of experience, are most often empirical event regularities, event regularities become the objects of (the only objects of) scientific inquiry. Bhaskar refers to this as the **epistemic fallacy**: the fallacy of reducing matters of ontology (existence or being) to matters of epistemology (knowledge). This epistemic fallacy generates an **ontological tension** in at least

two ways. **First**, it generates a tension between the standard philosophical characterization of scientific knowledge and the ontological presuppositions of practicing scientists. Practicing scientists look for the underlying, hidden, causal mechanisms that generate the empirical regularities they observe and consider these underlying causes, not the underlying

regularities, to be the proper objects of scientific inquiry. A second, related, tension emerges within the experimental practice of science. The empirical regularities that are supposed to be at the heart of science can only be observed in the closed environment of experimental systems, that is, there is nothing "natural" about the domain of natural sciences. Bhaskar offers transcendental realism as a solution to these problems.

Transcendental realism starts with an ontological distinction between the underlying causal mechanisms (generative structures, capacities,causal powers, etc.) and the observable patterns of events(empirical regularities). These causal laws are tendencies that may or may not exhibit themselves empirically in any particular situation. In the complex and open world outside the experimental context there are many causal forces at work, many tendencies, and that which becomes empirically manifest is coproduced by the interaction of these multiple causal factors.

For Bhaskar, the **human sciences** are clearly sciences, but they have their own unique characteristics and he does not in any way support their reduction to biology or phisics.

Van Fraassen´s The Scientific Image (1980) is an empiricist response to the realist response to the demise of the Received View. For **van Fraassen it** is important the difference between the epistemic and the pragmatic dimensions of science. Empirical adequacy is an epistemic criterion, but theory acceptance is based on a much broader set of evaluative standards that include pragmatic considerations: elegance, usefulness, simplicity and the ability to provide scientific explanations. Distinction between "observable" and "empirical", between "seeing" and "seeing that", and exposing "stone age" people to a tennis ball.

4.THE NATURALISTIC TURN

There is no place for a prior philosophy.(Quine)
This naturalistic movement turn away from a priori philosophy and toward a philosophical vision that is informed by contemporary scientific practice. According to this view, the theory of knowledge should employ the same scientific tools we use to investigate any other aspect of nature; epistemology so informed is **naturalized epistemology**.

4.1 Naturalizing Epistemology

A naturalist approach starts with first order questions and uses the answer to these first order questions to help with, to give some guidance to, questions of second order. Naturalists start with science (a posteriori) and use it to assist with philosophy (previously a priori). **Some questions**: specific science naturalism reduces epistemological questions to some particular scientific theory. BUT, what particular science?. Prescription versus description. Traditional epistemology was normative; naturalized epistemologies are only descriptive.

4.1.2. Quine's Naturalism

The germinal contribution is clearly Quine's **"Epistemology Naturalized" (1969)**. Quine is arguing us to replace a normative theory of cognition with a descriptive science. Naturalized epistemology should abandon its normative pretensions and focus exclusively on describing the knowledge production process. Quine considers a particular science that he proposes to naturalize on. Quine's naturalizing base is psycology, particularly behaviorist phycology.

4.2 Phycology and the Cognotive Approach to Knowledge

This Section will examine the literature on phycological approches to naturalized epistemology. The first subsection will examine the work of **Alvin Goldman** and others who employ contemporary cognitive psycology in the project of naturalization. The second subsection examines **Herbert**

Simon's impact on the literature of naturalized epistemology.

4.3.ENCOURAGEMENT FROM DARWIN:EVOLUTIONARY EPISTEMOLOGY.

In simplest possible terms, evolutionary epistemology argues that there is an interesting relationship between the process of biological evolution and the development of human knowledge.

4.3.1. Biology and Human Knowledge

The field actually contains two distinct but related subprograms: a more biological program that uses evolutionary theory to explain the development of particular cognitive structures in humans and animals(brains, sensory systems,..) and a more epistemologically oriented program, that uses evolutionary theory to account for the growth of scientific knowledge.

4.3.2 Popperian Evolutionary Epistemology

It is clear that Popper adopted some version of evolutionary epistemology in his later writings.

4.4 Eliminative Materialism and the Philosophy of Mind

Lo que hemos visto have been reformist in their epistemic spirit;Thre are certain contemporary naturalisms that seek to eliminate the traditional epistemological question altogether. For example: I explain what I did in terms of my beliefs and desires. Such explanations are called folk psycology (FP) since they constitute our traditional(folk) explanatory schema.

4. THE SOCIOLOGICAL TURN

Why shouldn't scientific beliefs be explained by the same type of social factors that we use to explain the beliefs of any other social group. It inmediatly raises questions about **which particular social theory** we should use in our explanatory schema. Do we explain the beliefs of scientists in functionalist , structuralist, behaviorist, Marxist, sociobiological, rational choice, or game theoretic terms?.

5.1 Society and Scientific Knowledge.

The sociological literature focused on the social and cultural aspects of science, and, on the fact that science could be understood in the same terms that one would understand any other type of social belief.

There are actually **three different phases**: the precursos-marxist and mertonian approaches-, the first generation- the Strong Program and and social constructivism- and the second generation.

5.1.2. The Marxist Tradition

Knowledge is determined by the existing relations of production.The main figure in this marxist tradition was J. D. Bernal.

5.1.3. The Mertonian Tradition.

Robert K. Merton's doctoral dissertation argued that the ideas, the norms and values of ascetic protestantism (not capitalist relations of production) created the proper cultural preconditions for the development of modern science. Merton's focus was on the external factors that determine the force, direction, and perhaps even complexion of science(not its actual content); the difference is that for Merton the relevant factors were sociological, like norms and cultural values, rather than economic forces like the marxian law of value.

Merton identified four such cultural values that, when taken together, uniquely characterize the ethos of science:

Universalism, Communism, Disinterestedness, and Scepticism.

5.2. The Sociology of Scientific Knowledge.

All of these different approaches are united by a shared refusal of philosophical apriorism coupled with a sensitivity to the social dimension to science.

5.2.1. The Strong Program

The Strong Program, unlike the Mertonian cchool is concerned with the content of scientific knowledge. The Strong Program relies almost exclusively on one specific approach to explaining scientific beliefs; such beliefs are explained on the basis of the social interests of the scientists.

5.2.2. Social Constructivism.

Major work in the constructivism program would include: Collins(1985), Cetina(1981) and Latour(1986).

The first and perhaps most visible characteristic of the constructivist literature is that most of the research contains detailed studies of scientific practice.

5.3 Nature, Society, SSK, and Economics

While a majority seem to agree that some type of a crisis exists, no hay acuerdo sobre el diagnostico de la crisis. In the paper “Epistemological Chicken” by Collins and Yearly(1992), the authors accuse two separate groups- the hyperreflexivists and what they call the French School- of pursuing reseach strategies that will ultimately lead to the destruction of SSK. The “chicken debate” has become a frequent point of departure for alternative approaches.

5.3.1. The Chicken Debate.

Collins and Yearly argue for social realism as an alternative to the relativism they find in the second- generation literature. Tere are reasons why particular scientific theories come to be dominant and the explanation is to be found in the social forces at work within the scietific community.

6.PRAGMATISM,DISCOURSE,AND SITUATEDNESS

We focus on:1). Classical Pragmatism; 2)Postmodernism; 3) Discursive turn and 4) Feminism.

6.1 The Pragmatic Turn

Consider the four issues:

1).Pragmatism provides a way out of what has become the major dilemma: being stuck between foundationalist philosophy and radical relativism.

2). Pragmatism blurs the relationship between theory and practice.

3). Pragmatism is fundamentally social.

4). Pragmatists have always been aware of the problems of theory-ladenness and underdetermination.

6.1.1. Peirce,Dewey, and Classical Pragmatism

For **Peirce**, scientific inquiry was a continous and self-correcting process of critical appraisal by those within the scientific community. Science was not legitimated by the words of philosophers but by its contribution to the enhancement of human life. Peirce's concept of truth involves someting **objective**(in the sense that it is a property of beliefs that exists independently of the beliefs of any particular individual); it is something **social** (it is a property of certain beliefs of the scientific community) ; and it is something that would be **at the end of inquiry**. This makes Peirce a scientific realist , but a scientific realist of a rather unusual sort. Instead of the traditional realism where scientific theories correctly represent the properties of the objective world, Peirce defines truth as that which the scientific community would believed at the end of inquiry, and then characterizes reality in terms of this truth; it is a realism that associates reality with possible knowledge.

For **Dewey**, as for Peirce, scientists do pursue truth, but the truth they pursue is not the standard notion of truth as a correspondence with an ultimate reality. For Dewey, truth is what works in the solution of concrete problems and furthers or enhances human life. This an instrumental notion of truth that sees truth as an effective instrument for the engagement of human life with material existence. It is an active, not a passive or reflective, notion of truth.

"**The hypothesis that works is the true one; and truth is an abstract noun applied to the collection of cases, actual, forseen and desired, that receive confirmation in their works and consequences".**

6.1.2. Classical Pragmatism and Economics.

We focus on some points of contact between pragmatism and economics. Two examples: The influence of Dewey on institutationalist economics and the thesis defended by Hirsch and De Marchi(1990) that Friedman's methology is a version of Deweyan pragmatism.

6.2. Neopragmatism and the Discursive Turn.

6.2.1. Rorty and Science as Discourse

For most authors, being informed by posmodernist ideas simply means maintaining a deep suspicion about traditional modernist stories. **Rorty** attacks what he considers is the central dogma of the Western Philosophical tradition: the **"mirror metaphor"** the core notion that the human mind "mirrors" the world. Rorty opens the door to the study of science as discourse or rethoric by focusing on human conversation; For Rorty, scientific knowledge is not the result of an attempt to mirror nature but rather the outcome of a particular type of social conversation: the scientific conversation. From here, it is but a short step to the explicit

study of science **as discourse or rethoric**: the view that science is best understood as **a type of persuasion**- but one that should be examined by employing the tools of theoretical analysis.

6.2.2. Economics, Neopragmatism and Rethoric.

The rethoric turn in Economics began with the publication of D. McKloskey's "**The Rethoric of Economics"in The Journal of Economic Literature in 1983.**

For Mckloskey, Methodology is an intellectual dead horse. **The legend is dead**: scientific theories are underdetermined and observations are theory-laden; science is social, interest-laden, situated, contextual and contingent; a priori philosophical speculation does not capture, and cannot capture, the actual practice of successful science; language and discouse matter to every aspect of human culture including science; none of the traditional philosophical dichotomies_ theory versus observation, discovery versus justification, positive versus normative,a priori versus a posteriori, or any others- constitute rigid or translocal designators; the significant things that have been said about science were said by the likes of Kuhn, Quine, Rorty,……, not Popper or logical positivists; and so on and so on. Because the study of what persuades is rethoric, the study of what persuades economists is the rethoric of economics: the study of suasion is the disciplinary interpretation of economy. "**Rethoric is the art of speaking. More broadly, it is the study of how people persuade". "**If we decide that the theory quantity of money or the marginal productivity theory of distribution is persuasive, interesting, useful, reasonable, appealing, acceptable, we do not also need to

know that it is True". A new set of questions: How does the science of economics persuade?, How does a particular economic paper, argument, theorem or author persuade? How is economic authority conveyed and maintained?. **Critics**: the first is what might be called the problem of **rethoric and irrationalism** and the second is the **so what?.**

6.3. Feminist Epistemology.

I would like to focus primarily on two main approaches: standpoint epistemology and contextual empiricism.

Standpoint theory simply claims that particular standpoints may- in certain contexts, and relative to specific class of questions- have a significant perspectival advantage over other standpoints. **Harding's** version of feminist standpoint epistemology starts from the familiar theme that all scientific beliefs are socially situated, but this situadness does not lead to relativism. All beliefs are socially situated some situations are just better than others. **Harding** offers eight reasons for privileging the standpoint of women. Harding calls the objectivity obtained by starting from the standpoint of women "strong objectivity" Previous objectivity was at best only "weak objectivity".

Contextual empiricism stars from the position that facts are social/ theory-laden and that science is fundamentally social. For Longino, science is social, but in no way implies that we must abandon the distinction between knowledge and opinion. **Longino's** answer is that the particular social structure of science allows it (at leat ideally) to exploit the multiplicity of the various conditioning social situations: in science "difference as a resource not a failure". The best way to empower the empirical domain is to have

representation from a wide range of different social perspectives.: wider representation means the availability of a wider range of sense experiences. Of course, while diversity may be neccessary for the growth of knowledge, it is not sufficient; The social organization of science must also be such that it allows for the critical engagement. The community of science is thus an "**idealized epistemic community**" that satisfies the following four critera **(Longino, 1992)**:

1). There are recognized avenues for the criticism of evidence and methods;

2). The community as a whole responds to such criticism;

3). There exit shared standards that critics can invoke;

4). Intellectual authority is shared equally among qualified pactitioners.

Longino's program is socially normative , not individually normative; there is a way to organize science so that it better serves our cognitive goals. The cognitive key to the social organization of science lies in its ability to foster effective criticism. Diversity is necessary, but the proper social organization this diversity promotes objectivity not chaos or relativism.

7 RECENT DEVELOPMENTS IN ECONOMIC METHODOLOGY

The literature on economic methodology has exploded during the last thirty years.

7.1. The Popperian Tradition

It seems ironic that his influence among economists is actually greater than his influence among philosophers of science in general.

7.1.1.Tensions within Popperian Methodology

They preach falsificationism but in fact almost never practice it.

Four general problem areas:

1). Duhem-Quine underdetermination problem.

2). Popper's recognition of issues like underdetermination and theory- ladenness.

3). The difficulties surrounding the notion of truth and the grounds for Popper's version of scientific realism.

4). The final tension is that while falsificationism is the standard interpretation of the Popperian position within economics, it at odds with the methodology that Popper actually endorsed when writing about the social sciences.

He emphasized the essential role of the " **Rationality Principle" (RP) and "Situational Analysis" (SA).** In the explanation of human behavior. Neither RP nor SA sit very confortably with falsificationism. An SA explanation of why agent A did act X can be given in the following schematic form:

Description of the situation: Agent A was in situation S.

Anlysis of the situation: In situation S the appropiate (rational) thing to do is X.

The Rationality Principle(RP): Agents always act appropiately given their situation

Explanandum: Therefore:A did X.

7.1.2. The Lakatosian Turn

The two authors most responsible for bringing Lakatos into economics were **Spiro Latsis and Mark Blaug**. The most significant of Lakatos's deviations from Popper occur at the methodological level. The most noticeable is that Lakatos switched the unit of analysis from scientific theories to scientific research programs. The two main methodological features that have received the most attention in economics are the structure and appraisal of particular research programs. Economists have tried to identify the hard core, positive huristic, and so on, and then to appraise the research program with respect to its theoretical and/or empirical progressivity.

I will focus my attention on just one particular case: **Roy Wintraub's (1985) study of General Equilibrium Theory**. The Neo-Walrasian program is organized around the following six hard core propositions:

1) There exist economic agents.
2) Agents have preferences over outcomes.
3) Agents independently optimize subject to constraints.
4) Choices are made in interrelated markets.
5) Agents have full relevant knowledge.
6) Observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium satates.

Comenta la evolución seguida por este programa.

Finally, and quite independently of Wintraub's study, there is a whole controversy surrounding the Lakatosian notion of a **novel fact**.

7.1.3. Critical Rationalism and Economics.

An alternative reading of Popper is critical rationalism. Falsificationism is simply a particular case of critical rationalism. Critical rationalism is normative without providing any strict rules for the conduct of scientific inquiry. The program asserts that there are rational reasons for believing in one theory rather than another, but these reasons are based on systematic criticism.

Falsificationist is not a theory of knowledge; it is simply the answer to the question of how one goes about exposing scientific theories to the maximun empirical criticism when the empirical basis is uncontested.

The methodological problem for critical rationalism is neither the ploblem of demarcation nor the problem of finding rules for the practise of science that will transmit truth from empirical observations to scientific theories**; the problem for critical rationalism is the question of how to organize our scientific and educational institutions in a way that maximizes productive criticism. This means that critical rationalist philosophy of science is a version of social epistemology: a version guided by the central notion of the role of criticism in the growth of knowledge.**

One version of critical rationalism, that of **Ratdnitsky and Bartley(1987),** considers that the proper industrial organization of our cognitive lives is the competitive marketplace of scientific ideas. If there are many alternative hipotheses competing in an open and critical environment, then knowledge will emerge from this marketplace of ideas in precisely the same way that economic efficiency emerges from a system of competitive markets.

I stressed that the objetivity of natural and social science is not based on an impartial state of mind in the scientists, but merely on the fact of the public and competitive character of the scientific enterprise and thus on certain social aspects of it. **Objetivity** is based upon mutual rational criticism, upon the critical approach, the critical tradition (**Popper)**.

Critical Rationalism fits much more confortably with both the practice of mainstream economics and **Popper's** own recommendations regarding the social sciences, than **Falsificationism.**

Critical rationalism was introduced into economic methodology by **Kurt Klappholz and Joseph Agasi** (1959). "Our view, on the contrary, is that there is only one generally applicable methodological rule and that is the

exhortation to be critical and always ready to subject one's hypothesis to critical scrutiny". **K-A** consider that empirical testing is important- it is one very important type of criticism- but it is a cardinal mistake to lay down the rule that empirical testing should be the only acceptable method of criticism.Criticism is a wide-ranging and many-faceted concept.It should not be restricted to one particularly quite narrow,dimension.

Among those currently writing in the field of economic methodology, **Lawrence Boland**(1982,1986,1997), has been the most consistent advocate of the critical rationalist approach. His view of science is **Socratic, based on learning through criticism.** I will call this view the **Socratic-Popper.** In a sense, the rule makers, falsificationist or otherwise, have the advantage of a clear message. So what would **Boland** put in the place of such rules?. Methodologists who follow the **Socratic Popper** will devote most of their time to fostering and encouraging criticism.

7.2 THE MILLIAN TRADITION.

This section will examine two recent interpretations of the Millian view: **Daniel Hausman (1992) and Nancy Cartwright (1989).**

7.2.1. Hausman

Hausman says that economics is an **inexact** and separate science: **inexact** because the tendency laws available in economics do not (given disturbing factors) allow for exact empirical predictions of economic phenomena, and **separate** because unique causal factors undergird all of the phenomena within its domain(pursuit of wealth of Mill or scarce means and unlimited wants for Robbins). According to **Hausman,** inexactness and separateness combine to give equilibrium theorizing its distinctive character. This character is captured in the following four properties:

1. Economics is defined in terms of the causal factors with which it is concerned, not in terms of a domain.
2. Economics has a distinct domain, in which its causal factors predominate.

3. The “laws” of the predominanting causal factors are already reasonably well known.
4. Economic theory, which employs these laws, provides a unified, complete, but inexact account of its domain.

The fundamental laws are already known –demonstrated by introspection and/or every day experience- there is no reason to try to falsify them.According to Hausman, the causal factors at work in economics give rise to tendencies; these tendencies may in fact manifest themselves in observable regularities, but sometimes they do not. One does deduce particular observations from a combination of these basic laws, initial conditions, ceteris paribus conditions, simplyfing assumptions, and so on, **but if the empirical predictions fail, it is never the basic laws that are rejected. First,** these laws are “known” and thus are not subject to refutation; **second,** they are only tendency laws subject to disturbing forces that make them empirically “inxact” at best; **third**, to reject the basic laws would mean to quit doing economics, since they define the separate science of economics; and,**finally,** the test is never a test of the laws themselves but only a test of whether the particular application exhibits the laws.

There are many disturbing factors that can interfer with a particular tendency being observed in any specific case.To use one of Cartwright’s examples: **Aspirins** have the tendency to relieve headaches but there can be disturbing factors that prevent this tedency from manifesting itself in any particular case(headache). Given that the empirical effects of these tendencies may or may not appear, depending on the relevant countervailing forces, the resulting laws are inexact “Tendencies are the causal powers underling the genuine regularities that inexact laws express”. This, of course, raises a serious question about the justification of such laws. Because they are inexact, they do not express true universal generalizations-some times they are true and sometimes they are not-and, thus, they are not at least on the traditional definitio, genuine scientific laws.

When are we justified in accepting such inexact laws?

Hausman discusses four different ways of thinking about the notion of an inexact (or tendency) law.
1). Inexact laws are approximate.
2). Inexact laws are probabilistic.
3). Inexact laws make counterfactual assertions about how things would be in the absense of interferences.
4). Inexact laws are qualified with vague ceteris paribus clauses.
Hausman suggests four criteria for evaluating whether a vaguely qualified inexact law is justified: **lawlikeness, reliability, refinability, and excusability.**

7.2.2. Cartwright

Cartwright is antifoundationalist while staying safely away from the slippery slope of relativism. The argument is simply that the final court of appeal of philosophical debates about science is the actual practice of science. **Cartwright** is antifoundationalist However, she thinks that there is objetive knowledge; it is just local desunified, and quite different from that of the Received View.
Cartwright's approach is broadly empirical and, she is openly hostile to the tradition of **humean empiricism.** From a humean, or radical empiricist, point of view, causality is simply a matter of the constant conjunction of empirical events: event A causes event B if and only if A happens before B and events of type A regularly occur in conjunction of events of type B. According to this view, scientific laws are simply universal statements about such empirical events regularities.
As previous chapters have copiously documented, there are a myriad of problems with the **humean tradition** but one issue a la que **Cartwright presta especial atención** es el olvido de los empiristas de "behind the scenes" forces, or "ocult" concepts such as causal powers, underlyng forces, or essential natures. She has introduced the essentialist vocabulary of natures, necessities, and capacities into the philosophy of natural sciences.

"The generic causal claims of science are not reports of regularities but rather ascriptions of **capacities**, capacities to make things happen, case by case. Aspirins relieve headaches. This does not say that aspirins always relieve headaches; It says that aspirins have the capacity to relieve headaches, a relativily enduring and stable capacity that they carry with them from situation to situation; capacity which may if circumstances are wright reveal itself by producing a regularity, but which is just as surely seen in one good single case". "**It is not the laws that are fundamental, but rather the capacities."**

Notice how **Cartwright's** argument for capacities interacts with her commitment to naturalistic empiricism. First, what science is must be regulated by the practice of science, and she argues repeatedly that real practicing scientists actually do presuppose that capacities and causal powers exist in the system they study. Second, given the problems of theory-ladenness and underdetermination, there is no reason to feel any more confident about the "observations" of traditional empiricists that there is to be confident about capacities and causal powers.

So what does her approach have to do with economics?. First, econometrics is offered as an example of how capacities show up in the work of pacticing scientists.The second is that the philosophical model for Cartwright's view of capacities is J. S. MILL analysis of tendency laws in economics.

With respect to econometrics, consider the following demand equation: q=ap+u. The estimated parameter a is a stable capacity.

Recall that Mill's main difference between economics and physics was that in the physical world causal forces act separately so that the nature of the causes can be inferred from the empirical behavior of the system in question. In economics things are not that simple; the social world is quite complex and economic causes are almost never isolated or act alone(nor are they additive).The result is that

in economics it is not possible to conduct empirical studies that are capable of isolating any of the varios, and constantly changing, causes.
The solution for Mill was to focus on "tendencies".
Cartwright follows Mill but differs from Hausman's interpretation. "Inexact laws are statements about capacities. They describe not what happens when a certain cause is present but rather what the cause has the tendency, or capacity, to do".

7.3 Realist Themes.

We focus on Lawson and Maki.

7.3.1 Critical Realism

Lawson follows Bhaskar's transcendental realism. In his book "Economics and Reality" (1997) he says that the goal of social science is **the identification of the deep structures and underlying causal powers that give rise to the general pattern of observed events; the goal is causal explanation and not the discovery of laws formulated in terms of constant conjuntions of empirical events. Since these** underlying causal powers are only weakly related to their empirical manifestations, it is not possible to discover these cusal forces by straighforward empirical means (positivist, Popperian, or any other approach). The appropiate investigative tool is **Retroduction: "**The goal is to posit a mechanism which if it existed and acted in the postulated manner, could account for the phenomenon singled out for explanation".Lawson's own contribution:
Critical realism=transcendental realism+a"special theory of social ontology".
Lawson's social ontology focus on human choice and intentionality. But there are also deep social structures and underlying causal mechanisms that influence the events on the surface of social life.**The critical realist approach requires both individual intentional action and deep social structures and relatioships.**
CRITICS TO LAWSON: the most important is the problem of what might be called **endearing structures.**

Critical realists want to identify the enduring and intransitive causal structures that lie behind the surface phenomena of social life, and yet they offer **no unique method** , no particular approach or technique that gives us privileged access to those enduring structures.

7.3.2. Realism and Anti-Realism in Economics.

Maki is more inmediately descriptive and less narowly focused on changing the ontological frame work of mainstream economic theory.Maki has argued repeatedly that economics is inmune to an entire line of debate- perhaps the major line of debate- about the question of scientific realism in physical science. His argument is that the theoretical terms of economics are in fact quite familiar from commonsense experience. The issue for economics is not the existence of the entities but the way they are arranged, that is, about whether the essential causal mechanims have or have not been included into the relevant theory.

One example is his recent work on Ronald Coase.

7.4. COGNITIVE AND SEMANTIC THEMES

7.4.1. Intentionality, and Economics

Rosenberg philosophical framework is basically a contemporary version of the Received View.

Economics has not demonstrated the type of empirical progress that characterizes the natural sciences. The empirical predictions of economists are few in number, never very specific, and not very accurate. Economic predictions are too generic and qualitative and, most importantly for Rosenberg, the empirical predictions of economists have not exhibited systematic improvement through time.

Rosenberg attributes the failure of economics to a feature that it shares with most other social sciences: the commitment to the intentional explanatory framework of belief, action, and desire.

8.- THE ECONOMIC TURN

Ecomists have analized agriculture, mining, iron and steel production,…. And the production of all sorts of goods and services, but they have neglected to analyse the production of knowledge. (**Machlup**).

Science theory has begun to take an economic turn. Economics is no longer just a subject for science theory, it has now become an important resource to be used in science theory. The previous two chapters focused primarily on the flow of ideas from philosophy and science studies into economics; this chapter will examine the flow of ideas in the opposite direction: from economics into contemporary science theory.

8.1. FACTORS THAT EXPLAIN THE ECONOMIC TURN.

Contemporary science theory suggests that practicing scientist follow their own (or group) interests and not necessarily the methodological rules laid down by philosophers of science. If that is the case, then why not employ economics- the social science that is more concerned with the collective consequences of self-interested behavior- to help explain the activity of these scientific agents?.

The remainder of this section will discuss six separate developments that have contributed to the economic turn in recent science theory.

1.Growth Theory. Particular developments taking place within growth theory since the late 1950s have systematically directed economists attention toward the role of science and scientific knowledge. (**Solow**). An even more significant step was made in the late 1980s and early 1990s with the new or endogeneous growth theory of **P. Romer.**

2. The Microeconomics of Science Policy.

Three major debates: **first,** British debate over social control of science. **Second,** the U. S. debate over government

funding of basic science in the period inmediately following World War II. **Third,** How much and in what specific ways, should the government be involved in basic research?.

3.Extending the Explanatory Reach of Microeconomics.

The combination of these two features- the desire to extend the tools of economics into new explanatory domains and tha ability to employ recent developments in economic theory while doing so- certainly helps to explain why the study of scientific behavior has recently piqued the interests of economic theorits.

4.Can Science be both Social and Rational?

Is there a way to save some kind of scientific rationality from the creeping relativism of recent science theory?. For over two hundred years, economists have focused on the question of how individually self-interested agents can, within the context of certain institutional structures bring about a result that is simultaneously (1), socially desirable and (2) not the intention of any individual agent or group of agents. Suppose that it could be shown that individual scientists do not need to follow the rules of scientific method in order for cognitively optimal results to emerge from the social context of tha scientific community.

5.Economics as a Naturalizing Base.

In Chapter 4,it was argued that one needs to choose a naturalizing base. Evolutionary biology and cognitive psycology have served as bases. So why not economics?.

6. Economics and Epistemology are deeply Intertwined.

Popper, Marx, Simon,…. Economics was always there; the only difference is that now with the economics of scientific knowledge –ESK- we actually get to see the rabbit go into the hat.

Chapter 5 emphasized the distinction between the sociology of science and the sociology of scientific knowledge (SSK). For SSK, the content of science is conditiond by the social context while for the sociology of science (Merton) it is presumed that science produces legitimate knowledge. I would like to employ a similar distinction between the economics of science and the economics of scientific knowledge (ESK). The economics of science predicts and explains the behavior of scientists and

scientific institutions, whereas ESK adds the question of whether those actions and institutions produce scientific products that are cognitively efficient and optimal (or if they are not optimal, how the institutions might be cahanged in order to improve epistemic efficiency).

8.2 -THE ECONOMICS OF SCIENCE

Dasgupta and Davis(1994), major figures in what has been called the “new economics of science” .Two of the most influential papers in this literature were **Arrow(1962) and Nelson(1959)** .The main contribution of these and similar papers was to apply the standard tools of walfare economics, particularly cost-benefit analysis and the theory of externalities and public goods, to the question of the "op“imal" l”vel of basic scientific research.

The socially optimal (or efficient)quantity of any particular good(Q_{SO}) is given by the quantity that maximizes the **net social benefit (NSB)** = total social benefit (SB)-total social cost (SC). The first order condition for this maximization problem requires the marginal social benefit (MSB) to be equal to the marginal social cost (MSC). For most goods in a market economy the problem is not determining the quantity Q_{SO} but rather determining the relationship between Q_{SO} and the quantity of the good produced by the competitive market that produce goods where suppy is equal to demand. Relevant issues: market failure, positive externalities, negative externalities,….The botton line for most authors was that basic scientific research has substantial positive externalities and is therefore underproduced by the competitive market. The main policy implication was that basic science needs to be subsidized.

This was not the only approach to the (old) economics of science. There also were a number of industrial organization economists who produced studies of the impact of science on a number of soecific industries (partucularly military).

8.3 THE ECONOMICS OF SCIENTIFIC KNOWLEDGE(ESK).

Three different approches

8.3.1 Philosophers and ESK.

There are many examples where philosophers employed economic concepts and argumentation in the service of science theory. I would like to focus on just one contemporary philosopher: **P. Kitcher. Kitcher certainly wants to save old-fashioned epistemic virtues.** The goal is to provide "a philosophical framework for the study of science which combines the insights of legend with the insights of its critics". Individuals are the subject of knowledge, but what counts as a justification, the standards of reliability, must be **social** standars. The relevant issue is ,thus, social epistemology, or, to translate the project into the language of economics, it is a study in (epistemic) industrial organization: the industrial organization of our cognitive lives. Economists are interested in finding out the arangement of our social institutions that is most conducive to economic efficiency; Kitcher's normative philosophical project is to find out the arrangement of our cognitive institutions that is most conducive to epistemic efficiency (that best encourages the formation of reliable beliefs). Knowledge still resides in individuals(not society) but the important questions are about the (epistemic) efficiency of our social institutions. **The main social epistemological project consists in the investigation of the reliability of various types of social processes.**
According to **Kitcher, the** key to the growth of knowledge- and here he borrows from biology- is diversity: in this case cognitive diversity. If real scientists do not follow the rules, if they do not always obey the methodological norms, it might actually be a good thing; if everyone followed the same set of rules, there would be cognitive uniformity and not (knowledge promoting) cognitive diversity. It is also fairly easy to see how economics comes into play. **Kitcher's most intensive use of economics is oriented to show how it is possible that egotistical motives could lead to a division of cognitive labour.** However, Kitcher is not, unlike certain other philosophers making an arguent for complete laisser-faire.

8.3.2-Economics and ESK

New economics of science; it is much more explicit about normative epistemology and the cognitive evaluation of the various scientific institutions it considers. As in the previous section, I will not attempt to summarize the arguments offered by (or most) of these different ecomomists. Rather,I would like to focus on two specific approaches: **P. Dasgupta and P. David (1994) and J. Wible (1995).**

P. Dasgupta and P. David (1994) provide a broad general framework for research in the new economics of science.They consider that the accumulation of reliable knowledge is an essentially social process. This particular reliability- producing form of social organization is called **open science ;** the most important distinguishing characteristic of **open science** is: results are made public as soon as possible and are generally available for all to see/use/criticize. The key to the success and continued reproduction of open science lies in the way tha its organizational structure channels individual self-interest into open behavior. The vehicle of this transmissionis the reward system and its emphasis on the priority of scientific discoveries. **Open Science** is thus a social organization where the individual`s own interest is best served by making reliable results, and only reliable results, public as quickly as possible. Thse results have clear implications for the design of an optimal incentive structure for the scientific community: **the better is the performance, the higher will be the reward.**

Their botton line is that the institutional structure of science is running just fineand is not in need of any major institutional reform, but it is not just fine because open science is a competitive market. Rather, science is just fine because open science has a unique institutional structure and reward system that is managed by the scientific communiyu itself. The political message: **defense of the autonomy of the open science.**

J. Wible, is interested in using economic analysis to understand how the particular social structure of science produces the growth of scientific knowledge, and, once the mechanism is understood,

be able to better evaluate various suggestions for improving the cognitive efficiency of science. He does much more than simply apply the notion of the invisible hand to the market place of scientific ideas.

Wible examines two different economic theories of science: a **substitutes view** and his own **complements view**. With repect to **substitutes,** the basic notion is that nonmarket institutions (government, nonprofits, ..) serve as substitutes for markets.The relevant concept is **trasaction costs.** Instead of asking: Why do firm exit? This approach would ask the question: Why does Science exist?. The answer would be that the institutions of science exist in order to reduce the transaction costs associated with the production of scientific knowledge in competitive markets.

As an alternative, **Wible** proposes his own approach to economic institutions. He sees the nonmarket institutions like the govertnement, the firm and the science, as complements to rather than substitutes for, the competitive market. The **epistemic problem** is that humans face fundamentalepistemic uncertainty – knowledge is always fallible, uncertain, tenuous, and constantely being revised- this uncertainty leads to the economic problem of **epistemic scarsity. The** solution to the fundamental problem of epistemic scarsity lies in **diversity.** A single institutional structure is too risky when it comes to the production of knowledge: we need institutional portfolio diversification.

CONCLUSION

Two comments about the recent methodological literature(1) that simple rules-based economic methodology has quietly passed from the scene, and (2) that its disappearance need not be, and has not been, the death knell for philosophical and science – theoretical reflection on the discipline of economics. No doubt many of the rule-makers believed they were keeping the barbarians from the gates. My argument is simply that we abandon the narrow rules-based definition of economic methodology and redefine the field to be any literature that substantivily involves both economics and science theory; It is a broader field of methodological inquiry. I will refer to this newly expanded field as the **new economic methodology.** I simply need a term for all of the literature discussed above. There is a massive, growing, and extremely interesting literature that involves both economics and contemporary science theory, and it would be nice to have a single term to refer to it all.

9.1 Lessons from the New Economic Methodology

- **The Received View and The Legend** are gone. They are not available to be used as economic methodologists have attempted to use them in the past. This means that a certain type of rules-based methodology is no longer available.
- **The** search for a few narrow methodological rules that will definitively differentiate good scientific economics from other stuff is no longer the main subject of methodological writing.
- **T**he narrow rules-based view of economic methodology was itself somewhat of an **historical aberration.**
- Tere are some identifiable general features that emerge from within the diverse literature of contemporary science theory: Science theory is:**antifoundationalist, naturalistic, sensitive to issues of theory-ladenness and undetermination, and acutely attuned to the social character of science.**
- The studies that make their way into the new methodological literature **are much more careful** about the details of the

economics being examined, whether it be in the history of economic thought or studies of contemporary practise.

- Philosophy has not entirely left the stage: just one particular narrow brand of philosophy of science. **It is just that a much wider range of philosophical ideas are now available for consideration.**
- The use of a wider range of philosophical resources also **reintroduces** a number of philosophical ideas that were considered to be off limits in methodology just a few decades ago:

Metaphisics matters: metaphisics and ontology can now be seriously discussed within economic methodology without the accompaniment of positivist finger wagging.

Pragmatism is back: When the **Received View** was in sole possession of the epistemic high ground the only options seemed to be the binary choice of **Received View or relativism.** Now there are other options.

Ethics also has reentered the discussion of how philosophy and economics interact.

Philosophy of mind matters to discussions about economics in ways that it hasn't in years.

Philosophy of Mathematics matters to the relationship between mathematics and economic theory.

- There is also an increased tendency to**just go it alone** with respect to philosophy. The point is that those looking at economics are increasingly likely to find their own answers to questions about economicsrather than borrowing from any existing approach to scientific knowledge..
- The idea that **science is social** also introduces a fundamental change in the relationship between philosophy of science and the social sciences. **Science is fundamentally social** but it goes deeper than that; scientific knowledge is not one thing and

human interests something else. From Kuhn, to SSK, to pragmatim, to feminist epistemology: Knowledge and interests are deeply intertwined; or to put it alternatively, “interests” are not separate from “knowledge-producing interests”.