CAHIER 8557

Methodology in Economics:
The Logic of Appraisal

by

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This is a modified version of some material of a master's essay presented at Queen's University, 1982. I am indebted, for their comments, to Reuven Brenner, Université de Montréal; H.S. Gordon, Indiana University; and Queen's University; and to Richard G. Lipsey, Queen's University.

Ce cahier est publié conjointement par le Département de science économique et par le Centre de recherche et développement en économique de l'Université de Montréal.

Cette étude a été publiée grâce à une subvention du fonds F.C.A.R. pour l'aide et le soutien à la recherche.
ABSTRACT

This paper intends to develop a coherent methodological framework concerned with the appraisal of scientific theories in economics, and which is based on a postulated aim of science. We first define the scope of a methodological inquiry (precise definition of what is meant by the logic of appraisal of scientific theories) and review the work of Popper and Lakatos in the philosophy of science. We then use their results to develop a rational structure of scientific activity. We identify and analyse both a micro and macro framework for the process of appraisal and single out the importance of so-called 'fundamental assumptions' in creating externalities in the appraisal process which forces us to adopt a multi-level analysis. Special attention is given to the role and significance of the abstraction process and the use of assumptions in general. The proposed structure of scientific activity is illustrated with examples from economics.
RESUME

Ce travail développe une analyse méthodologique cohérente concernant l'approbation des théories scientifiques en économie basée sur un postulat concernant le but général de l'activité scientifique. Tout d'abord, nous définissons le domaine de cette analyse méthodologique (définition précise de ce qu'est la logique de l'approbation des théories scientifiques) et faisons une revue des travaux de Popper et Lakatos dans le domaine de la philosophie des sciences. Nous utilisons alors leurs résultats pour développer une structure rationnelle de l'activité scientifique. Nous identifions et analysons à la fois une structure micro et une structure macro pour le processus d'approbation et soulignons l'importance des soi-disants 'postulats fondamentaux' dans la création d'externalités dans ce processus qui nous force d'adopter une analyse à plusieurs niveaux. Une attention spéciale est donnée au rôle et à l'importance du processus d'abstraction et à l'utilisation de postulats en général. La structure proposée de l'activité scientifique est illustrée à l'aide d'exemples du domaine de la science économique.

"If economics is to progress, economists must give absolute priority to the task of producing and testing falsifiable economic theories. In the final analysis, it is only the mechanism of hypothesis testing that can be relied on to weed out political and social prejudices at a rate faster than the one at which they are being continually recreated by new circumstances. The Mecca of economics is not, as Marshall thought, biology or any other branch of science. The Mecca of economics is the method of science itself."1

Mark Blaug

This paper intends to develop a coherent methodological framework concerned with the appraisal of scientific theories in economics, and which is based on a postulated aim of science. Ever since William Nassau Senior wrote about the status of Political Economy as a science in the early nineteenth century2, various economists have been busy, on and off, to expound their views on how economics should and/or is being pursued qua science. The nineteenth century was, as Blaug puts it, the verificationist era, while the twentieth century saw Hutchison introduce falsificationism3. The latter principle has remained dominant in methodological writings even though wide divergences occurred. In the last thirty years or so, economic methodologists have been busy mainly to criticize, justify or re-interpret a major work, namely Friedman's Methodology of Positive Economics. Most of these criticisms have not been cast in terms of an explicit framework of reference, thereby weakening
any arguments. It is the purpose of this paper to present such a framework in the falsificationist tradition.

The paper is as follows: section 1 postulates an aim of science (based on Popper's work) and carefully defines what is meant by the logic of appraisal of scientific theories, thereby setting the scope of the analysis. Section 2 presents a review of the work of Popper and Lakatos in the philosophy of science, necessary to justify the framework proposed in sections 3 and 4. Section 3 proposes a disaggregated structure of a typical theory and enquires about the relationship between each component and its appraisal with respect to the empirical evidence. Section 4 proposes a global framework required by the occurrence of externalities in the appraisal process due to the existence of fundamental (untestable) assumptions. Finally section 5 gives some concluding comments about the possible use of such a framework.

Section 1. A metaphysical basis

In this section we will clarify what is to be the scope of a methodological analysis, define the concepts to be used and discuss the metaphysical basis on which the following sections will rest. Methodology in economics (or any other science) is concerned with the logic of appraisal of scientific theories and aims at explaining the growth of (true) knowledge. We therefore have to carefully define four terms: logic, appraisal, scientific and theory; and to propose an aim of science. We begin with the latter.

Following Popper we propose that the aim of science is to provide explanations of phenomena of the world surrounding us as they uncover themselves through sense-experience.

"(...) it is the aim of science to find satisfactory explanations of whatever strikes us as being in need of explanation. By an explanation (or a causal explanation) is meant a set of statements by which one describes the state of affairs to be explained (the explicandum) while the others, the explanatory statements, form the 'explanation' in the narrower sense of the word (the explicans of the explicandum)."

This quote needs elaboration on at least two points: the reference to causality and the meaning of satisfactory. Implicit in this proposed aim of science is that the world of experience can be explained by a cause-effect relationship between the explicans and the explicandum. It is not necessary to
presume that all phenomena are the effect of definite causes but we maintain that, to be of interest, only such phenomena can be appropriately investigated. Therefore an explanation will take the form of a causal structure.

A necessary condition for an explanation to be satisfactory is that it must be "in terms of testable and falsifiable universal laws and initial conditions", a sufficient condition is that these have been tested and corroborated. The problem of proposing criteria for satisfactory explanations is the crux of the problem in methodology; section 2 is devoted to justifying and elaborating the above criterion.

Popper's requirement of explanations in terms of universal laws seems rather excessive and indeed it is. Most philosophy of science studies are modelled with physics where the existence of universal laws is less problematic. This is not so in a field like economics. Since universal laws (in natural sciences) are the outcome of corroborated theories, we may take the latter as being the appropriate framework on which an explanation must rest; i.e. an explanation is satisfactory if it is in terms of corroborated theories and initial conditions.

As a rule the explicandum is more or less known to be true. This is so because most scientific inquiries start with a problem to be explained by explicans which have to be discovered (i.e. unknown at the moment the problem is stated). "Thus scientific explanation, will be the explanation of the known by the unknown".

It is worth quoting Popper at length on what this entails about the requirements on these explicans:

"The explicans, in order to be satisfactory (satisfactoriness may be a matter of degree), must fulfil a number of conditions. First it must logically entail the explicandum. Secondly, the explicans ought to be true, although it will not, in general, be known to be true; in any case, it must not be known to be false even after the most critical examination. If it is not known to be true (as will usually be the case) there must be independent evidence in its favor. In other words, it must be independently testable; and we shall regard it as more satisfactory the greater the severity of the independent tests it has survived." 11

With suitable ad hoc rearrangements any set of explicans can provide an explanation for a given explicandum (i.e. a given problem), therefore to be satisfactory a set of explicans must also imply other testable implications (consequences) which can provide a means of independent testing.

In other words what we want are true explanations. The concept of truth implicit in the above discussion (and the rest of the paper) can, following Tarski's theory of truth, be intuitively understood as 'correspondence with facts'. There are no criterion which can guarantee us that 'truth' has been found but we can derive some for the progress towards it.

This notion of truth is essential if we are to draw a distinction between theories as a search for knowledge and theories as a search for instruments (i.e. technological applications).
"If we wish to elucidate the difference between pure and applied science, between the search for knowledge and the search for power or for powerful instruments, then we cannot do without it [this notion of truth]. For the difference is that, in the search for knowledge, we are out to find true theories, or at least theories which are nearer than others to the truth-which corresponds better to the facts; whereas in the search for theories that are merely powerful instruments for certain purposes, we are in many cases, quite well served by theories which are known to be false 24.

Thus not only do we wish to draw a distinction between positive and normative aspects 15, but also between applied and pure science. In this paper we are only concerned with the latter 16.

From the previous discussion, we can interpret the growth of knowledge 17 as being "(...) not the accumulation of observations, (...) but the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones 18. This suggest the concept of verisimilitude which may be perceived as an index measure of the difference between the truth-content and the falsity-content of a theory 19. For science to be rational it must grow, i.e. it must be characterized by a succession of theories as a sequence with increasing verisimilitude. This, in effect, characterize the aim of science, i.e. it provides the means by which science is said to explain phenomena satisfactorily, increasing verisimilitude representing the idea of approaching comprehensive truth.

We are now in a position, having proposed an aim of science, to define the four key concepts setting the scope of our inquiry, i.e. the logic of appraisal of scientific theories.

To be consistent with the above, a theory is defined as a structure of the form A > C where A is a set of antecedents, C is a set of consequents and > is the implicational connective. Implicit in it is a causal relationship which imputes the cause of C to the elements contained in A 20. A theoretical structure of this form along with initial conditions provides, via its implications, a framework such that a satisfactory explanation can be derived if the theory is corroborated (we note that for this structure to be falsifiable we need A to be empirically corroborated 21). A theory is said to be scientific if indeed it is falsifiable 22.

By appraisal we mean the process of criticism which leads to the approbation or rejection of a given theory. It is important to note that this process is a social one (social referring to the scientific community in the related fields).

"The objectivity of science arises not because the individual is impartial, but because many individuals are continuously testing each other's theory." 23

In the following discussions, when it is argued that a component or a structure should be corroborated we do not mean by this that a given scientist (theory builder) should provide corroborating evidence for every components in order to be considered having achieved a potentially valid theory. This requirement would be unreasonable even though it would be preferable. The process of corroboration
and falsification is rather one of social criticism and when we argue for the corroboration of a component we mean that if falsifying evidence is found, it is to be interpreted as a strike against the theory. In a sense the advantage is to the runner until he/she is caught²⁴.

Finally it remains to elaborate on what is meant by the logic of the appraisal process. A methodological framework based on the logic of appraisal implicitly assumes that the scientific community is 'rational' in its appraisal, in the sense that it behaves in ways consistent with the above postulated aim of science, and that the rules of social criticism are consistent with it. In a sense we can view this methodological framework as part of a research programme based on the rationality of the scientific community as its fundamental assumption²⁵. It is not denied that ideological influences or social and political pressures do exist as determinants of appraisal (especially in economics)²⁶, what is contended is that a programme based on rationality, not only yields consistent rules, but the rational reconstruction²⁷ of the history of science which it implies is likely to fit the actual history better than a research programme based on another assumption, i.e. it is likely to be better corroborated by the empirical evidence given by the history of a given scientific field.

This implies that the methodological framework to be presented in this paper is far from being a mere list of rules of good behavior, it is rather a theoretical enquiry about scientific activity which can itself be falsified.
Section 2: Philosophy of Science

Before Karl Popper published his *Logik der Forschung* the inductivist view, was ruling in the philosophy of science. According to the inductivist view, good scientific practice is characterized first by the unprejudiced observation of facts. From those singular statements, universal statements (i.e. hypothesis, laws or theories) are inferred inductively. Thus the link is from facts to theories and again to facts for verification. The fundamental problem with this view which has been labelled Hume's problem of induction, is in establishing the truth of universal statements. The inductivist principle being invalid, universal statements are never verifiable. However they can be falsified, so that one can never prove a theory but we can reject it as being false. It is on the basis of this logical asymmetry that Popper builds his logic of scientific discovery.

As explained earlier, a theoretical statement can be represented by a proposition of the form $A \Rightarrow C$, which, according to the rules of logic is false only when A is true and C is false. If the set of experimental instances is infinite then the theory is in principle impossible to verify; "however, one instance of a false prediction (false C) in a valid experimental instance (true A) is sufficient to refute the theory". Thus only the falsifiability of a theory is possible in principle. From this, Popper derives his demarcation criterion which states that to be scientific a theoretical system must be falsifiable, i.e. it must rule out some events.

If a set of basic statements (empirical evidence) contradicts the predictions of a theory, then the theory is said to be falsified; if not, it is said to be corroborated (as opposed to proven) and waiting for further tests. The degree of corroboration depends on the severity (especially) and the multiplicity of the failed attempts to refute it. Thus the path of science is seen as one of 'conjectures and refutations'; a theory is corroborated if we are unable to find any facts to refute it, not if it agrees with many. To achieve scientific status a theoretician must specify "in advance an experiment such that if the results contradict the theory, the theory has to be given up". This has the implication, among others, of drawing a distinction between the theoretician and the experimenter. "the theoretician proposes, the experimenter - in the name of nature - disposes."

The above view has been labelled (by Lakatos) dogmatic falsificationism. By itself such a series of 'conjectures and refutations by hard facts rests on two false assumptions: 1) that there does exist a psychological demarcation between theoretical and observational entities and 2) that if a statement is said to be observational it is viewed as being proven from facts. Popper,
indeed, was not a dogmatic falsificationist and dealt with this
problem by arguing that science is a set of agreed upon rules.

It is worth quoting him at length:

"The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being."35

The main convention implicit in Popper's work are the fol-
lowing: 1) the distinction to be made between theoretical and
observational terms; 2) an agreement with respect to the rejec-
tion bounds of probabilistic theories; 3) an agreement about the
truth-value of observational propositions; 4) a decision about
whether a refuting experiment applies to the specific theory or
to the initial conditions, the auxiliary hypothesis or the ceter-
is paribus clause if there is one (i.e. the Duhem problem); and
5) a convention about the appraisal of theoretical structures
containing syntactically metaphysical terms.

We are thus led to the implication that not only proofs of
tories are logically impossible but that strict disproofs also
are; the best we can do is to refute a theory. In this sense
falsification is very different from disproof. Popper's solution
to the empirical basis problem leads to what Lakatos has labelled
'methodological falsificationism' which can be viewed as a branch
of conventionalism36. Methodological falsificationism is more
than the requirement that a scientific hypothesis be, at least in
principal, testable; it involves a complete set of rules which the
theoretician must follow if he/she is to claim scientific status.
(This also involves a redefinition of the demarcation criterion
so as to include the requirements of those methodological rules37).

The main methodological rules are the following: 1) to
ensure the testability38 of scientific statements and emphasize
severe testing rather than the number of them (i.e. a demand for
the highest empirical content39); 2) ad-hoc hypothesis are accep-
table only if they increase the empirical content of the theory,
i.e. that it increases the degree of falsifiability (at least not
reduce it); and 3) a new theory should explain the old corrobo-
rated regularities so that the growth of knowledge is possible (i.e.
the principle of correspondance).

This last requirement is probably the key one to consider
since it provides the justification for all the others. With it
science is seen as a quest for truth, as defined by Tarski to be
'correspondence with facts': i.e. the growth of knowledge is the
accumulation of explanatory power, since Popper defined the aim
of science as "(...) to find satisfactory explanations of whatever
strikes us as being in need of explanation (...), the explanation
of the known by the unknown."40
Later on, with Conjectures and Refutations, Popper introduced the idea of verisimilitude (i.e. truth-content minus falsity-content). He then relaxed both rules 1) and 3). With respect to rule 1), the requirement is for the highest verisimilitude, i.e. we need not disregard a theory which has some falsifiers, for it may still be better than any other available to explain the facts; and with respect to 3) that a new theory possesses greater verisimilitude than the overthrown. This latter approach was labelled sophisticated conventionalism by Lakatos (mainly for the implication that a theory is not rejected unless an alternative is available).

To have a purposeful methodology of science it should be able to somewhat describe the history of science, and on this count Popper's methodological framework is refuted. As Lakatos argues: "(...) Popper never offered a theory of rational criticism of consistent conventions. He does not answer the question: 'Under what conditions would you give up your demarcation criterion?'" More specifically the following criticisms apply: 1) the rule that an acceptable new theory must have greater empirical content than its predecessors is too severe (a new theory may have a great deal of promises); 2) Scientists don't always test a theory as severely as possible; a new theory must be developed, we must endeavor to extend its potential range of applications, and this fruitful process could be unjustly stopped by early refutations; 3) we may be justified to reintroduce an experimentally refuted theory, for we may have been able to develop it again. This very fact that theories are not introduced fully developed, but evolve with time, should allow us to keep inconsistent theories in the hope that further theoretical work remove these inconsistencies. In expanding Popper's framework, Lakatos attempts to deal with these criticisms.

According to Lakatos a methodological framework should be based on the history of science:

"(...) all methodologies function as historically (or meta-historical) theories (or research programmes) and can be criticized by criticizing the rational historical reconstruction to which they lead."

Analysing the history of science, he sorts out two main 'stylised facts': 1) tests are at least three-cornered fights between rival theories and experiments; 2) the most interesting experiments lead to confirmation, not falsification. From then on, two ways are open to him: either abandon the attempt to give a rational explanation of scientific changes and to use social psychology (as Polanyi and Kuhn do) or to reduce the conventional element in falsificationism (for we cannot possibly eliminate it) and replace the naive versions of methodological falsificationism(...) by a sophisticated version which would give a new rationale of falsification and thereby rescue methodology and the idea of scientific progress.

Lakatos' version effectively gets rid of the fourth and fifth decision-type conventions. To achieve this the unit of appraisal becomes a series of theories rather than a single one.
This calls forth a new demarcation criterion: "We 'accept' problemshifts as 'scientific' only if they are at least theoretically progressive, if they are not, we 'reject' them as pseudo-scientific." For Lakatos, to give the scientific label to a single theory is a mistake, we must appraise a series of theories where each theory has at least as much content as the unfurled content of its predecessors. Such a series is theoretically progressive if the new theory predicts unexpected facts, i.e. if it has excess empirical content over its predecessors; it is empirically progressive if some of this new content is corroborated; i.e. if the new theory leads to the actual discovery of new facts.

This implies that falsification does not depend solely upon experimental results:

"Contrary to naive falsificationism, no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification before the emergence of a better theory." Falsification itself becomes historical, and crucial experiments can be recognized only ex-post in the light of a new theory; and this falsifying experiment of the old theory becomes the confirming instance of the new one (recall the second 'stylized fact').

The fourth decision-type convention (the Duhem problem) becomes redundant since a scientific theory has to be appraised together with its initial conditions, auxiliary hypothesis and especially its predecessors; it does not matter which part of the theoretical structure is problematic since we appraise a series of theories.

"[...] we do not have to decide which ingredient of the theory we regard as problematic and which ones as unproblematic; we regard all ingredients as problematic in the light of the conflicting accepted basic statement and try to replace all of them. If we succeed in replacing some ingredients in a 'progressive' way (that is, the replacement has more corroborated empirical content than the original) we call it falsified."

Similarly the fifth decision-type convention becomes redundant since we can deal with theories containing syntactically metaphysical terms by shifting the appraisal to a series of theories.

Lakatos' scientific methodology of series of theories becomes extremely helpful in analysing such series (the most important class indeed) which have a continuity which connects their members, "this continuity evolving from a genuine research programme adumbrated at the start." This research programme consists of methodological rules: on the one hand the negative heuristic tells us what path to avoid; and the positive heuristic, what path to pursue.

A scientific research programme is characterized by its 'hard core' which contains the fundamental assumptions of the programme; they may be syntactically metaphysical or testable in principle but the 'negative heuristic' of the programme forbids any attempt at overthrowing them. The 'hard core' is thus protected from falsification and remains as long as the research
programme is not degenerating\textsuperscript{52}. The 'protective belt' of the programme is the set of auxiliary assumptions that bears the brunt of tests and that are subject to adjustments or eliminations. The 'positive heuristic' of the programme consists in directing the articulation of this refutable 'protective belt'; it sets out the long term research policy: "a programme which lists a chain of ever more complicated models simulating reality"\textsuperscript{53} by means of instructions which it proposes.

Alterations of the 'protective belt' lead to a series of theories which are part of the research programme, the aim of which is to provide a progressive problemshift which explains new facts or have heuristic power\textsuperscript{54} (on which we appraise research programmes).

Indeed the requirement imposed on a research programme is that:

\(\ldots\) each step constitutes a consistently progressive theoretical problemshift. All we need in addition to this is that at least every now and then the increase in content should seem to be retrospectively corroborated: the programme as a whole should also display an intermittently progressive empirical shift.\textsuperscript{55}

So the history of science consists of competing research programmes; and to overthrow a research programme, i.e. to eliminate its 'hard core' not only must it be degenerating but there must be a rival one which explains its previous success and which "supersedes it by a further display of heuristic power.\textsuperscript{56}

There is no such thing as instant rationality and instant learning; tolerance is in order.

The methodology of scientific research programme is itself a meta-research programme: i.e. a research programme about research programmes, and should be accepted only if it is not degenerating in explaining scientific developments via its 'internal history', i.e. if external history plays a small role in explaining scientific behavior\textsuperscript{57}. Lakatos' framework can be viewed as part of the falsifiabilist research programme as initially propounded by Popper. In this sense falsificationism is a progressive research programme if Lakatos' rational reconstructions have excess empirical content over those implied by a Popperian approach\textsuperscript{58}.

With the above interpretation we can assess the relationship between Popper's and Lakatos' frameworks. Popper's falsificationism can be said, in economic jargon, to be a perfect world model. Indeed it is a costless, frictionless and also to some extent timeless view of scientific activity: essentially, there is no opportunity costs involved in rejecting a theory. But we have to recognize that the scientific community is a world of transactions (of ideas, theories and discoveries) no more 'perfect' than any other 'market'; there are frictions, costs, hazards and externalities involved which must be considered. The opportunity cost of rejecting a theory may indeed be quite high: retooling, time involved, uncertainty about the falsifying experiments, the availability or not of an alternative, etc. We therefore need some 'tactical practices' to take care of these
short-term phenomena that will at the same time require enough rational practices so that in the longer run (when information gathering is more 'perfect' and frictions are negligible) the principle of falsificationism can be operative in Popper's sense. As Maxwell noted

"(...)if we wish to follow Popper's rules in the long run, then, in certain circumstances, we will well be advised to break those rules on a short-term basis. Each criticism (see p.14) argues in effect that it is against our interest to enforce too rigidly Popper's essentially long term strategic rules in the short term tactical level."

Therefore, Popper's framework can be viewed as laying the foundations of the falsifiabilist research programme (i.e. its basic core) over which Lakatos' methodology of scientific research programmes provides modifications with excess empirical content. The next section will give a more detailed structure of the appraisal of a theoretical entity and relate the ideas of this section to the field of economics.

Section 3: A Methodological Framework at the Micro Level

With the understanding of the metaphysical basis described above and the results obtained by Popper and Lakatos, we are now in a position to develop a consistent methodological framework based on falsifiability. The first step is to enquire about the structure of a typical theory and describe the logical relationship between each of its components.

Upon this analysis we will see that the existence of 'fundamental assumptions' imposes an externality on the basic structure, which forces us, in a second step, to develop a framework which is more global, taking as its basic constituents individual basic theories, and which resolves, in an efficient manner, the falsifiability problem imposed by the externality. The first step we will call the micro-framework, and the second the macro one. This will lead us to a three level structure involving models, theories and research programmes.

The Micro-Framework

To keep simplicity in the argument we consider a theory to be a proposition of the form: \( A \rightarrow C \) where A is a set of antecedents, C a set of consequents (or predictions, or implications) and \( \rightarrow \) is the implicational connective. Consistent with our previous requirements we also need that it implies a causal relationship so that the above asserts: If A holds then (because
of such and such) C also holds. As it has been often stressed
the rules of deductive logic imply that the proposition 'A > C'
can be falsified if and only if A is true60.

We can also note the following axioms with respect to the
purely logical relationship between the antecedents and the
consequents61:

1) Modus ponens: if all the antecedents are true then
   the consequents must be true because the latter are
   only the results of a deductive process.

2) Modus Tollens: if the argument is logical and any one
   of the conclusions is false then at least one of the
   antecedents is false.

3) The inadmissibility of reverse modus ponens: it is
   incorrect to argue that the antecedents are true
   because the consequents are, i.e. the fallacy of
   affirming the consequents.

4) The inadmissibility of reverse modus tollens: it is
   incorrect to argue that if any assumption is false
   then at least one of the consequents must be false,
   i.e. the fallacy of denying the antecedents.

These apply to the 'logical truth' of 'A > C'; however, as
argued above, taking Tarski's notion of truth, we are inter-
tested about truth. We therefore have to amend (1) and (2), since
in this context they are false ((3) and (4) remain valid).

The difference lies in the crucial relevance of the process
of abstraction implicit in any empirical theory, a process misun-
derstood in a great many methodological works in economics62.
Consider A(1), A(2), ..., A(n) (A(i) ∈ A) to be individual elements
of the set of antecedents A. What an empirical theory asserts is
that if the set A (the union from 1 to n) is true and sufficient
(for an explanation of C) then C is true empirically; i.e. if
the set A is true and the process of abstraction is adequate to
represent the causal structure of the model then C is true.

The process of abstraction can be viewed most instruc-
tively as a meta-assumption; i.e. an assumption about assumptions.
What it does is to assert that the set of antecedents (A(1) ... A(n)),
if true, is sufficient to imply that C is true empirically. We
therefore have a relationship between the sets A and C, via the
abstraction process, so that the truth of the structure 'A > C'
cannot be determined by the truth of A alone, i.e. modus ponens
does not apply.

For example the elements A(1) ... A(n) may be true indi-
vidually and not imply C as being true empirically because, say,
A(n+1) up to A(n + T) are omitted elements which play a significant
role in the causal determination of C. Similarly modus tollens
does not hold because C can be false and A true by way of an
inadequate process of abstraction. Therefore the antecedents are
not independent of the consequents, since the abstraction process
makes the appraisal of the antecedents dependent upon the conse-
quents. This implies that both the antecedents and the consequents
need to be empirically corroborated for the theory to be so. The former to ensure a testable situation and the latter to actually test the theory.

It should be clear that the process of abstraction is essentially the process of theorizing itself, it is what makes a theory more than a mere factual and exhaustive description. It intends to capture the relevant forces at work while neglecting the other relatively minor ones.

In this sense, Friedman may have been right when he argued that:

"To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes into account for none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained. (...) the relevant question to ask about the assumptions of a theory is not whether they are descriptively "realistic", for they never are, but whether they are sufficiently good approximations for the purpose in hand."64

...if only by "assumptions" he meant abstractions.

From our analysis, for a theory to be corroborated, the truth of the assumptions is a necessary condition, while the addition of an adequate abstraction process (represented by A and C both being true) makes it a sufficient condition. To use Sen's terminology64, the process of abstraction purposefully does not attempt to get the whole truth, nevertheless it keeps nothing but the truth. As Flora Gill so rightly pointed out,

"(...) although abstraction is involved in every hypothesis (and therefore the hypothesis is inevitably "descriptively false" in the sense that not all the dimensions of the phenomena are accounted for) it does not follow that the assumptions which underlie the abstraction must themselves be "descriptively false". Consequently, the need to abstract cannot be used as a pretext to remove assumptions from the ambit of criticism.

In summary, up to now, we derived that for a theory of the form 'A ⊃ C' (with a causal relationship) where A is the set imposed by the abstraction process, to be empirically corroborated, we need both the sets A and C to be empirically corroborated. Considering especially the 'fallacy of affirming the consequents' axiom, it is inappropriate to argue that the validity of a set of assumptions is determined by the success of its predictions.

The concept of simplicity, which has been stressed by Friedman, cannot be used as a primary criterion of the validity of a theory. However, we can analyse it in a non ad hoc way by considering a theory which is simple as 'better' than an alternative (both being corroborated) in the sense that for the former the set A may come closer to represent only the necessary and sufficient elements in the causal structure determining C, while the latter may have superfluous elements.

A final comment is that, from Gödel's incompleteness theorem, it is impossible to have a complete and coherent theoretical structure in the sense that every component of a theory is
implied within the theory itself⁶⁶. This means that a theoretical structure must contain at least one component which is exogenous, i.e., there must be at least one assumption to be tested (that is for which the validity cannot be determined by the implications of the theory).

We now turn to a more disaggregated approach in discussing the structure of a theory and its appraisal. Figure 1 presents a proposed structure which represents possible components which are actually encountered in theories.

-The consequents

The consequents of a theory are what are usually called predictions or implications; they are the result of the deductive process; what the theory explains and predicts. Logically there is no distinction between the two classes, but there is nevertheless an important practical one. As argued by Popper, a theory starts with a problem which it wants to explain, i.e., to which the intended predictions are related. The 'auxiliary implications' are these consequents which gives predictions about phenomena for which the theory was not specifically intended. The important point to be stressed is that both sets of consequents should be empirically corroborated for the theory to be potentially valid⁶⁷. This requirement arises mainly from the fact that one can explain anything if one is ready to have false predictions as well. As Massey wrote:
"In a system of economics, one might wish to have as theorems all true (or perhaps all highly confirmed) sentences that can be written in the notation of that theory. Nothing is more easily done if one doesn’t mind having false sentences also included in the actual output of the system. It is very easy to attain truth (just assert everything), as easy in fact as it is to avoid error (assert nothing) what is difficult is not the separate pursuits of these goals but their joint prosecution."

The Antecedents

We will discuss now the elements of the class of antecedents and how their adequacy can be appraised. (We will postpone the discussion about the fundamental assumptions to the end.)

A simplification occurs, when, within a postulated range of behavior (or other specifications), we choose a value (or functional) which provides computational ease. These have been included with the process of abstraction for they are essentially the same. Of course these simplifications introduce an element of ‘unrealism’ and so cannot be tested independently since they are indeed intended to somewhat distort reality to provide computational ease. The only way to see whether a simplification is adequate is to see whether it changes the tendency result or the general magnitude of the resulting predictions within a desired range of approximation. That is, to see whether the predictions are robust to different specifications.

Identities and definitions are logically the same, definitions being a subset of identities. Definitions are used as a shorthand to express the symbols in the theory, and identities, more generally, are helpful “to organize our thinking or to expose logical errors.” The important point to bear in mind is that identities don’t have any empirical content, they don’t tell us anything about the world of experience, they are necessarily irrefutable. They are a shorthand for clarity of expression in helping the deductive process. With respect to definitions, more specifically, it is important not to try to find any essentialist meaning of a concept (as opposed to the nominalist approach), words are chosen to represent what we intend them to represent and it is futile to try to seek any so-called meaning ‘true in essence’ and still more futile to argue over this essential meaning of a definition. The only criticism that can arise about definitions and identities is about whether they are used consistently.

Rules of correspondence are used to connect logical theoretical entities to the experimental data, they are necessary in order to make the theory testable. Whether all or some of the theoretical entities should be linked to the empirical basis has given rise to some debates. On the one hand, strong empiricists argue that all of them should be so, a position often associated with Samuelson among others. On the other hand, the weak empiricists require that only some theoretical entities should be related to the empirical basis; they argue that it is sufficient to make the theory testable without putting too much restrictions on its possible scope. The latter position is adopted here. The choice of an appropriate
proxy for a theoretical concept is indeed a difficult task; suffice it to say that it can be tested by 1) its consistency with the internal logical structure,75 and 2) by empirical evidence about a logical implication of using a particular proxy.76

The antecedents usually called assumptions are: law-like statements, behavioral assumptions, structural assumptions and parametric specifications. These should all be corroborated in order to provide a valid theory.

Law-like statements are simply the conclusions of another theory which has been well corroborated; in a sense these are the combination of other components, but for practical purposes it is useful to make the distinction.

Behavioral assumptions refer specifically to the behavior of economic agents, as individuals or as a group. The usual controversy is whether or not postulating an as-if behavior is acceptable, a controversy which resulted mainly from Friedman's essay in relation to the profit maximization assumption. The use of such an as-if postulate is consistent with methodological instrument- alism, but "[it] not only refuses to offer any causal mechanism linking business behavior to the maximisation of returns; it positively rules out the possibility of such an explanation."77

Parametric specifications are simply assumptions assigning a particular range of values to some theoretical variables. They may be considered a subclass of behavioral and structural assumptions.78

Structural assumptions specify various conditions under which the analysis holds. These may refer to a type of economy, of market forms, relevant institutions, technological or organizational conditions etc.79 The main role of this type of assumptions is to restrict the domain over which the analysis is intended to apply. The same comments as above apply with respect to an as-if formulation.

The most important component of the antecedents are the 'fundamental assumptions', because they directly affect, in a critical way, the appraisal of a theory. They are behavioral or structural assumptions that have empirical significance but which are not conceivably testable (at least for the time being) and that "may be significant steps in the arguments reaching conclusions which are empirically testable."80 These have also been labelled differently according to their role in the theory: 'heuristic principle', 'basic postulates', 'useful fictions', 'procedural rules' or 'definitional assumptions'.

The important feature nevertheless is that they are not independently testable. This does not mean that their truth is irrelevant, but that, in the face of uncertainty, we consider them as true for the time being, so that it does not impede the derivation of testable predictions. It is important to note that according to the rules of logic presented before, it is a fallacy to argue that successful predictions entail greater confidence about the 'fundamental assumptions'; we must take them as true for the time being, that is, not falsified. We cannot properly
determine their validity, but we can invalidate them. This is where the concept of research programmes set out by Lakatos becomes useful.

Being independently untappable, the fundamental assumptions act as an externality on the appraisal process. The latter must thus be altered, i.e. we can no more adopt a strict Popperian approach to testing. One way to take account of this problem is to consider the appraisal in terms of series of theories, each having the same set of fundamental assumptions. Lakatos, as seen in the previous section, provided a logical framework for this kind of appraisal. These series of theories correspond to what he called a research programme, and for our purpose we can think of them as being both vertical series, i.e. series that modify the existing theory towards more empirical content; and horizontal series which apply the existing theory to a wider number of fields. The set of fundamental assumptions corresponds to the concept of 'hard core' which is protected from criticism by the 'negative heuristic'. The 'positive heuristic', in a way sets out the research agenda of the programme in a way that it be consistent with the hard core.

The appraisal is in terms of research programmes, greater confidence is attached to a programme which shows increasing empirical content (i.e. progressive) than to a degenerating one. Finally a research programme is rejected only if there is an alternative one available. We therefore have that the set of fundamental assumptions is appraised in terms of the whole research programme.

we consider them as valid until the latter is rejected. Machlup recognized the importance of fundamental assumptions a long time before the work of Lakatos, even though he did not put it in the framework of a series of theories:

"The fact that fundamental assumptions are not directly testable and cannot be refuted by empirical investigation does not mean that they are beyond the pale of the so-called "principle of permanent control", that is beyond possible challenge, modification or rejection. These assumptions may well be rejected, but only together with the theoretical system of which they are a part, and only when a more satisfactory system is put in its place [...]."

Of course this implies that the process of falsification of a research programme (and thus the fundamental assumptions) is very long and quite uncertain, so that in the end the choice among competing 'hard core' may be determined by the success of the programme, but the belief in the fundamental assumptions is more likely to be based on faith.

Two comments are in order before we turn to the macro-framework. First it should be noted that the testing of a theory always involves the introduction of some 'auxiliary assumptions'. These have alternatively been labelled assumed changes and set out the appropriate conditions in a testing of the form: if such and such occurs (assumed changes) than because of this theory the following outcomes will result (predictions). This is the Duhem problem, that a test procedure not only tests the theory but the 'auxiliary assumptions' as well.
Secondly, to get more insights about the role of assumptions, we may enquire about the motives behind their postulation. Why do we assume? It turns out that we assume when we don't know or cannot know. The latter case corresponds to the fundamental assumptions, the former to the others, mainly behavioral and structural. Because we want assumptions to be corroborated, when so they could be treated as equivalent to facts. This implies a different structure of the antecedents: abstraction - fundamental assumptions - definitions - facts. Therefore there is validity in the argument that assumptions are always of the fundamental kind. However we are concerned here with the behavior of the scientific community, and in practice theories are not set out in the latter form (i.e. assumptions are not presented well corroborated), and there always remains an element of uncertainty about their corroboration so that our former structure is actually better suited for our purpose.

Section 4: A Macro Framework.

The externality imposed by the presence of fundamental assumptions forces us to consider a more global approach to the structure of scientific activity in economics. This section proposes a three level structure consisting of research programmes, 'theoretical branches' and models, in decreasing order of generality. As Boland pointed out:

"(...) falsification of a model of a theory does not necessarily imply the falsification of the theory itself. The methodological problem at issue concerns the logical relationship between models and theories and the limitations imposed by the principles of logic."

An attempt will be made first to define the above concepts and to relate them to a version of a Lakatosian framework of scientific activity. Secondly we will enquire about the relationship between forms of testability and the level of generality, and also between uncertainty and testability.

First of all, we can consider a field of scientific activity (such as economics) as composed of various research programmes, competing or co-existing. Each research programme may then consist of various 'theoretical branches' which, while keeping the same 'hard core' have significant differences in the 'positive heuristic'. The theories of those 'theoretical branches' are in turn represented by various models which take only some of their features.
It is sometimes argued that these differences are such that neoclassicism and institutionalism don't intend to explain the same set of phenomena. In this sense, they may be said to be rather co-existing than competing research programmes. However, an important point to be noted is that each of these research programmes are not differentiated by their methodology, in the sense that empirical evidence is the 'omnipresent'.

Marxism distinguishes itself analytically especially by its methodological approach. Indeed, it has been argued, by Popper among others, that Marxism is a degenerating research programme. Whether one can accept that Marxism is degenerating or not is a matter of debate, but one can agree that Marxist writings in the present day (compare Marx himself for instance) exhibit a shift toward methodological propositions. This shift toward methodological propositions is a shift away from empirical falsification per se, but that in its present state it is a degenerating research programme, so that its intended explanations of value and distribution, less and less falsifiable in order to test the theory of value and distribution as set out mainly by Ricardo, James Mill, McCulloch and to some extent John Stuart Mill. Their hard core, included mainly the following fundamental assumptions:

- The school of thought, or school of thought, is considered to be a research programme. In economics, at least five different ones exist which we can label as: Keynesianism, Neoclassicism, Institutionalism, Marxism and the Austrian School. We will not here spell out extensively each element of these research programmes, but we can instead give the main differences.

- Neoclassical economics is mainly characterized by the following: rationality, stability, clearness, and perfect foresight. On the other hand, Keynesianism, while allowing uncertainty to determine the equilibrium outcome, and market clearing (or the so-called auctioneer). Both of them base their analysis on methodological holism.

- Institutionalism bases its study on methodological holism and accepts the principle of rationality as a fundamental assumption. It instead favours behaviorism as the psychological foundation for individuals in the economic system, and methodological holism makes them adopt an objective structure which is more the type of pattern-model or story-saying.
assumptions: the law of diminishing returns, Malthus law of population, and the proposition that we can specify a dichotomy between productive and unproductive activities among occupations. This classical programme led mainly to the labor theory of value and to the wage-fund theory which was overthrown when neoclassicism came by with the marginal analysis.

Within a research programme there may exists different 'theoretical branches'; for example, within Keynesianism we may note 'post-Keynesianism' (associated with such name as Joan Robinson), and the so-called 'American Keynesianism'. Within neoclassicism we may consider the general equilibrium approach (of the Walrasian type), monetarism, rational expectations (and perhaps supply-side economics). Thus, in this sense, we may take a 'theoretical branch' as being an organized system of ideas. Within a research programme, different 'theoretical branches' are mainly distinguished by how problems are approached and about what features should be emphasized. For example 'post-Keynesianism' gives more emphasis to historical and institutional features than 'American Keynesianism' and are more interested in analysing the path toward an hypothetical equilibrium rather than its properties. Within neoclassicism, the 'rational expectation' branch gives more emphasis to the role of the endogeneity of expectations in the economic process than monetarism; and while these two are mainly interested about questions pertaining to the role of the government with respect to cyclical fluctuations, the supply-side branch is more concerned with its role with respect to the size and impact of its budget.

It is to be noted that the overthrow of a 'theoretical branch' does not imply the overthrow of the corresponding research programme for there may be another one that is valid. On the other hand, the validity of such 'theoretical branch' is determined by the various (numerous) models they give rise to; indeed by whether or not this series of models represents a progressive or degenerating problem shift.

A model is defined as a theoretical structure which takes into consideration a limited number of the features associated with the corresponding 'theoretical branch'. Their scope is more limited and they are empirically more restrictive. Indeed the main goal of building models is to put the system of ideas of the research programme to tests. Therefore while the latter may be expressed in general terms without specific reference to empirical entities, a model must be specified in terms such that its parts (antecedents and consequents) are empirically testable. Indeed a model is a prototype of a theoretical structure as specified in the previous section.

The emergence of new 'theoretical branches' is a very occasional phenomena and new research programme occurs even more rarely, however new models occur in a multitude of forms. Just as the rejection of a 'theoretical branch' does not imply the rejection of the research programme, the refutation of a model does not imply the rejection of the theoretical branch, again
because a multitude of models can represent it. Indeed it is the progressiveness of a series of models which permits us to consider a 'theoretical branch' as valid; i.e. if the various models explain an increasing amount of new facts within its system of ideas. In order to refute a theoretical branch we do not require the refutation of all the ensuing models but that they represent a degenerating problemshift.

While we noted that new 'theoretical branches' and new research programmes are rarely introduced, this does not imply that little work is carried out with respect to them. On the contrary, especially for neoclassicism, there is an important part of the work devoted to what we may call the basic core of the research programme. This work has no empirical character and for this reason may be labelled 'language theory'. Its purpose is to develop the basic system of ideas (whether formally or informally) and to study its logical implications (see for example the whole literature on general equilibrium analysis). Such work has its function in the sense that it may help the derivation of various testable models; however, without the latter they are useless if economics is to be a scientific activity.97

We may now consider the relationship between the level of generality and the mode of appraisal. An interesting feature of a model, compared to the concepts of research programme or 'theoretical branch' is that it is somewhat an independent unit, since we consider the fundamental assumptions as given. Therefore the appraisal of a model can be considered Popperian in the sense that we test the model directly with the empirical evidence without considering its relationship with other models in the research programme. This is not the case for the latter since its appraisal involves testing various sub-units which are not independent of each other; this is done using the Lakatosian framework. We are therefore led to the following proposition: as the level of generality of a theoretical structure increases, i.e. from models to research programmes, the mode of appraisal differs, i.e. from Popper's methodological falsificationism to Lakatos' problemshift.

Furthermore we can consider the relationship between uncertainty about the empirical basis and a particular theoretical component with respect to the 'principle of tenacity'. We may consider the testing of a particular model to be first subject to the author's criticism. If completely implausible results are first obtained with respect to the auxiliary implications or the predictions (no uncertainty) then the model is either revised or simply rejected (strict falsificationism). The model may also be part of a discussion paper and subject to local criticism; out of it may come further modifications (e.g. introduction of more plausible assumptions, etc.) and publication. The model is then subject to social criticism, uncertain predictions or implications may then be further tested. The more uncertain the empirical basis pertaining to the implied predictions, the more likely is the author (for instance) likely to consider the model valid in the face of contrary evidence. Finally a model may be retained until
a better one (on empirical ground) is available (methodological falsificationism). We therefore have that as the uncertainty about the empirical basis increases, whatever the mode is operational, it gets less strict and the 'principle of tenacity' is more operational.

In conclusion, an important implication of this structure of scientific activity is that the refutation of the fundamental assumptions requires the refutation of the research programme itself. This process is indeed very long since it involves a process of degeneration of the structure of models and 'theoretical branches'. It is thus understandable that they remain in use for such a long time, indeed much longer than the life of an economist. Therefore the choice of such fundamental assumptions is more likely to be based on faith rather than on empirical evidence.

Section 5: Concluding Comments

As concluding comments, we can ask what is the purpose, if any, of such a methodological framework, i.e. what can we get out from such a methodological study?

As Klappholz and Agassi strongly emphasized, criticism should be an ever ruling principle in scientific activity: "All one can do is to argue critically about scientific problems". No methodology should prone a retreat from criticism. But what kind of criticism are indeed most likely to lead us to the kind of results scientists are aiming at?

Recently McCloskey argued that any methodology should be discarded, and stress the importance of rhetoric as the most important practice. But what is rhetoric if not criticism? It transpires from McCloskey's analysis that criticism about anything is valid (whether it be literary devices, style of presentation, etc.) Where we disagree with him, among other things, is that criticism should be oriented toward the empirical evidence as given to us by the world of sense-experience (i.e. 'the real world'). In this sense the role of a methodological analysis, such as the present one, is to render the process of criticism more effective in carrying out its role in the corroboration and refutation of scientific hypothesis. Given a postulated aim of science, which we take as being perfectly reasonable (i.e. to explain phenomena of the world surrounding us as they uncover
themselves through sense-experience), the above analysis provides a coherent framework justifying the use of criticism oriented toward the relationship between a theoretical structure and the empirical evidence.

"First and foremost, methodological arguments can be used to advocate the critical attitude, by trying to demonstrate its usefulness or by arguing against different approaches. Given that the critical attitude is adopted, methodology can further its practical application by the appraisal of the 'status of particular propositions'. It is helpful to be told, for example, that one has unwittingly formulated a statement as a tautology, or rendered it untestable. It is useful to discuss explanatory power, informatively content, and the degree of testability."

Adopting a framework such as the one presented in this paper can indeed avoid arguments at cross-purposes by recognizing, in scientific practice, what is the role of a component of a theory and by stating it clearly. For instance, this could be achieved by expliciting what kind of assumptions are used, what is their role and why they are used; especially, fundamental assumptions have to be made explicit (and as sparse as possible). Furthermore, the distinction between language-theory and empirical theory should be made clear in expliciting how it is related to extensions of the research programme which actually provide testable models which can potentially falsify the basic core. Nevertheless, the foremost normative implication is still that scientists should drive toward formulating falsifiable hypothesis, as Popper so strongly emphasized.

These are normative implications of the framework we developed, except for the falsifiability principle, they don't intend to dictate scientists' behavior (this would be quite pretentious), but only to facilitate the process of criticism already present. On the 'positive side' the framework also has some implications.

First we can test this methodological framework via the history of thought of a field like economics by applying it to what Lakatos called the 'rational reconstruction' of the history of science. This paper indeed provides a framework with which one can analyse the history of economic thought, that is, in terms of which were (and are) the research programmes, by what fundamental assumptions were they characterized, what was the basic core, how was it developed and tested, etc. We can judge our framework by seeing if the 'internal history' (which is explained by our own framework) provides a better explanation then the 'external history' (or the one provided by an alternative framework based on different postulates). At first sight a Lakatosian framework seems to do reasonably well. As Blaug argues:

"To be convincing, the externalist thesis in the history of ideas must produce instance of (i) internally consistent well corroborated, fruitful and powerful scientific ideas which were rejected at specific dates in the history of science because of specific external factors, or (ii) incoherent, poorly corroborated, weak scientific ideas which were in fact accepted for specific external reasons. I can think of no unambiguous examples of either (i) or (ii) in the history of economics and therefore..."
conclude that a Lakatosian 'rational reconstruction' would suffice to explain virtually all past successes and failures of economic research programmes.\textsuperscript{103}

However, a lot of work remains to be done to reaffirm his conclusion, and also to extend the falsifiabilist research programme in economics, of which this is only one step, especially by making it more falsifiable\textsuperscript{103}.

1 Blaug (1980), p.156.
2 See Senior (1927).
3 See Hutchison (1938).
4 On this subject see Popper (1972), chap.5 "The Aim of Science" and Popper (1963), chap. 10 "Truth, Rationality and Growth of Scientific Knowledge", on which the following draws heavily.
5 Popper (1972), p.191.
6 The concept of causality is a far-reaching one in philosophy and the above statements would rightly deserve more justifications, however we feel it is outside the scope of this paper. For an inquiry specific to economics, see Hicks (1978).
8 "For there is little point in asking for an explanation of a state of affairs which may turn out to be entirely imaginary." Popper (1972), p.191.
9 See Popper (1963), p.222.
11 ibid. p.192.
13 "The status of truth in the objective sense, as correspondence to the facts, and its role as a regulative principle, may be compared to that of a mountain peak which is permanently or almost permanently, wrapped in clouds. The climber may not merely have difficulties in getting there- he may not know when he gets there, because he may be unable to distinguish, in the clouds, between the main summit and some subsidiary peak. Yet this does not affect the objective existence of the summit, and if the climber tells us 'I have some doubts whether I reached the actual summit' then he does, by implication, recognize the objective existence
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8 "For there is little point in asking for an explanation
of a state of affairs which may turn out to be entirely
9 See Popper (1963), p.222.
11 ibid, p.192.
12 About Tatski's theory of truth, see Popper (1963), chap.10,
"Truth, Rationality and the Growth of Knowledge", Popper
(1972), chap.9, "Philosophical Comments on Tatski's Theory
of Truth" and Tarski (1943-4).
13 "The status of truth in the objective sense, as correspon-
dance to the facts, and its role as a regulative principle,
may be compared to that of a mountain peak which is perma-
nently, or almost permanently, wrapped in clouds. The clim-
ber may not merely have difficulties in getting there- he may
not know when he gets there, because he may be unable to dis-
tinguish, in the clouds, between the main summit and some
subsidiary peak. Yet this does not affect the objective
existence of the summit, and if the climber tells us 'I have
some doubts whether I reached the actual summit' then he
does, by implication, recognize the objective existence
of the summit. The very idea of error, or of doubt (in its normal straightforward sense) implies the idea of an objective truth which we may fail to reach." Popper (1963), p. 226.

14 ibid., p. 226.

15 For a view that such a distinction cannot be made, see, among others, McKenzie (1981), and Samuels (1981). For a support, see Blaug (1980), chap. 5, and Blaug (1978), pp. 707–10.

16 Applied science can be viewed as a derivative of pure science in the sense that the applications are derived from corroborated theories or falsified theories which are nevertheless sufficiently good as a first approximation (e.g. Newton’s theory).

17 Scientific knowledge is not the only kind of knowledge; however we contend that “the study of the growth of scientific knowledge is (…) the most fruitful way of studying the growth of knowledge in general.” Popper (1963), p. 216. We bypass here any discussion about epistemological questions which pertains to inquire about the source of knowledge.


19 see Popper (1963), chap. 10.

20 The theoretical structure is to be elaborated in subsequent sections.

21 From the rules of logic, the relevant truth-table is as follows:

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22 See also section 2. This does not imply that the so-called language- theory, i.e. theories not set out in falsifiable terms (e.g. the whole literature on walarlian general equilibrium for instance) have no role to play in science; but their usefulness must be assessed in relation to other empirical structures to which they refer or imply. See section 4.

23 Robinson (1962), p. 27.

24 A runner which catches himself may avoid losses of energy, nevertheless.

25 This concept will become clearer in section 3.

26 Joan Robinson indeed stressed this factor: “In the general mass of notions and sentiments that make up an ideology, those concerned with economic life play a large part, and economics itself (that is the subject as it is taught in universities and evening classes and pronounced in the leading articles) has always been partly a vehicle for the ruling ideology of each period as well as partly a method of scientific investigation, (…) so economics limps along with one foot in untested hypothesis and the other in untestable slogan (…) The great difficulty in the social sciences (if we may presume to call them so) of applying scientific method, is that we have not yet established an agreed standard for the disproof of an hypothesis.” Robinson (1962), pp. 7, 28 and 26.

27 see footnote 57 for a definition.

28 The english translation, The Logic of Scientific Discovery has first been published in 1959. The following references will be from the 1968 edition, Popper (1968). The english title is rather misleading since Popper never intended to explain discovery but rather appraisal: “(…) it is irrelevant from the point of view of science whether we have obtained our theories by jumping to unwarranted conclusions or merely by stumbling over them (that is by ‘intuition’) or else by some inductive procedure.” Popper (1957), p. 135.

29 Two main attempts have been made to justify the principle of inductivism. One is to argue that the truth of universal statements is known by experience. But then again to be so it must come from singular statements (since experience can only be related to sense-observation of facts); so that the principle of inductivism itself has to be viewed as a universal statement, which itself, in turn, has to be justified by inductive inference. We are thus caught in an infinite regress which breaks the argument. See Popper (1968), chap. 1, section 1. The other attempt, made by Kant, is to regard the principle of inductivism as being valid 'a priori'; but then again this is no justification.


31 So instead of arguing for any essentialist notion of science, Popper determines whether a theory is scientific according to its methodological basis. More precisely: “A theory is to be called ‘empirical’ or ‘falsifiable’ if it divides the class of all possible basic statements unambiguously into the following two non-empty subclasses. First, the class of all those basic
statements with which it is inconsistent (or which it rules out or prohibits); secondly, the class of those basic statements which it does not contradict (or which it permits'). (...) a theory is falsifiable if the class of its potential falsifiers is non-empty." Popper (1968), p.34.

This is contrary to inductivism which defines the demarcation criterion in an essentialist way, accepting as scientific only those statements reducible to elementary statements of experience such that it is possible to assign them a definite 'truth' or 'falsity', that is, of being 'conclusively decidable'. This criterion rests on the inductivistic principle.

32 Lakatos (1978), p. 13 (emphasis added). This certainly seems to present a problem for the scientific status of theories which predict some probability distribution of events, for "hypothesis regarding probabilities are as far as their logical form is concerned (and without reference to our methodological requirement of falsifiability), neither verifiable nor falsifiable. They are not verifiable because they are universal statements, and they are not strictly falsifiable because they can never be logically contradicted by any basic statements." Nevertheless, hypothesis regarding probabilities can be made falsifiable by some prior convention on bounds: "According to my view, however, probability statements, just because they are completely undecidable are metaphysical unless we decide to make them falsifiable by accepting a methodological rule. Popper (1968), pp.261-2 (emphasis added). These methodological rules (conventions), it will be argued later, apply not only to hypothesis about probability statements but to any hypothesis.


34 Observations are not purely objective; on the one hand, they are guided by theoretical propositions, and on the other, theoretical entities may become observational entities with the development of instruments and techniques. See Epector (1966a) and (1966b).


36 Conventionalism was developed especially by Poincare, Milhaud and LeRoy. Poincare believed that scientists' conceptual framework eventually turn into prisons which cannot be demolished. Criticism of this position led to two rival schools: Duhen's conservative conventionalism (or 'methodological justificativism') and Popper's methodological falsificationism. See Lakatos (1978), chap.1, section 2-b.

37 This is essential since by suitable rearrangements it is always possible, introducing 'ad-hoc hypothesis', to make the theory fit any facts whatsoever.

38 It is worth noting that falsifiability entails consistency, since a inconsistent statement is self-contradictory which implies that it is compatible with any event, thus can never be refuted. Also Popper argues at great length that the requirement of simplicity is reducible to the degree of falsifiability. See Popper (1968), esp. chap. 7.

39 This is equivalent to asking for the highest degree of corroboration. Let $P(x)$ be an index of the empirical content of the statement $x$, $0 < P(x) < 1$ (i.e. that it be intersubjectively testable, since $P(x)$ (tautology) = 0 and $P(x)$ (contradiction) = 1. Consider the conjunction ab where a and b are theoretical statements. We have that $P(a) < P(ab)$ if $P(b)$, i.e. the empirical content of a conjunction is greater than any one of the individual statement. However, the probability of a conjunction is smaller than the probability of either of its components, i.e. $P(a) < P(ab) < P(b)$. So there is an inverse relation between probability and content, i.e. empirical content increases with increasing improbability. Therefore the methodological rule is equivalent to giving preference to the corroborated theories which have the greatest improbability.

40 Popper (1972), p. 191. In view of the comment in the previous footnote: "Yet we also stress that truth is not the only aim of science. We want more than mere truth - what we look for is interesting truth - truth which is hard to come by (...) which has a high degree of explanatory power, in a sense which implies that it is logically improbable truth." Popper (1963), p.229.


42 To paraphrase Kant: 'Philosophy of science without history of science is empty; history of science without philosophy of science is blind'. Quoted in Lakatos (1978), p. 102.


44 See Maxwell (1972).


46 ibid., p.31.

47 ibid., p.34.
This will become clearer later on when we discuss the concept of 'hard core' and its relation to the fundamental assumptions.

Examples in economics of statements that may be included in the 'hard core' of a research programme are the following assumptions: substantive rationality or procedural rationality (see Simon (1976)), optimizing behavior or sufficiency behavior (see Lastis (1976)), inherent stability of the economic system or inherent instability (see Leijonhufvud (1976)), etc. See also section 4 below.

Lakatos (1978), p.50. About models Lakatos writes: "A model is a set of initial conditions (possibly together with some observational theories) which one knows is bound to be replaced during the further development of the programme, and one even knows, more or less, how. This shows once more how irrelevant 'refutations' of any specific variant are in a research programme: their existence is fully expected, the positive heuristic is there as a strategy both for predicting (producing) and digesting them. Indeed, if the positive heuristic is clearly spelt out, the difficulties of the programme are mathematical rather than empirical." ibid., p.51.

"I use 'heuristic power' here as a technical term to characterize the power of a research programme to anticipate technically novel facts in its growth. I could of course use 'explanatory power'" ibid., p. 69.

The internal history is the intellectual history, the behavior conducted by rationality as if scientists were behaving according to the methodology of research programmes. Internal history is social history, determined by non-scientific factors. For Lakatos' attempt to reconstruct a part of the history of science according to the methodology of research programmes, see 'History of Science and its Rational Reconstructions', in Lakatos (1978).

An alternative view is presented by Lakatos who argues that his framework is part of a wider research programme labelled demarcationism (as opposed to skepticism who ranks science on an equal footing as any other beliefs) and elitism (who
delets a special class of individuals decide what is science). "Demarcationist historiography recognizes that all history of science are inevitably methodology-laden and that one cannot avoid 'rational reconstructions'. Each different type of demarcationism leads to a different 'internal reconstruction', with correspondingly different anomalies and different 'external problems'. These 'rational reconstructions', however, can be compared according to well defined standards and the history of demarcationists--classical inductivism, probabilism, conventionalism, falsificationism, methodology of scientific research programmes -- itself constitutes a progressive research programme." Lakatos (1978b), p.110-1.

Maxwell (1972), p. 136 (emphasis added).

see footnote 21.

see Boland (1979).

see Friedman (1953) for instance. However see also Gill (1981) and Sen (1980).


see Sen (1980).

It is especially imperative to ask for the corroborations of the assumptions, for then if the consequences are false the problem is likely to be found in the process of abstraction.


For example, the search theory in the field of labor seeks to explain unemployment but with the implication that quits are countercyclical. The fact that this is empirically false is a valid criticism against the theory.

Massey (1965), p.1158.
For these reasons there are indeed severe difficulties with respect to the appraisal of the consequences, for instance when are falsifying experiments not to be counted as important in light of the intended predictions?


ibid., p. 126.

"(...) it is clear that equations commonly designated as
identities, by virtue of the meaning of language, are intended to be and are in fact empty statements. They tell us nothing at all about the universe and are necessarily irrefutable. Such being the case one might legitimately conjecture that a dismissal of identities from their current employment in economic theory would occasion no loss. If an identity is an empty statement, it has nothing to impart to a theory." ibid., p.119.

72 see Popper (1962).

73 see Havrilesky (1968).

74 A simple example may throw some light on this subject. It is often argued in welfare economics that the marginal utility of a commodity can be given a money measure by the equilibrium price at which it is bought; this is an example of a rule of correspondence. On the other hand, despite all the attempts made, it is generally agreed that the total utility enjoyed by an individual cannot be given any cardinal measure, nevertheless the concept is extremely useful and can lead to testable implications. Thus it seems that a weak empirical position is less restrictive.

75 For example it is inappropriate to measure consumption by a money measure of the flow of services when the analysis purports to explain variation in aggregate expenditure.

76 E.g., in the example in note 74, the marginal utility of a good can be represented in money terms by the equilibrium price only if the purchase represents a small portion of the total income.


78 For example, whether or not goods are inferior, substitutable or complementary; whether a demand curve is elastic or not, propensity to consume greater or less than unity, utility function representing risk-aversion or risk-neutrality, etc..


80 Machlup (1955), p.11.

It is worth noting that this concept of 'fundamental assumption' is not peculiar to the social sciences; they are also found in the physical sciences, for example the assumption of gravitation and that the universe is constituted of matter. As Krupp noted: "Maximization is to the market equilibrium what gravity is to physical equilibrium". Krupp (1966), p.40.

Examples of such assumptions are: profit maximization, rational behavior, that individuals can and do order their preferences, the principal of dynamical stability and the market clearing mechanism.

81 The only case considered by Lakatos.

82 See also Maxwell (1976) who argues that the choice of a hard core influences the pattern of discovery, i.e. the problems to be analysed in a research programme.

83 See the next section for further analysis in terms of research programmes.

84 Machlup (1955), p.11. Unfortunately Machlup did not put forward the requirement that the other assumptions be well corroborated. In the end he advocated a position similar to Friedman's.

85 See Machlup (1955), p.11.


87 We can consider two research programmes as competing if they intend to explain a common set of phenomena; and co-existing if they purport to different areas.

88 Dynamical stability here refers to Samuelson's correspondence principle which is necessary to make a theory testable. See Gordon (1955) for dynamical stability exposed in the context of operationalism. See also Wilber (1978) for market clearing.

89 It may be argued that neoclassicism allow for uncertainty, but it does so mainly in an ad hoc way; that is the equilibrium outcome is determined with perfect foresight and the practice is to see how uncertainty may affect it. On the other hand keynesianism allows uncertainty to determine the equilibrium outcome directly. (See also Lastis (1972) for an exposition of the neoclassical hard core.)

90 See Wilber and Harrison (1978), Fusfeld (1980) and Dugger (1979). For example: "These characteristics of institutionalism - holistic, systemic, evolutionary - combined with the appreciation of centrality of power and the recognition of the importance of non-rational human behavior, differentiate institutionalism from standard economics. Formal models simply cannot handle the range of variables, the specificity of institutions, and the non generality of behavior." Wilber and Harrison (1978), p.72.
"(...) institutional economics can be understood as a set of concatenated theories or pattern models composed of institutions as the building blocks and with behaviorism as the psychological foundation. In contrast, neoclassical economics can be understood as a set of hierarchical theories or predictive models composed of individual firms and individual consumers as the building blocks, with subjectivism or methodological individualism as the psychological foundation." Dugger (1979), p. 904.

ibid., 907.

"The pattern model is tested empirically by comparing hypothesized institutional structures (qualitative pattern) with observations," ibid., p.901. and "(...) the explanation of a whole system is tentatively held as true, until an alternative or revised pattern is able to supersede the old model by incorporating an even greater variety of data." Wilber and Harrison (1978), p.76-7.

See Popper (1957) and (1962).

Supposing that Marxism is indeed a degenerating research programme it is interesting to note that some of their proponents try to put forward a methodological framework based on apriorism to defend it. See Holis and Nell (1975).


It is interesting to note that before its overthrow, this classical programme became somehow less and less falsifiable, as supported by the following comment by James Mill about the labor theory of value: "If the wine which is put in the cellar is increased in value one-tenth by being kept a year, one-tenth more labor may be correctly considered as having been expanded upon it." Quoted in Stigler (1958), p.363.

This may provide a justification for the huge literature on general equilibrium analysis, by arguing that their purpose is to elaborate the basic core of the neoclassical research programme. However, one can certainly criticize neoclassicism for devoting too much effort to this task and very little for testing various models derived from it. A research programme is useless if it is not repeatedly tested.


See McCloskey (1983).

Not in the political usage of the term but in the sense in which the scholarly greeks taught it. It should be clear from the present analysis, that his caricature of modern methodology is out of line.


Blaug (1976), p. 177.

It was, indeed, Feynabend's (1975) criticism of Lakatos' analysis: that it was close to being unfalsifiable so that in the end it was equivalent to his anarchistic position. We do not accept such an interpretation, but realize that Lakatos' framework is less stringent than Popper's, that is, it forbids less.
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