

LAKATOS'S APPROACH ON PREDICTION AND NOVEL FACTS†

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ABSTRACT: Lakatos's approach to prediction and novel facts is of considerable interest. Prediction appears in his conception in at least three different levels: a) as an important *aim* of the research programs; b) as a *procedure* -a key method- for increasing our scientific knowledge both theoretically and empirically; and c) as the way to *assess* the scientific character of knowledge claims -means for evaluating results-. At all these levels he envisions a close connection between prediction and novel facts.

The paper has four aims. First, to examine his concept of "prediction" in Lakatos's MSRP, taking into account different aspects (semantical, logical, epistemological, methodological and axiological). Second, to clarify the notion of "novel facts", which requires the consideration of the various ways in which *new facts* can be understood. Third, to examine the prediction of novel facts as *criterion of appraisal* (theoretical, empirical and heuristical). Fourth, to explore Lakatos's approach (i.e., the concept of prediction linked to novel facts) in connection with the field of *economics*, in order to shed new light on issues that have been discussed in recent years.

Keywords: prediction, novel facts, Lakatos, methodology, criterion of appraisal, economics.

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Among the many interesting aspects of Lakatos's methodology of scientific research programs is his approach to prediction and novel facts. Even though his views on the importance of this issue did not remain at the center of attention in mainstream methodology of science in the last twenty five years -mainly because other topics have received *more* attention (explanation, progress, truth and truthlikeness...)-, the study of the role of prediction of novel facts as a criterion of appraisal is an area where he tried

to make a significant contribution. Moreover, it is an aspect of his thought which has enjoyed particular attention within the methodology of economics. Nevertheless, within general methodology of science, there is a still controversy as to whether explanations or predictions provide superior evidential support for scientific theories¹.

Prediction appears in Lakatos's methodology of scientific research programs in at least three different levels: a) as an important *aim* of the research programs²; b) as a *procedure* -a key method- to increase the scientific knowledge both theoretically and empirically; and c) as the way to *assess* the scientific character of the knowledge claims -a form of evaluating results. At all these levels he sees a connection between prediction and novel facts. This link has a clear relevance for the grasping the semantics of *prediction* as well as for the logical relation between "explanation" and "prediction". Moreover, the link between prediction and novel facts has an epistemological content which affects the methodological procedure for *increasing* scientific knowledge as well as the methodological process of *evaluating* science according to the reliability of predictions. These different aspects of prediction in Lakatos's conception -semantical, logical, epistemological and methodological- require attention here in order to clarify his approach. They are related to axiological issues (the cognitive value of prediction in the scientific research) as well as to ontological ones (due mainly to the role of novel facts).

The analysis will be in four steps. Firstly, I examine Lakatos's concept of "prediction" in the methodology of scientific research programs (MSRP), taking into account the different aspects pointed out in the previous remarks. Secondly, I clarify the notion of "novel facts", which requires the consideration of the various ways in which *new facts* can be understood. Thirdly, to examine the prediction of novel facts as *criterion of appraisal* (theoretical, empirical and heuristical). Fourthly, to explore Lakatos's approach (i.e., the concept of prediction linked to novel facts) in connection with the field of *economics*, in order to shed new light on issues that have been discussed in recent years.

1. Prediction in the methodology of research programs

An initial difference can be found in Lakatos regarding prediction and research programs. On the one hand, there is the prediction *about* the research program itself insofar as it is a *human undertaking*. His position is straightforward: "the growth of a research programme cannot be predicted"

(Lakatos 1970, p. 69, n.)³. The reason for this is the creativity of the human mind which establishes a potentially unending stream of activity for which there is no saturation point: "there is no predictable or ascertainable limitation on human imagination in inventing new, content-increasing theories" (Lakatos 1970, p. 72, n.). On the other hand, there is the prediction *within* the research program: here is mainly a methodological element which -for him- has a central role in the development and appraisal of the research programs:

all the research programmes I admire have one characteristic in common. They all predict novel facts, facts which had been either undreamt of, or have indeed been contradicted by previous or rival programmes (Lakatos, 1974b, p. 5) ⁴.

Although Lakatos uses the word "prediction" many times *within* the methodology of research programs, its meaning is not always clear. He does not offer a *semantical* definition of "prediction" -with a clear sense and reference- but rather a *pragmatic* characterization -an use within a specific realm-: it is a proposition (a "particular hypothesis") supported by a theory -cf. Lakatos (1968, p. 192)- whose content is related to novel facts. And, insofar as he links predictions to novel facts, it seems that he is connecting predictions and future events. However, this is not the case as it can be seen in the discussion of the *sophisticated falsificationism*: "I use 'prediction' in a wide sense that includes 'postdiction'"⁵. Therefore, for him, prediction could be also backwards instead of being always forwards (i.e., related to the future). In addition, it means that he accepts that there could be "novel facts" of the past, which *prima facie* seems at odds with the notion (semantical and ontological) of *new* facts.

Nevertheless, Lakatos ordinarily uses "prediction" for the theoretical anticipation of a novel fact within a research program, even though he maintains that "the novelty of a factual proposition can frequently be seen only after a long period has elapsed" (Lakatos 1970, p. 69)⁶. Sometimes, he emphasizes his interest in pre-dictions (i. e., something said before it happens) which are successful, and he sees there a difference in comparison with other methodologies:

the successful *predictions* of novel facts which constitute serious evidence for a research programme and therefore vital parts of internal history, are irrelevant both for the inductivist and for the [naive] falsificationist (Lakatos 1971, p. 114).

From a logical point of view, prediction is -for Lakatos- a *proposition* of a theory rather than an argument. On the one hand, he maintains that pre-

diction is a "particular hypothesis" which depends on the theory (cf. Lakatos 1968, p. 192)⁷. And, on the other hand, in the structure of the research program (hard core, protective belt, heuristic rules...) he emphasizes more prediction than explanation. This position seems to favor an asymmetry between explanation and prediction (i.e., not a mere temporal anisotropy between them). However, for Lakatos, an adequate specific theory within a program must "explain" (i.e. entail) all known relevant facts (so, e.g., Einstein's theory must entail all the facts already entailed by Newton's theory). What he had in mind was the *adjustability* of specific theories to any given finite set of data (i.e., Duhem's problem); and hence the intuitive judgment that only "predicted" evidence really tests. But the latter never meant that "explanation" of known facts was irrelevant.

Lakatos criticizes the logical empiricists who see scientific hypotheses purely from the point of view of concordance with experimental data, because for him the primacy of prediction is clear: "new scientific hypotheses are assumed not simply in order to patch up gaps between data and theory but in order to predict novel facts" (Lakatos 1970, p. 86). His conception considers a research program as degenerating when accommodating known facts, whereas he conceives that a progressive program is one which anticipates new facts⁸. Moreover, "when the program ceases to anticipate novel facts, its hard core might have to be abandoned" (Lakatos 1970, p. 49), which means that it should be replaced by a new one with a different hard core.

According to this prevalence of prediction over explanation, Lakatos criticizes those who -like Th. S. Kuhn (1957, p. 224)- do not give special importance to a theory which happened to have anticipated a factual discovery. The criticism has implicit a clear difference between explanation and prediction:

like Mill and Keynes before him, Kuhn cannot understand why the historic order of theory and evidence should count, and he cannot see the importance of the fact that Copernicans *predicted* the phases of Venus, while Tychonians only explained them by *post hoc* adjustments (Lakatos 1971, p. 115, n.).

So, besides the temporal anisotropy between explanation and prediction, there is in Lakatos a structural preference (i.e., "logical") in favor of prediction insofar as it is innovative.

Epistemologically, prediction is linked in Lakatos to the possession of *testable implications*: it is an element of a theory which has observable consequences regarding some novel fact (cf. Lakatos 1970, p. 83). In addition,

prediction should be matched with the main epistemological problem of the methodology of scientific research programs, which is the connection of verisimilitude (i.e., the difference between the truth content and falsity content of a theory) with the scientific gambit of pragmatic acceptances and rejections. In other words, prediction is -for him- an epistemological rational exercise related to verisimilitude -cf. Lakatos (1971, p. 113)- (in Popper's technical sense and, therefore, it is an approach affected by the criticisms against the Popperian interpretation of verisimilitude -Hands, D. Wade 1991a, pp. 58-75-).

Where Lakatos departs epistemologically from Popper is in the *positive role* of experience: it is not a mere negative instance. Learning about a theory is -for him- primarily related to the new facts which it anticipates:

for the sort of Popperian empiricism (sic) I advocate, the only relevant evidence is the evidence anticipated by a theory, and *empiricalness (or scientific character) and theoretical progress are inseparably connected* (Lakatos 1970, p. 38).

In addition, there is a *heuristic progress* -a progressive problemshift- which, in principle, depends on the successful prediction of some novel fact: a research program progresses when its theoretical growth anticipates its empirical growth (i.e., it keeps predicting novel facts with some success -progressive problemshift-)⁹, whereas it stagnates when it gives only *post hoc* explanations of discoveries or facts anticipated by a rival program (cf. Lakatos 1971, p. 112) (like the Cartesians, which explained what Newtonians predicted, but only *post hoc*)¹⁰.

Methodologically, prediction is used for the *appraisal* of research programs both theoretically and empirically: a program is

theoretically progressive if each modification leads to new unexpected predictions and it is *empirically progressive* if at least some of these novel predictions are corroborated (Lakatos and Zahar 1976, p. 179).

Prediction plays also a key role in the interpretation of the scientific change -the *heuristic progress*-, which is not seen from the point of view of incommensurability of theories but rather as the superseding of theories: "one research programme *supersedes* another if it has excess truth content over its rival, in the sense that it predicts progressively all that its rival truly predicts and some more besides" (Lakatos and Zahar 1976, p. 179). This was the case of Copernicus's program.

Yet Lakatos distinguishes in his own approach a *method* -the methodology of scientific research programs- and a *meta-method* -the methodology

of *historiographical* research programs- which is a methodology of scientific research programs of *second order* (Lakatos 1971, pp. 131-132). The former -MSRP- includes programs with a structure (hard core, protective belt...) which is appraised mainly in terms of excess of empirical content; and the latter -MHRP- introduces a straightforward historical view: the research programs are appraised in historiographical terms (i.e., there is prediction of novel historical facts, unexpected in other historiographies, and those predictions should be corroborated by historical research -Lakatos (1971, p. 133)-). Blaug maintains that "there is (...) no inherent connection between MSRP and MHRP and it may well be that the former is true and the latter is false" (Blaug 1991, p. 503). It seems that he goes too far in denying the inherent connection between them, but he is right when he claims that scientists do not accept or reject methodologies according to Lakatos's historiographical criteria of appraisal.

Axiologically, successful predictions have in Lakatos's view a clear value for scientific research: "what really count are dramatic, unexpected, stunning predictions" (Lakatos 1974b, p. 6; cf. Lakatos 1974b, p. 5). The anticipation of future events is an epistemic value which, in principle, has for him more weight than the explanation of already known facts. Scientific progress is seen above all as the process of reaching the aim of successful predictions, due to the stronger weight given to prediction over explanation. Prediction has at least a *quadruple role* in his approach: i) it is the aim which is sought because of a superior epistemic value; ii) it is what shows theoretical progress within a methodology of research programs (i.e., the procedure should lead towards an increasing number of reliable predictions); iii) it is what counts as empirical progress insofar as the predictions are corroborated; and iv) it is central in the heuristic progress when a research program is superseded by a new one.

Even though prediction has a crucial role in Lakatos's approach, his characterization of this concept is far from being thoroughly articulated in the key philosophical realms (semantical, logical, epistemological, methodological and axiological) and so presents some deficiencies. To sum up, semantically, there is no definition of *prediction*. He offers a pragmatic interchangeable use of "prediction" and "postdiction", which are clearly two different notions (cf. Gonzalez 1995, pp. 35-56; especially, pp. 53-54). Logically, he seems in favor of an asymmetry between explanation and prediction, and at the same time he shows an overwhelming preference for prediction which requires more attention. Epistemologically, prediction appears in a better context here in Lakatos than in Popper insofar as the ex-

perience has not a pure negative role, but that approach is affected by the problems related with the Popperian verisimilitude. Methodologically, Lakatos advances in the appraisal of scientific progress when he sees three different perspectives (theoretical, empirical and heuristical), but he excessively emphasizes the task of prediction. Axiologically, prediction is evaluated as a central value of scientific research when it seems to me more accurated to consider prediction as one important element among others, even though it could be more innovative and difficult from a heuristic point of view than explanation.

2. *The notion of "novel facts"*

Due to the number of times that Lakatos mentions the expression *novel facts* in his methodology of research programs, it seems that it should be easy to identify what he means by "novel facts". This is not the case. Moreover, he himself recognizes that it is not obvious what a novel fact is: "there may be conflicting views about whether an accepted basic statement expresses a *novel fact* or not" (Lakatos 1971, p. 117). In addition, it is not completely clear what a *fact* is, because it is not something purely given but rather construed on the basis of the reality. Lakatos does not focus on the definition of *fact*, and in this point an idea of P.F. Strawson could be useful: "facts are what statements (when true) state" (Strawson 1950, p. 136).

Regarding the concept of "novel fact", Lakatos's approach primarily oscillates between the following two directions. The first one is *epistemologico-ontological*: it allows an ontological view of prediction, such as the anticipation of the existence of an entity (e.g., a planet); and the second one is *epistemologico-methodological*, because the novelty is in the reinterpretation, or in the use, of the knowledge already available¹¹. He stands in the first direction in his strongest claims: novel facts are "facts improbable in the light of, or even forbidden," by a previous theory -Lakatos (1970, p. 32)- (e.g., the phases of Venus). This supposes epistemological incompatibility between old and posterior knowledge: "a new fact must be improbable or even impossible in the light of previous knowledge"¹². However, he follows the second direction when he holds that "we should certainly regard a newly interpreted fact as a new fact" (Lakatos 1970, pp. 70-71, emphasis added in the original). The difference is clear: here the expression "new fact" does not refer at all to an ontologically new fact (i.e., an undiscovered fact, something real unknown) but rather an epistemological perspective which allows reinterpretations of known facts.

There is also the possibility of a pure methodological meaning -novelty regarding the *process of problemshift* of research programs-. This third possibility arises from the existence in Lakatos of three criteria for appraising research programs: the criteria of theoretical, empirical and heuristic progress (cf. Lakatos 1978a, p. 189). The novelty regarding the *process of problemshift* of research programs can follow on from the third one: the *heuristic* progress. It can be connected with the heuristic power, "the power of a research programme to anticipate theoretically novel facts in its growth" (Lakatos 1970, p. 69, note). The problem however is whether this third direction of novel facts -the methodological novelty- has life by its own in his approach, or does it rather depend on its direct connection with the other two directions (the epistemologico-ontological and the epistemologico-methodological). Before this third possibility is studied, the variety of views on novelty should be considered.

Nowadays, there is available a list of six different kinds of *novelty*. 1) Strictly temporal novelty: the fact was unknown when the theory was proposed; 2) heuristic view of novelty: the fact was not used in the construction of the theory; 3) novelty with respect to background theory (A. Musgrave): the fact was not predicted by the best existing predecessor to the theory; 4) novelty regarding background theory (J. Watkins): a fact which has no counterpart among the consequences of the predecessors to the theory; 5) temporal novelty for the individual: the fact is unknown to the person who constructed the theory at the time the theory is constructed; and 6) novelty with respect to design (i.e., facts the theory was not designated to explain) (Hands 1991b, pp. 96-99 and Backhouse 1997, p. 115).

I think that the correspondence is clear between the positions 1) and 2) and the directions epistemologico-ontological and epistemologico-methodological already pointed out. Moreover, Lakatos recognizes explicitly that he changed his mind under E. Zahar's influence (Zahar 1973, pp. 95-123, and Lakatos and Zahar 1976, pp. 184-185). Initially, the novel facts were those which were never before observed (e.g., the return of Halley's comet, the discovery of Neptune, the Einsteinian bending of light rays...) and which were forbidden by the rival program. After the acceptance of the modification, a novel fact could be a fact already known (such as Mercury's perihelion) which gives empirical support to a new theory (in this case, to Einstein's theory). This fact is not novel temporally speaking: its novelty consists only in its having not been used in the construction of the theory and, therefore, that novelty lies in the lack of methodological use of the knowledge already available.

However, it seems more difficult to identify the third possibility which I mentioned before, because it is more implicit than explicit. It has resemblances with what A. Musgrave has in mind insofar as the novelty of this third case is related to a *background theory* rather than a background knowledge like in kinds 1) and 2). His view, which corresponds to type 3), stresses that the basic question is not so much if evidence *e* confirms a hypothesis *h* but rather if an evidence *e* supports *h'* more than *h*". Thus the point of comparison is not the "background knowledge" in general but rather the old theory which it challenges.

According to this view, a new theory is independently testable (or predicts a 'novel fact') if it predicts something which is not also predicted by its background theory (Musgrave 1974, pp. 15-16).

In other words, following his interpretation, there is a difference in the potential falsifiers: the new theory has some potential falsifiers which are not also valid for the old theory.

Musgrave thinks that his interpretation follows from the problem of *excess of empirical content*, because -for Lakatos- to be independently testable or to have "excess empirical content" over its predecessor supposes that a new theory must predict *novel* facts (i.e., facts improbable in the light of, or even forbidden by, the old theory). This interpretation includes another feature: a "novel fact" defined in that way -as improbable or forbidden- could be a *known* fact. Thus, it assumes that Lakatos does not *explicitly* adopt a strictly temporal view of novelty (Musgrave 1974, p. 16, note), which is wrong because sometimes he does¹³.

If Lakatos's criterion of heuristic progress consist in the acceptance of "problemshifts as 'scientific' only if they are at least theoretically progressive", and progress is measured by "the degree to which the series of theories leads us to the discovery of novel facts" (Lakatos 1970, p. 34)¹⁴, then it could be a *heuristic novelty* when a new theory predicts something which is not also predicted by its background theory. Thus, in the progressive Newtonian problemshift, each successive link "predicts some new fact; each step represents an increase in empirical content: the example constitutes a *consistently progressive theoretical shift*" (Lakatos 1970, p. 48). Does this methodological conception allow for the possibility of avoiding a strictly temporal view of novelty all together? It seems that here it is possible to apply Lakatos's principle of considering "a new interpreted fact as a new fact" (Lakatos 1970, pp. 70-71). Explicitly, he does not demand that "each step produce *immediately* an *observed* new fact" (Lakatos 1970, p. 49).

What Lakatos has in mind therefore when using the expression "novel facts" could be distinguished in three different aspects: a) an epistemologico-ontological feature (the fact was unknown when the theory was proposed); b) a epistemologico-methodological characteristic (the fact was not used in the construction of the theory); and c) a methodological trait (the fact was not predicted by the best existing predecessor to the theory). Both a) and b) appear explicitly in his writings, whereas c) is rather implicit, but it seems quite reasonable to expect that he would be open to the two sources of this third aspect: i) predictions which conflict with the previous theory, and ii) predictions concerning phenomena about which the previous theory says nothing at all (cf. Musgrave 1974, p. 16). The other varieties of "novel facts" (those regarding the consequences, the individuals and the design) do not belong to his methodology of scientific research programs. The prediction of novel facts (unknown, known and heuristically unused) is -for him- an aim, a procedure and a result of science structured in research programs.

3. *The prediction of novel facts as the criterion of appraisal*

Both in his *method* -the methodology of scientific research programs- and in his *meta-method* -the methodology of *historiographical* research programs-¹⁵ Lakatos uses the prediction of novel facts as the criterion of appraisal. In the case of MSRP its role follows three different lines: theoretical, empirical and heuristical. 1) The *theoretical* appraisal is progressive when it shows that the modification of the theories of the program lead to *new* unexpected *predictions* (i.e., each new theory has some excess of empirical content over its predecessor), and the *theoretical* appraisal is degenerative when it only explains the given facts it was intended to explain. 2) The *empirical* appraisal is progressive when some of the novel predictions are *corroborated* (i.e., each new theory leads us to the discovery of a new fact), and it is degenerative when there is no such corroboration of novel facts. 3) The *heuristical* appraisal is progressive when *the problemshift* leads to the discovery of novel facts (i.e., the series of theories of the program show a positive degree in the changes of scientific problems which is at least theoretically progressive), and it is heuristically degenerative when the problemshift remains in the sphere of the accommodation of known facts (cf. Lakatos and Zahar 1976, p. 179; and Lakatos 1970, p. 33-34).

When Lakatos evaluates his own criteria of appraisal, he reaches the conclusion that in one sense his view is more liberal than Popper's appraisal of

theories which in another sense it is more strict¹⁶. It is more tolerant insofar as a research program can have anomalies and grow despite inconsistent foundations and some *ad hoc* movements. And it is more restrictive because it

demands not only that a research programme should successfully predict novel facts, but also that the protective belt of its auxiliary hypotheses should be largely built according to a preconceived unifying idea, laid down in advance in the positive heuristic of the research programme (Lakatos 1974a, p. 149).

The prediction of novel facts is linked to an excess of empirical content of a theory over its predecessors.

These attempts to offer a solid alternative to Popper's methodology and, at the same time, to preserve some falsificationism in a sophisticated version -in research programs as unit of appraisal- have drawn several criticisms. Two of them are relevant here: the first one is the unnecessary reduction of the realm of appraisal; and the second is the overemphasis on the role of prediction. The realm of appraisal could be reduced unnecessarily if the theoretical, empirical and heuristical appraisals rely only on novel facts. In other words, if it is accepted that "the only observational phenomena which have *any* bearing on the assessment of a research programme are those which are 'novel'" (Gardner 1982, p. 1). But, as has been pointed out earlier, known facts are also relevant for Lakatos, not only for explanation but also for prediction. However, Lakatos's stress on novel facts could go beyond what he has in mind, especially when he maintains that "we must require that each step of a research programme be consistently content-increasing" (Lakatos 1970, p. 49).

Is there in Lakatos an overemphasis on the role of prediction? This question leads us to the very controversial issue of whether explanation or prediction provide more evidential support for scientific theories. The controversy can lead to several views: on the one hand, to giving more weight to prediction than to explanation -the predictivist thesis-; and on the other hand, to rejecting that prediction has more weight than explanation and, hence, to the view that matching past or present evidence with hypotheses is good enough: the explanationist view, also known as "accommodation" or "retrodiction" perspective. The "irrelevance" position -it does not matter whether a particular hypothesis happens to be propounded before or after its examination- is usually seen in this second group; but it could be interpreted as an intermediate conception which asserts that the choice is an

open question which depends on several factors and, therefore, there is no a priori preference for prediction or for explanation.

Each camp has some persuasive supporters. In the predictivist camp, besides Lakatos, are other important authors: G. W. Leibniz (1678 [1969, p. 188]), W. Whewell (1860, p. 273), P. Duhem (1914 [1974, p. 28]), K. Popper (1965, pp. 241-243)... Among the critics of the predictivist thesis -although they differ in the emphasis of their criticisms- are some well-known philosophers: J. S. Mill (1843 [1973, p. 500]), C. G. Hempel (1965, p. 10), some Bayesians (Howson and Urbach 1989, p. 97), and those thinkers that choose "understanding" instead of "predicting" within the methodological debate prediction-understanding (cf. Gonzalez 2001b). Their views usually move in favor of the accommodation or retrodiction perspective -to match past or present evidence with hypotheses is good enough- or can lead to the irrelevance position proposed by J. M. Keynes:

the peculiar virtue of prediction or predesignation is altogether imaginary (...) the question as to whether a particular hypothesis happens to be propounded before or after their examination [of its instances] is quite irrelevant (Keynes 1921, p. 305)¹⁷.

Brush is a critic of the general predictivist thesis and adopts this last position. He points out three orientations of philosophy of science where "novel prediction" has played a dominant role in the past two decades: Lakatos's methodology, Bayesian analysis and scientific realism. In the case of the Bayesian approach, he thinks that their supporters have defended

all four possible positions: Bayesian analysis is (i) valid because it favors novel predictions, (ii) valid because it does not favor novel predictions, (iii) invalid because it favors novel predictions, and (iv) invalid because it does not favor novel predictions (Brush 1995, p. 134).

In the case of scientific realism he points out the (no) miracle argument: how can theories of electrons, space-time and DNA "correctly predict observable phenomena if, in reality, there are no electrons, no curved space-time, and no DNA molecules"? If there are no such entities, then it would be a miracle that there to be a succesful prediction about those phenomena (cf. Putnam 1978, pp. 18-19).

Does novelty make a difference in terms of more empirical support? Brush thinks that there are semantical problems: he claims that "what philosophers mean by explanation is just what scientists mean by prediction (sic)" (Brush 1995, p. 135). In addition, he sees historical problems. He considers the following case histories: I) acceptance despite failed predic-

tions; II) rejection despite successful predictions; III) acceptance independent of confirmation of novel predictions; IV) retrodiction counts as much as novel prediction; V) acceptance after novel prediction is confirmed but some problems remain; VI) novelty does count but little; and VII) novelty is crucial. His conclusion is clear: "the predictivist thesis gains little empirical support from the history of science" (Brush 1995, p. 141). However, he grants two methodological reasons in favor of novel prediction:

(a) the publicity generated by a successful novel prediction may lead scientists to *pursue* a theory that they would otherwise ignore; (b) a novel prediction may stimulate experiments designated to test the theory and thus contribute more to the advance of knowledge than a retrodiction (Brush 1995, pp. 135-136).

Therefore, even though he is a critic in historical terms, he recognizes some methodological values in novel predictions.

Two elements could be considered in evaluating Brush's criticism of the predictivist thesis: the realm of research, and the kind of argument. In the first case we have that, even though the methodology of science has a strong link with the history of science, it is not reducible to history of science (and vice versa): the methodology of science includes a prescriptive character (Gonzalez 1990a). Thus, Brush's analysis is useful but it is not the "last word". Moreover, he himself says that "I don't claim that philosophy of science must be validated by history of science" (Brush 1995, p. 133). In the second case, it should be pointed out that the main reason for placing more emphasis on the importance of making predictions is basically the (no) miracle argument. Blaug stresses this factor of methodological evaluation:

a theory that successfully predicts out-of-sample data from sample data is likely to have captured some aspects of objective reality because otherwise its record of predictive success is simply miraculous. Theories may also be simple, elegant, general and fruitful but none of these desirable properties in any way guarantees verisimilitude, that is, nearness to truth about objective world (Blaug 1991, p. 502).

As a methodological criterion of scientific progress and appraisal, prediction of novel facts is important. Compared to accommodation or retrodiction, prediction might have methodological advantages from the point of view of the *process* -it could be more innovative than the other procedure-; and from the perspective of *evaluation*, prediction seems more strict (its success or failure -e.g., in the case of Halley's comet- is determined in a comparatively simpler way). Methodologically, prediction has

more heuristic value than explanation insofar as it innovates and opens more possibilities of research (which seems to be implicitly accepted by some critics of the predictivist thesis, such as Brush). Nevertheless, a successful prediction cannot conclusively prove the theory involved: it is *one* criterion of appraisal but not the only one. However, there is another problem, which arises within Milton Friedman's predictivist thesis (cf. Friedman 1953): the possibility of successful predictions based on false assumptions in the hypotheses. This problem has received special attention in the methodology of economics.

4. *Lakatos's criterion of appraisal and economics*

Some authors have argued that Lakatos's ideas could be used legitimately to defend Friedman's instrumentalism, despite Lakatos's well known opposition to that methodological approach (cf. Klappholz 1991, especially, p. 53). In this regard, it has been emphasized that Spiro Latsis, his main follower in the methodology of economics, "used Lakatos's ideas to defend Friedman's instrumentalism" (Shearmur 1991, p. 40; cf. Latsis 1972, pp. 207-245, especially, pp. 234-242). This assertion is correct insofar as it is made as a depiction of Latsis's views, but it is not fair if it implies that Lakatos himself was an instrumentalist rather than a realist in his methodology of scientific research programs.

Latsis holds that

Friedman's position is vindicated by Lakatos's methodology. False assumptions may be rich in true (and also in false) consequences, and long term progress may be founded on an intuitively false hard core (Latsis 1972, pp. 241-242).

Thus, he interprets the famous "F-twist" -the irrelevance of the absence of realism in the assumptions, which means accepting a descriptively false hypothesis¹⁸- as a mere "provocative formulation of the truism that the hard core of a powerful research programme may consist of counter-intuitive over-simplifications" (Latsis 1972, p. 242). According to this analysis, the weakness of Friedman's approach would be its defensive character and the lack of a clear empirical criterion of progress. Therefore, Latsis accepts, to a large extent, an instrumentalist position -the predictivist thesis of Friedman- which can be completed by filling in the gaps that he finds in that view. For it is the case that an instrumentalist approach could have a non defensive character as well as a clear empirical criterion of progress¹⁹.

But Lakatos did not reject realism and support an instrumentalist methodology based on the primacy of the prediction of novel facts as an aim in and of itself. Indeed, some of the criticisms that he received afterwards (for example, from Laudan) arose because he did not adopt an instrumentalist approach (cf. Gonzalez 1990b, especially, pp. 161-162). He described the "research programs" as connecting the problemshift to new steps for increasing theoretical progress and empirical progress. For Lakatos, each step in the research program requires a consistent content-increase (clearly, it is not a mere search for success in the accuracy of predictions). And the positive heuristic should work to get a chain of ever more complicated models simulating reality, and thus it associates the models with the effort of grasping the reality (cf. Lakatos 1970, pp. 48-50).

It can be argued that Lakatos accepts the presence of inconsistencies in the case of scientific change or even progress based on inconsistent foundations²⁰. Moreover, it might be stressed that he thinks that some of the relevant research programs in the history of science were grafted onto older programs with which they were inconsistent. But it seems clear that, in his approach, scientists tolerate the inconsistencies only temporarily: as the young grafted program strengthens, the supporters of the new program try to replace the old program altogether (cf. Lakatos 1970, pp. 56-57). The reason for this competitive attitude of the champions of the new program is clear: the members of the inconsistent set cannot be all true, and therefore some of them must eventually be replaced.

Imre Lakatos avoids the risks of a predictivist thesis based on the acceptance of false assumptions. Even though he stresses "the 'instrumental' aspect of imaginative research programmes for finding novel facts and for making trustworthy predictions" (Lakatos 1970, p. 100), his framework is a *critical realism* directed towards a well planned building which matches the facts which are to be housed in them²¹. The impact of his conception on economics is clear, although it has suffered ups and downs (cf. Gonzalez 2001a). Here we have seen the problems of his view on prediction in the methodology of research programs and also the oscillations in his perspective on novel facts. His approach continues the predictivist line of Leibniz, Whewell and Popper, and it has a repercussion in the methodology of economics. However, despite the emphasis that Lakatos put on prediction of novel facts as criterion of appraisal, his position was insufficiently developed from the methodological point of view.

Notes

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- 1 This was one of the key issues at the 1994 meeting of the Philosophy of Science Association, cf. Brush (1995) and Achinstein (1995).
 - 2 Prediction is not the main aim of science on Lakatos's view. Truth is the fundamental aim; predictive success is the indication that we may be approaching truth.
 - 3 Lakatos's references are to the reprints in his *Philosophical Papers*, vol. 1 (1978a) and vol. 2 (1978b).
 - 4 There is an inconsistent element here, because if the predicted novel facts are contradicted by previous or rival programs, then they would be already known.
 - 5 Lakatos (1970, p. 32, note). Cf. Lakatos (1970, pp. 69-71 and 76). Cf. Lakatos (1971, p. 114).
 - 6 This a mysterious claim, because *prima facie* the novelty of a factual proposition does not require to be seen after a long period.
 - 7 In addition, Lakatos defends what he calls the "theoretical approach" to the reliability of predictions: "while theories may be said to be supported by evidence, 'predictions' are supported by theories" (Lakatos 1968, p. 192).
For Worrall, what Lakatos seems to have in mind in this statement is related with the issue of (so-called) "theory-ladenness" of observation. In this regard, Worrall considers that ultimately there must be sentences whose truth value we can decide *independently* of theory. (Otherwise the whole scientific enterprise would be without foundation). *Personal communication*, March 2000.
 - 8 Lakatos qualifies once this position once: "I define a research programme as degenerating even if it anticipates novel facts but does so in a patched-up development rather than by a coherent, pre-planned positive heuristic" (Lakatos 1971, p. 112).
 - 9 This "heuristic progress" could be interpreted as an anticipation of his acceptance of the Zahar-Worrall view. It meant explanation/prediction of an old/new fact in a way that "cohered" with the general ideas underlying the program.
 - 10 Another case of heuristic progress is the following: "the progress is not even so much in the actual novel predictions which go beyond the premises -in this case that planets might move also in parabolas- but in the mathematical and physical novelty of problem-solving techniques which later lead to, and form part of, a progressive research programme" (Lakatos 1978c, p. 101).
 - 11 Worrall maintains that Lakatos was confused in this issue insofar as predictions are always 'ontological': they always say that something exists or some effect occurs. Again the stuff about "reinterpretation" is a reflection of the fact that Lakatos was vaguely aware of the correct Zahar-Worrall view [on novel facts] even when officially he was highlighting *temporally* novel facts. Hence he realised that there was no reason why,

e.g., planetary stations and retrogressions should not fully support Copernican theory, despite being known for centuries before Copernicus, and so he talked about Copernicus's "reinterpretation" of stations and retrogressions (he may not actually use that example then, but it captures the point). But -according to Worrall- this confusion is shown to be quite unnecessary once the Zahar-Worrall's view is adopted", *Personal communication*, March 2000.

- 12 Lakatos (1970, p. 34, note). It means that the fact could be an entity discovered later on (e.g., Neptune).
- 13 "Nobody had thought to make such an observation before Einstein's programme. Thus, in a progressive research programme, theory leads to the discovery of hitherto unknown novel facts" (Lakatos 1974b, p. 5).
- 14 Following the Zahar-Worrall view on novel facts, "heuristic progress" can also be understood as some particular shift in theory "cohered" with the heuristic principles underlying the program. If you get increase of empirical content (whether new or old facts) in a way that "cohered" with the heuristic, then you have "heuristic progress".
- 15 "Where Kuhn and Feyerabend see irrational change, I predict that the historical will be able to show that there has been rational change. The methodology of research programmes thus predicts (or, if you wish, 'postdicts') novel historical facts, unexpected in the light of extant (internal and external) historiographies and these predictions will, I hope, be corroborated by historical research. If they are, then the methodology of scientific research programmes will itself constitute a progressive problemshift" (Lakatos 1971, p. 133).
- 16 For Lakatos, "progress is marked by instances verifying excess content rather than by falsifying instances, and 'falsification' and 'rejection' become logically independent" (Lakatos 1974a, p. 150).
- 17 This position is endorsed in Achinstein (1995, p. 163).
- 18 For Latsis, the Chicago School's self defence includes that "even obvious falsity of the hard core in the standard interpretation need not deprive a research programme of its progressive character" (Latsis 1972, p. 241).
- 19 An example of an instrumentalist approach which does not have a defensive character and includes an empirical criterion of progress is Laudan's methodology. This is made clear in his first conception of methodology, which has also a conceptual criterion of progress (cf. Laudan 1977), and it is even more patent in his second methodology, which relies in the concept of "evidence" (cf. Laudan 1996). On his views, cf. Gonzalez (1998).
- 20 He points out that Bohr's program had fast progress on inconsistent foundations, cf. Lakatos (1970, p. 67).
- 21 It is the case that, despite Latsis's interpretation, there is nothing in Lakatos to support a *long-term* instrumentalism (as opposed to a short-term attitude of 'we will eventually produce a theory that at least *might be* "realistic" but give us one breathing-space in the meantime').

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