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# **Neoclassical Economics as a Method of Scientific Research Program : A review of existing literature**

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January 2016

Online at <https://mpra.ub.uni-muenchen.de/75738/>

MPRA Paper No. 75738, posted 22 Dec 2016 06:06 UTC

# Neoclassical Economics as a Method of Scientific Research Program

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## ***Abstract***

This paper aims at a commentary on the Neoclassical Economics as a Method of Scientific Research Program which argues that many theories in Neo-Classical Economics when tested using Lakatos' Methodology of Scientific Research Program, were not categorised as 'progressive research program'. However, some endogenous growth theories have now been tested by few researcher as progressive in terms of Lakatos' Scientific Research Program.

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# Neoclassical Economics as a Method of Scientific Research Program

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## *1. Philosophy of Science and Neoclassical Economics: A brief summary*

Neoclassical economics and physical sciences have many things in common. Both economists and physicists, formulate laws based on their observations. Like physicists, economists rely on mathematics to formalize theories that are often not constrained by experimental evidence. Like physicists, economists reduce complex phenomena to basic units, such as the utility of the rational individual, and then explain the complex phenomena in terms of the interaction and aggregation of the basic units. However, one of the most important similarities is that neoclassical economists frequently justify their discipline, particularly their methods, using theories of scientific method devised by philosophers of science, especially by philosophers of physics. Traditionally, philosophy of science has been closely identified with the philosophy of physics, especially the ontological and epistemological issues in the physical sciences<sup>2</sup>. Moreover, one of the more important issues in traditional philosophy of physics is the demarcation of science from pseudoscience<sup>3</sup>.

Philip Mirowski in his book <sup>4</sup> charts the historical development of economics vis-a-vis that of physics, especially in terms of the reliance of economics on the physical laws of energy conservation. Specifically, he contends that neo classical economists patterned the notion of utility after the notion of energy as it arose in late nineteenth-century physics. The development of physics, then, served as a template for the development of economics as a science. Partially by this means, economists laid claim to scientific status for neoclassical economics. Although dependence on the physical sciences assisted economists initially in founding their discipline, it eventually led to serious problems. Although the perception of neoclassical economics as a science by comparing it with physics is changing among the new generation of economists today, economists have generally relied on various philosophers of science, especially on the philosophers of physics, to defend the scientific status of economics.

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<sup>2</sup> Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Ian Hacking, Cambridge University Press, 1983

<sup>3</sup> Karl Popper, *Science as Falsification, Conjectures and Refutations*, (1963)

<sup>4</sup> Philip Mirowski, *More Heat than Light: Economics as Social Physics, Physics as Nature's Economics*, Cambridge University Press, 1989

Positivism and falsificationism had a profound impact upon economic methodology during the mid-twentieth century. For example, Milton Friedman wrote a widely referred/debated essay on positive economics<sup>5</sup> in the early 1950s. Although he does not cite any philosophers of science in the essay, his analysis of economic methodology mimics the discussion occurring among these philosophers at this period in history. For instance, Friedman claims “positive economics is, or can be, an ‘objective’ science, in precisely the same sense as any of the physical sciences”. This objectivity is made possible, according to Friedman, through the testing of theoretical claims and predictions—a position that weakly but definitely resembles logical positivism/empiricism. But Friedman avoids the problems associated with logical positivism/empiricism by limiting positive economics on two counts. The first is that of the *theoretical claims*, “Logical completeness and consistency are relevant but play a subsidiary role”. The second, and more important, is, “The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria of simplicity and fruitfulness, themselves notions that defy completely objective specification”.

According to Lakatos (1970), scientists gather around a hard core of a research programme that is protected from incidental change, by both *positive* and *negative heuristic belts*. He argued that scientific change is not the result of “instant rationality” (i.e., naïve falsificationism,) but generally of protracted rational negotiations within the professional community. In place of naïve falsificationism, Lakatos substituted a “sophisticated” falsificationism that “combines the best elements of voluntarism, pragmatism and the realist theories of empirical growth”.

Although some economists have attempted to utilize Lakatos’ scientific methodology to defend neoclassical economic method, not much has been achieved. As Hausman(1989) notes, “Apart from philosophical difficulties with their views, Kuhn, Lakatos, and Feyerabend have been hard to apply, for they are evasive on questions of theory appraisal, which still interest most of those writing on economic methodology”<sup>6</sup>. Caldwell (1982) also claims that Kuhn or Lakatos’ methodology may disappoint economists, “who would prefer that

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<sup>5</sup> Milton Friedman, *The Methodology of Positive Economics*, in *Essays In Positive Economics*, Chicago: Univ. of Chicago Press, 1953), pp. 3-43 [https://campus.fsu.edu/bbcswebdav/orgs/econ\\_office\\_org/PowerPoint\\_Files/2023-Joe\\_Calboun/2023\\_Chapter\\_01/Friedman-Essays\\_in\\_Positive\\_Economics.pdf](https://campus.fsu.edu/bbcswebdav/orgs/econ_office_org/PowerPoint_Files/2023-Joe_Calboun/2023_Chapter_01/Friedman-Essays_in_Positive_Economics.pdf)

<sup>6</sup> Daniel M. Hausman , *Economic Methodology in a Nutshell*, The Journal of Economic Perspectives, Vol. 3, No. 2 (Spring, 1989), pp. 115-127

methodology offer a rigorous, objective, prescriptive framework”<sup>7</sup>. Finally, Redman (1991) argues that economists have misapplied Lakatos’ notion of research programme so as to “obscure and clutter economic thought.”<sup>8</sup>

The literature on economic methodology is concerned mainly with questions of theory confirmation or disconfirmation or empirical theory choice. The central question is usually, "How one can tell whether ... economics is good science?" (Hausman, 1989) Economists would like methodologists to provide the algorithm for doing good economic science-and they want the algorithm to vindicate their own practice and to reveal the foolishness of those who do economics differently. For example, Milton Friedman (1953)<sup>9</sup> tells economists that good theories are those that provide correct and useful predictions, while Paul Samuelson (1947, 1963)<sup>10</sup> tells economists to formulate theories with "operational" concepts that are, ideally, logically equivalent to their descriptive consequences.

Before we analyse Neoclassical Economics as a case study for Lakatos’ methodology of *Scientific Research Program*, it is essential we outline some of the basic principles of the concept of Lakatos’ *Scientific Research Programme* in the following section.

## ***2. The Methodology of Scientific Research Programme by Imre Lakatos***

In this section we will try to understand the demarcation criteria in *Lakatos’ methodology* between scientific and pseudo-scientific research programs. The scheme requires a brief exposition of the main elements of Lakatos’ methodology and their interpretation, based on Falsification and the Methodology of Scientific Research Programs.

According to Lakatos’, validation in science involves not individual theories but clusters or interconnected theories which may be called scientific research programmes. A research programme is essentially a sequence of theories within a domain of scientific inquiry. Each later, or successor theory, is held to mark an advance over its predecessor. The move from one theory to its successor within a research programme is called a "problem shift." The question of the rationality

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<sup>7</sup> Bruce J. Caldwell, *Beyond Positivism: Economic Methodology in the Twentieth Century*, Pub: Allen and Unwin, London (1982)

<sup>8</sup> Redman, D. A., *Economics and the Philosophy of Science*, Cambridge: Cambridge University Press.(1991)

<sup>9</sup> Milton Friedman, *The Methodology of Positive Economics*, in *Essays In Positive Economics*, Chicago: Univ. of Chicago Press, 1953), pp. 3-43

<sup>10</sup> Paul A. Samuelson, *Foundations of Economic Analysis*, Harvard University Press 1947(Enlarged ed., 1983)

of changing one's beliefs in science, or how does scientific knowledge "progress" over time, is thus transformed - into the question of asking "When is a problem shift progressive?"

Problem shifts may be "progressive" in two ways: theoretically or empirically. Theoretically progressive problem shifts move towards newer theories that enable us to make predictions that are better than its predecessor. A problem shift is empirically progressive if in addition to predicting new observable evidence, actual observation confirms new predictions. For a research programme as a whole, to be progressive, each problem shift within a research programme must be at least theoretically progressive, and occasionally empirically progressive. In other words, in a progressive programme, each move from an old theory to a new must enable us to predict more; also, at least some of these predictions must also be confirmed. A programme that fails to display these characteristics is called "degenerating." A rational scientist should stick with a progressive programme and abandon a degenerating programme.<sup>11</sup>

In designing new theories to replace the old, the scientist pursuing a research programme adheres to a *constellation of beliefs* (Kuhn, 1962) which Lakatos calls a "heuristic". This heuristic includes both positive and negative aspects.

The negative heuristic specifies certain claims of the research programme as not revisable: "tinkering" with these claims is not permitted as long as one adheres to the programme. They thus rope in a "hard core" which cannot change from one theory to the next. Revising these beliefs is "off limits." It must be noted here, that this premise of Lakatos is analogue to Kuhn's contention that the normal scientist accepts a paradigm "dogmatically."<sup>12</sup>

The positive heuristic represents a body of beliefs which are allied to the hard core as well as suggestions regarding how these beliefs can be revised. These beliefs can be tinkered with; a research programme essentially consists of 'how to reshape these beliefs in the light of potentially refuting observational evidence so as to protect the "hard core" from being refuted'. Thus they form a "protective belt" surrounding the hard core.<sup>13</sup>

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<sup>11</sup>Imre Lakatos, Alan Musgrave,( 1970), *Falsification and the Methodology of Scientific Research Programs*, Criticism and the Growth of Knowledge.

<sup>12</sup> Thomas Kuhn (1962) *The Nature and Necessity of Scientific Revolutions, The Structure of Scientific Revolutions*, University of Chicago Press, 1962

<sup>13</sup> Imre Lakatos, Alan Musgrave (ed),( 1970), *Falsification and the Methodology of Scientific Research Programs*, Criticism and the Growth of Knowledge.

As a research programme progresses, scientists will attempt to refute or falsify the then accepted theory. But when refuting evidence is encountered, according to Lakatos, the scientist will not consider the programme as "refuted." Instead he/she will begin to alter the assumptions of the "protective belt" in ways permitted or suggested by the positive heuristic, such that the "hard core" of the programme can be retained unscathed. As long as such moves enable scientists to predict new phenomena (i.e. it is theoretically progressive), and at least some of those predictions get confirmed by observation/empiricism (i.e., intermittently empirically progressive), the programme is progressing and it is rational to pursue it.<sup>14</sup>

However, when modifications to the theory only protect the hard core from refutation, but do not predict new phenomena, and/or none of those new predictions get confirmed by observation, then the programme is degenerating and the rational scientist abandons it.

To put it simply, Lakatos identified the conditions which must be met in order for it to be heuristically rational to replace an old scientific theory by a new one. (1) The new theory must "predict *novel* facts, that is, facts improbable in the light of, or even forbidden, by" the older one; (2) The new theory must explain "the previous success" of the older one: it must contain "all the unrefuted content" of the latter; (3) "Some of the excess content" of the new theory must be corroborated. If (1) and (2) are satisfied, replacement of the old theory by the new is a "theoretically progressive problemshift." If (3) is also satisfied we have an "empirically progressive problemshift."

Ultimately, the usefulness of any research program is to be judged on its ability to explain an increasing number of hitherto unexplained phenomena.' A "progressive" research program, to borrow the terminology of Lakatos, possesses an expanding empirical content. Theories within a progressive research program are able to explain *novel facts* or regularities that were previously unexplained. Conversely, a "regressive" research program is one whose theories require continuous ad hoc changes in order to shore up the fundamental axioms upon which they are based. The theories in a regressive research program continually confront empirical refutation, and they must be amended accordingly.<sup>15</sup>

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<sup>14</sup> *ibid*

<sup>15</sup> *ibid*

A research programme is a sequence of theories governed by a set of rules or heuristics. Each theory consists of two kinds of statements, those constituting the hard core, and those characterising its protective belt. All theories in the sequence share a common hard core, while each has a more or less different protective belt. The heuristic of the programme consists of a set of rules that govern the movements along the sequence. The negative heuristic is the simple proscription against revising or rejecting the hard core, while the positive heuristic instructs how to modify the protective belt so as to settle tensions between theory and empirical evidence while at the same time protecting the hard core. Hence what we get is a sequence of theories sharing a hard core and governed by positive heuristic for what they do not share, namely the protective belt. <sup>16</sup>

### ***2.1 Latsis' Application of 'Methodology of Scientific Research Programme' in Neo Classical Economics***

In Latsis's application, the models of perfect competition and monopolistic competition belong to the same research programme of situational determinism. They are members in the sequence of theories that share hard core assumptions such as profit maximization, perfect information, and independent decisions by firms. They differ little with regard to their protective belt statements: one assumes homogeneous products, while the other assumes product differentiation. The positive heuristic of the programme consists of the rules guiding the analysis of equilibrium conditions and comparative statics.

On the *Methodology of Scientific Research Programme*, theories are held to be empirically accountable, but empirical evidence is channelled to hit the protective belts only, while leaving the hard core intact. Theory modification is constrained by empirical evidence, but it is also constrained and guided by the tenets of a programme so as to guarantee continuity across theory changes. This is hoped to rule out ad hoc modifications. Yet theory modification is a key idea in the *Methodology of Scientific Research Programme*. Empirical accountability is not a matter of instant fit between theory and the data; it is rather a matter of dynamic empirical performance across the sequence of theories. This is where the concept of progress comes in.

The normative appraisal of a programme is in terms of progress. A progressive programme is one that exhibits both theoretical progress (it yields a novel prediction each time there is a move to another theory along the sequence) and empirical progress (those predictions fail to be contradicted by evidence). A

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<sup>16</sup> *ibid*



degenerating programme is one that fails to exhibit theoretical and empirical progress. In Latsis's application, situational determinism comes out as a degenerating programme, since monopolistic competition fails to yield novel predictions in the absence of required input data.<sup>17</sup>

Normative appraisal is essentially comparative. It is comparative in that theory versions are to be compared across the sequence within a programme. And it is comparative in that a programme being appraised is to be compared to other programmes for its relative progressiveness. In principle, degenerating programmes should be refuted and replaced by progressive programmes. It is well known that Lakatos was unable to solve the difficult problem of setting rules for determining the conditions under which such refutation and replacement should happen. For how long is a research programme permitted to degenerate before being overthrown? The heuristic of a programme would not tell as it is only supposed to govern what happens within the programme. In line with this inconclusiveness in Lakatos's framework, Latsis did not rush to radical conclusions in his application. He granted that the programme of situational determinism may successfully serve as a test bed for the development mathematical techniques, and that it can be used for answering certain questions that *behaviouralism* cannot answer. He was thus unwilling to pass final judgement in appraising these rival programmes. This was surely understandable also due to the young age of the behavioural programme.

In the course of the subsequent years, others published numerous applications of the Lakatosian framework, including those to international trade, general equilibrium, new classical macro, and to schools of economic thought such as Keynesian, Marxian, Austrian, and neo-Ricardian. Few of these studies did what the framework recommended doing, namely comparing rival research programmes for their relative progressiveness.

In the next section we will discuss the conceptual foundations of Neoclassical economics as a case study of Lakatos' Scientific Research Programme and outline the famous Spiro Latsis-Milton Friedman letters/ debate.

### ***3. Milton Friedman and Neoclassical Economics: A case study of Scientific Research Programme***

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<sup>17</sup> Spiro Latsis, *Situational Determinism in Economics*, The British Journal for the Philosophy of science, pp 207-245, 1972. <http://www.jstor.org/discover/10.2307/686686?uid=3738256&uid=2&uid=4&sid=21103845472177>

In 1972, Spiro Latsis published a case study titled 'Situational Determinism in Economics' in 'The British Journal for the Philosophy of Science'. The paper put forwards the methodology of neoclassical economics as a case study of *Scientific Research programme* (Lakatos, 1970). It accomplished Milton Friedman's methodology as 'pseudo-scientific' in terms of Lakatos's evaluative philosophy of science. According to Lakatos's methodology, the demarcation between scientific and pseudo-scientific theories consists of their ability to predict, testable empirical facts. Latsis claimed that Friedman's methodology of neoclassical economics had failed this criterion of Lakatos's Scientific Research programme and hence cannot be demarcated as scientific.<sup>18</sup> According to Latsis, Friedman's methodology was what Lakatos termed as 'degenerating'. He also put forth a program of "economic behaviorism,"<sup>19</sup> and went on to organize a conference around methodology in economic research programs and his criticisms have sparked an ongoing debate over the nature of economic research (Latsis, 1972).

According to situational determinism, the situation in which an agent finds itself determines its behaviour. The inner workings of the agent do not matter, as the external situation completely constrains its behaviour. The agent's situation is construed so as to leave it with no choice: in a single-exit situation, there is just one way to go, so there is no room for genuine choice, decision-making, and entrepreneurship. Behaviour becomes a matter of reaction instead of action.

Latsis argued that situational determinism is a degenerating research programme. The model of monopolistic competition as a successor of perfect competition within the same programme fails to make theoretical progress even in the sense of generating novel predictions. Therefore, a case can be made against situational determinism and for an alternative research programme such as economic behaviouralism. Unlike situational determinism, economic behaviouralism pays serious attention to the psychological and organisational details of the inner functioning of economic agents. On this programme, behaviour is not determined by the situation alone, but the inner environment of agents becomes relevant, and behaviour becomes a matter of genuine decision-making in multiple-exit situations. While the programme of situational determinism is autonomous with respect to psychology and organisation theory, economic behaviouralism is dependent on contributions from these fields of inquiry.<sup>20</sup>

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<sup>18</sup> *ibid*

<sup>19</sup> *ibid*

<sup>20</sup> *ibid*

To quote Latsis,

“In Friedman's case, one would have to explain why the market behaves as if firms wanted to maximise profit etc. These unsolved problems of reduction of the hard core to 'more realistic' ones do not prevent either the theoretical or the empirical progress of the programme. Empirical falsifications coming from 'indirect' tests do not harm progress either. Anomalies, unsolved puzzles can always abound: they only provide economists with work in elaborating the protective belt. So far 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. The trouble comes only when we appraise the theoretical and empirical progress of the programme: the adhocness or non-adhocness of the adjustments in the ever more complicated protective belt. This is, as I tried to show, where the neoclassical programme-after a period of initial progress-started failing. Friedman never seriously analysed the theoretical adjustments made by the neo-classical school (which he openly defends) for adhocness.

Thus the Friedman-Machlup methodology can easily be characterised as an attempt to defend a research programme against utopian standards of falsificationism. Their only major slip in these defensive manoeuvres was Friedman's much discussed thesis-christened by Samuelson the 'F-twist'-that 'to be important... a hypothesis must be descriptively false in its assumptions'. This was nothing more than a blunt and provocative formulation of the truism that the hard core of a powerful research programme may consist of counter-intuitive over-simplifications. The real weakness of this methodology is its purely defensive character and its lack of a clear empirical criterion of progress.” (Latsis, 1972)<sup>21</sup>

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<sup>21</sup> *ibid*

### ***3.1 Friedman's Defence***

In defence, Friedman wrote a 17 page letter to Latsis in December 1972 and wrote he has been “talking *Progress* all the time”<sup>22</sup>. He counter-claimed that the neoclassical monopoly competition model had in fact shown empirical progress by predicting phenomena not previously observed and were subsequently confirmed by empirical evidence.<sup>23</sup> The example he gave was a prediction of Chamberlain's monopolistic competition model that "the standard explanation for the Standard Oil monopoly was wrong", which he added had been theoretically predicted by Aaron Director and empirically confirmed by John S. Magee paper ‘Predatory Price Cutting: The Standard Oil (N. J.) Case’<sup>24</sup>. In particular, Friedman cited the paper that was specifically intended to prove Dr. Director's thesis that monopolists always prefer merger to predatory pricing. The paper is widely believed to prove that the US government's 1911 breakup of the Standard Oil trust as a monopolistic combination hurt rather than benefited consumers because prices of oil products were actually lower under the efficiencies the trust created.

Later, Lakatos invited Friedman to submit a discussion note based on his December 1972 letter to Latsis for publication in a symposium on the issue of the scientific status or not of neoclassical economics, but Friedman never took up the invitation.<sup>25</sup>

### ***3.2 Empirical Findings for and against Friedman's conclusion***

In 1996 Elizabeth Granitz and Benjamin Klein uncovered how Standard Oil used its dominant position in refining to sell refined oil at a monopoly price, and purchase crude at a monopoly price. Magee's revisionist analysis was heavily criticized, for reasons that demonstrate Lakatos' critique was accurate. Christopher Leslie (2013)<sup>26</sup> showed that Magee's claim that Standard Oil was not priced predatorily was false, and that Magee misread, misinterpreted, and ignored evidence. Economists James Dalton and Louis Esposito reexamined the trial record, and found it “contains considerable evidence of predatory pricing. Simply stated, the record does not support Magee's conclusion ...” Magee's

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<sup>22</sup> Imre Lakatos and Paul Feyerabend, *For and against Method: Including Lakatos's Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence*, Edited by Matteo Motterlini, University Of Chicago Press, 1999

<sup>23</sup> *ibid*

<sup>24</sup> John S. McGee, Predatory Price Cutting: The Standard Oil (N. J.) Case, *Journal of Law and Economics*, Vol. 1. (Oct., 1958), pp. 137-169

<sup>25</sup> Imre Lakatos and Paul Feyerabend, *For and against Method: Including Lakatos's Lectures on Scientific Method and the Lakatos-Feyerabend Correspondence*, Edited by Matteo Motterlini, University Of Chicago Press, 1999

<sup>26</sup> Christopher R. Leslie, Predatory Pricing And Recoupment, *Columbia Law Review*, 2013

*falsification* of Standard's predatory pricing accusation was based on it being "logically deficient." Friedman's claim that Magee "empirically confirmed" Aaron Director's "theory" was wrong. Magee's test was logical, not empirical: Standards cannot have predatory price.

Three years later, in 1976, Friedman was awarded Nobel Prize in Economics for his achievements in the fields of consumption analysis, monetary history and theory and for his demonstration of the complexity of stabilization policy. Friedman's own predictions of an accelerating rate of inflation due to attempts to use expansionary monetary policy in order to attain an unrealistic employment target, as described in his Nobel lecture are cited by others as an example of *a novel phenomenon successfully predicted by neoclassical economics*. This research ultimately led to a breakdown of the popular belief in economics in the mid-20th century that there was a long-run trade-off between unemployment and inflation. Robert E. Lucas(1981)<sup>27</sup> argued that the Friedman-Phelps model was "as clear cut an experimental distinction as macroeconomics is ever likely to see". Roger Backhouse<sup>28</sup> argued that Friedman and Phelps had predicted *novel facts* that were corroborated by the events of the 1970s. Mark Blaug(1992)<sup>29</sup> argued that Friedman's 1968 paper and its successful prediction of *novel facts* was itself a proof that Friedman's monetarist, neoclassical research programme was a *progressive research programme*.

But recent studies throw some question on the impact of money supply on 1970's inflation, on both theoretical and empirical grounds. Theoretically, money supply is difficult to identify and foresee, as it is based on vast, disaggregated financial data. Something unperceived cannot be used in "rational decisions." Empirically, it has been observed that the 1970s oil shocks, output gap, and productivity deceleration are more statistically significant than money supply, in explaining the era's inflation. For example, a paper by Katrin Assenmacher-Wesche and Stefan Gerlach (2006)<sup>30</sup> that uses data from 1970 to 2003 for Euro zone, observed the cost push shocks, in particular import prices and output gaps play more significant role in determining inflation compared to quantity-theoretic variables like money supply, output growth and velocity.<sup>31</sup>

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<sup>27</sup> Lucas, Robert, "Tobin and Monetarism: a Review Article," Journal of Economic Literature, 29 (June 1981), 558-585.

<sup>28</sup> Roger E. Backhouse, *Interpreting Macroeconomics: Explorations in the History of Macroeconomic thought*, Publisher: Routledge, 1996.

<sup>29</sup> Mark Blaug, *The Methodology of Economics: Or, How Economists Explain*, Series: Cambridge Surveys of Economic Literature, 1992

<sup>30</sup> Katrin Assenmacher-Wesche and Stefan Gerlach, *Understanding the Link between Money Growth and Inflation in the Euro Area*, Center for Economic Policy Research <http://stefangerlach.com/CEPR-DP5683.pdf>

<sup>31</sup> *ibid*

In Lawrence Boland's piece, "A Critique of Friedman's Critics" (1979)<sup>32</sup>, Boland argues that Friedman's methodology is best understood as a variant of the philosophical position known as instrumentalism, and that if Friedman is so interpreted; many critiques of his position existent in the economic literature miss their mark.

### ***3.3 Other Criticisms on Friedman's methodological instrumentalism***

Caldwell (1980) in his paper, "A Critique of Friedman's methodological instrumentalism"<sup>33</sup> attacks Friedman's essay (1953)<sup>34</sup> on some of the following grounds.

#### ***3.3.1 Friedman postulates: The goal of science is to discover hypothesis that predict well.***

Cadwell(1980)<sup>35</sup> argues that philosophers of science since 1940's have been unanimously rejecting the notion that only goal of science is prediction. He adds "once one takes the position that explanation is the goal of science, the instrumentalist view of theories and theoretical terms is considerably weakened. If science seeks theories that have explanatory as well as predictive power, then theories that merely predict well may not be satisfactory, and the view that theories are nothing than instruments must be rejected."

#### ***3.3.2 Friedman believes: Assumptions are not a locus of testing for theories, their realism does not matter.***

Much of the debates on Friedman's position are due to this proposition. Caldwell questions the use of the term 'Realism'. He says 'Realism' is often confused with concepts such as testable, confirmation and truth. Theories may be untestable and may seem unrealistic (without any evidence), yet true. Similarly, a theory could be testable, realistic, highly confirmed yet false. According to Caldwell, use of these terms (like testable, realistic, highly

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<sup>32</sup> Lawrence A. Boland, *A Critique of Friedman's Critics* Journal of Economic Literature, American Economic Association, vol. 17(2), pages 503-22, 1979

<sup>33</sup> Caldwell, *A Critique of Friedman's methodological instrumentalism*, Southern Economic Journal, Vol.47, No.2, PP 366-374 (1980)

<sup>34</sup> Milton Friedman, *The Methodology of Positive Economics*, in *Essays In Positive Economics*, Chicago: Univ. of Chicago Press, 1953), pp. 3-43

[https://campus.fsu.edu/bbcswebdav/orgs/econ\\_office\\_org/PowerPoint\\_Files/2023-Joe\\_Calhoun/2023\\_Chapter\\_01/Friedman-Essays\\_in\\_Positive\\_Economics.pdf](https://campus.fsu.edu/bbcswebdav/orgs/econ_office_org/PowerPoint_Files/2023-Joe_Calhoun/2023_Chapter_01/Friedman-Essays_in_Positive_Economics.pdf)

<sup>35</sup> Caldwell, *A Critique of Friedman's methodological instrumentalism*, Southern Economic Journal, Vol.47, No.2, PP 366-374 (1980)

confirmed, truth) interchangeably has rendered Friedman's position all the more confusing.

### *3.4 Hands' (2001) Criticism of Neo Classical Economists for borrowing Popper-Kuhn-Lakatos' methodology*

Hands(2001) writes, economists must develop their own metaphors and methodology to advance and justify economics as a science. There are certainly ample differences between economics and physics and sufficiently unique issues in economics and its practice such that economists can develop an economic methodology apart from the neoclassical economic methodology that depends on the philosophy of physics. Hands(2001) challenges economists to develop a foundation for economic methodology not grounded on the methodology of logical positivism/empiricism or Popperianism or even on the methodology of Kuhn or Lakatos but on a more pluralistic postmodern approach to science.

## ***4. Recent use of Lakatos' Methodology of Scientific Research Programme in evaluating Neo Classical Economic theories and Endogenous Growth models***

Many of the recent work done by various researchers on this area have applied Lakatos' Methodology of Scientific Research to test the 'progressiveness' of Endogenous Growth theory.

Mario Pomini (2012)<sup>36</sup> studies the emergence of endogenous growth theory from the point of view of Lakatosian categories. It uses the Methodology of Scientific Research Programs proposed by Lakatos (1970) in order to explain why the endogenous growth approach was not incorporated into the neo-classical growth program until the late 1980s, although the essential features were well known during the 1960s. The thesis which results is that the new growth theory may be seen in terms of an extension of the neo-classical research programme to incorporate theoretical elements which previously fell beyond its scope. Pomini says even if the MSRP is not without its critics among economic methodologists it remains a useful framework within which to analyse the evolution of economic

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<sup>36</sup> Mario Pomini, *The Neoclassical Endogenous Growth Theory on Retrospect: a Lakatosian Interpretation*, Studi e Note di Economia, Anno XVII, n. 2-2012, pp. 249-276

ideas (for example, Hands 2001<sup>37</sup>). He concludes that the new (endogenous) growth theory may be seen in terms of an extension of the neo classical research programme in the sense of Lakatos to incorporate theoretical elements which previously fell beyond its scope.

Another paper by Michal Brzezinski and & Michal Dzielinska,(2009)<sup>38</sup> suggests that by Lakatos's standards, Schumpeterian variant of endogenous growth theory is both theoretically and empirically progressive over neoclassical growth theory. They criticize Cavusoglu and Tebaldi's (2006) paper on Lakatosian appraisal of growth theories on three grounds. First, they hold that Cavusoglu and Tebaldi do not provide a proper structure of theory comparison in their contribution. Second, they argue that Cavusoglu and Tebaldi use an inadequate version of Lakatos's appraisal criterion. Third, they show that there are seminal endogenous growth models, which predict income convergence among countries.

Sandra Silva's( 2009)<sup>39</sup> paper proposes a reflection on evolutionary technological change and economic growth theory, which starts from the Lakatosian methodology of scientific research programmes (MSRP) as an appraisal criterion. It analyses the confrontation of these evolutionary theories with what can be seen as their 'rival research programme', the new neoclassical growth models.

#### **4. *Some Criticisms of Lakatos' Methodology of Scientific Research Programme***<sup>40</sup>

While Lakatos' Methodology of Scientific Research Programme was touted by his followers as an important criteria of 'demarcation between scientific and pseudo-scientific theories, it also created some of the major debates among philosophers and neo classical economists at that time. It may be noted that the Lakatos' Methodology was not free of pitfalls. We list some of the following criticisms against Lakatos Methodology of Scientific Research Programme (Maki, 2008)<sup>41</sup>:

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<sup>37</sup> Hands, D.W, *Reflection without Rules*, Cambridge (UK), CambridgeUniversity Press. (2001),

<sup>38</sup> Michal Brzezinski and & Michal Dzielinska, *Is endogenous growth theory degenerating? Another look at Lakatosian appraisal of growth theories* Journal of Economic Methodology, vol. 16, issue 3, pages 243-263, 2009

<sup>39</sup>Sandra Silva, *On evolutionary technological change and economic growth: Lakatos as a starting point for appraisal*, Journal of Evolutionary Economics, Springer, vol. 19(1), pages 111-135,2009

<sup>40</sup> Uskali Maki, *Method and appraisal in Economics*, 1976-2006. Journal of Economic Methodology, vol. 15, issue 4, pages 409-423, 2008

<sup>41</sup> *ibid*



- i. There is no easily identifiable stable hard core and positive heuristics in economic theorizing.
- ii. There is no active generation of novel predictions in economics.
- iii. There is no room in the Methodology of Scientific Research Programme for straightforward inductive support by empirical evidence.
- iv. There is no systematic role in the Methodology of Scientific Research Programme for social institutions and their history.
- v. There is no systematic account in the Methodology of Scientific Research Programme of how progress in terms of novel predictions connects with the goal of approaching truths about the real world.

Lakatos structured his methodology by examining the history of physical sciences throughout the last three hundred years. Therefore, all judgments are characterised against programmes of physics. That is, an assumption prevails without argument that all disciplines must possess the characteristics of physics to qualify as scientific. Of course, areas of study such as Marxism and astrology would therefore be seen as unscientific because they don't conform to physical principles. Obviously people and societies cannot be examined in the same way one examines a scientific phenomenon; the complexity of living things is such that even biology exhibits important differences to that of physics. Moreover, theories held within the social sciences effect how people function in society.

## **6. Conclusion**

So we see that there are many theories in Neo Classical Economics which, when tested by various philosophers using Lakatos' Methodology of Scientific Research Program, were not able to categorize them as progressive research program. Though many of these theories later "predict novel facts, that is facts improbable in the light of, or even forbidden, by the older one" (Lakatos,1970). Some endogenous growth theories have now been tested by few researcher as *progressive* in terms of Lakatos' Scientific Research Program. We must not forget that Lakatos' Scientific Research Program too, had its own shortcomings and it cannot be considered a gospel, when it comes to testing the 'progressiveness' of a theory.

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