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On

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The Methodology of Economic Research Programmes

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Recently the discipline of economics has been drawn into the controversy surrounding the late Imre Lakatos' work in the history and philosophy of science. The publication which will be examined here[1] has been followed by other works of the same genre, and the trend will most certainly continue.[2] Work in this area is being animated by both the economics profession and those involved in the general history and philosophy of science.

The rise of 'Growth of Knowledge' theories[3] and the resuscitation of communication between historians and philosophers of science has initiated an attempt to broaden the base of metascientific thought. From the economic camp there has been a revival of interest in methodological concerns initiated at least in part by the general disharmony within the discipline which has recently surfaced for the first time since the Keynesian revolution.[4] Alternative scientific methodologies provide economists with alternative vehicles for appraising the history of their field and offer the potential for a 'deeper' reconciliation of current controversies. For philosophy of science economics provides both a simple 'testing ground' and also an opportunity to explore a 'rationality' in many ways different from their traditional domain of natural science.

1 Latsis (1976*a*).
2 Blaug (1967*b*) and O'Brian (1976).
3 Broadly, this covers recent work such as that which has been produced by Feyerabend, Kuhn, Lakatos and Toulmin, but also including parts of more traditional thought such as Popper.
4 The use of the term 'revolution' is not intended to 'bias' the discussion, since an important part of the work being discussed concerns the degree to which Keynesian economics constituted a 'revolution'. See Blaug (1976*a*), Leijonhufvud (1976).

Method and Appraisal in Economics consists of a number of papers, most by economists, which grew out of the Nafplion Colloquium on Research Programmes in Physics and Economics held in Nafplion, Greece, in September 1974. Some of the papers in the volume offer general criticism of the application of Lakatosian methodology, some general praise, and still others offer specific case studies in the history of economics. The volume as a whole is delightful, and it will rightfully serve as an important point of departure for the great amount of work in this area which will certainly follow.

This paper will not attempt to summarize the general arguments of Lakatos' Methodology of Scientific Research Programmes (MSRP), since there are many summaries available.[5] Nor will it summarize the various contributions to this volume or undertake a detailed criticism of an individual essay. Instead, it will offer a broad criticism which is applicable to most of the contributions. It will argue that there is an important part of Lakatos' work, which needs to be emphasized in order properly to 'apply' the MSRP, which has not been emphasized in this volume. It will be demonstrated that this aspect of MSRP is a result of its Popperian antecedency. Failure to underscore this aspect can allow perspectives on the relationship of the history of a scientific discipline to a methodology of science to prevail which are inconsistent with Lakatos' central argument. In other words, Lakatos' work has necessary implications which are either disregarded or not fully understood by the majority of these authors. This leads to inconsistencies and delimits the value of this work in a general evaluation of the MSRP.

The result of this lacuna is that advocates of Lakatosian methodology are found to take very non-Lakatosian views on the relation between history and philosophy of science, and critics of Lakatos who advocate a more 'Popperian' methodology are skeptical of Lakatos in precisely the areas where his work is a refinement of Popper's.

This paper will proceed by first pointing out an important 'conventionalist'[6] aspect of Popper's work, and then demonstrating that the MSRP is 'Popperian' in this 'conventionalist' sense also. It will then attempt to show that this aspect is 'essential', rather than adventitious in appraisal of Lakatos' programme. Finally, it will argue that Lakatos' view has implications related to this 'conventionalist' component not recognized by the majority of the contributors to *Method and Appraisal*.

THE RISE OF DOGMATIC FALSIFICATIONISM

As Lakatos makes very clear, the tradition prior to the early part of the twentieth century was that 'knowledge meant proven knowledge'.[7] Good scientific method was a way of extending that proven knowledge. Empirical Justification (the historically dominant view in philosophy of science) argued that acceptable scientific knowledge must be 'proven'; either proven 'facts' or statements logically derived from these proven 'factual propositions'. As Lakatos argues:

5 Lakatos (1970*b*) and Lakatos (1971*a*) are of course the best sources. With a caveat regarding the issues discussed below; Blaug (1976*a*), Leijonhufvud (1976), and Latsis (1976*b*) present laconic presentations of the terms and basic structure of the MSRP for those desiring a more cursory introduction.

6 The meaning of 'conventionalist' in this context will become apparent as the discussion proceeds.

7 Lakatos (1970*b*), p. 91.

Classical empiricists accepted as axioms only a relatively small set of 'factual propositions' which expressed the 'hard facts'. Their truth-value was established by experience and they constituted the empirical basis of science.[8]

Since deductive logic allows only nonampliative inferences it was necessary to develop an 'inductive logic' which would transmit truth from 'factual propositions' to 'general laws'. Thereby the scope of proven knowledge was extended from the empirical basis by truth-preserving ampliative inferences.[9] The recognition of the Humeian 'problem of induction' eventually overthrew the belief that such inductive logic could be found.[10]

The attempt to reconcile the empirical basis of proven knowledge with the problem of induction resulted in dogmatic or 'naturalistic' falsificationism. The argument was that while science could not prove, it could conclusively disprove. Armed with the logic of *modus tollens* and the empirical basis, dogmatic falsificationism set out to extend the body of proven knowledge by a process of elimination. Theories are fallible; once they fail, they cease to be contenders of knowledge. Lakatos characterizes dogmatic falsificationism:

Dogmatic falsificationism admits the fallibility of all scientific theories without qualification, but it retains a sort of infallible empirical basis. It is strictly empiricist without being inductivist: it denies that the certainty of the empirical basis can be transmitted to theories.[11]

According to the logic of dogmatic falsificationism, science grows by repeated overthrow of theories with the help of hard facts.[12]

There are at least two criticisms of this methodology. The first is a problem stemming from its empiricist foundations which led Popper to develop a more sophisticated version of falsificationism. The second conundrum is sometimes referred to as the 'Duhemian Problem' and applied to all forms of falsificationism, dogmatic as well as more sophisticated versions.[13] It is, in some respects, this problem which led Lakatos to abandon 'falsificationism' altogether and develop the MSRP. For the purpose of this discussion this second problem can be disregarded since it is technically absent from sophisticated falsificationism and the MSRP.[14] The problem of the empirical basis though, needs to be examined in some depth for a proper understanding of Lakatos' work and its place in the Popperian tradition.

THE PROBLEM OF THE EMPIRICAL BASIS

The most blatant problem with dogmatic falsificationism, and the one which Popper sought to eliminate in his more refined falsificationism, is the problem of

8 Lakatos (1970*b*), p. 94.
9 Terminology due to Salmon (1966), Ch. 1.
10 This of course disregards the voluminous works in the probabilist programme, which are very important to an overall sketch of the philosophy of science, but not particularly interesting to the issue presently at hand.
11 Lakatos (1970*b*), p. 96.
12 Lakatos (1970*b*), p. 97.
13 See Laudan (1965), for a brief but clear explanation of the history of the Duhemian problem.
14 This problem is not germane to the arguments of this paper but it is an interesting issue since it involves the problems of falsification with a ceteris paribus clause, which plays a paramount role in economic theory.

the empirical basis. The difficulty is that 'facts' are 'theory-laden'. The process of obtaining 'knowledge' of the empirical basis is a process which involves a contribution of the observer to the observed. Observations are not something we 'have' but something we 'make'.[15] Conflicts between a scientific theory and data do not constitute conflicts between theory and nature, but inconsistency between a theory and an observation theory. There is no guarantee that the observation theory is correct: there is no guarantee that the 'facts' correspond to the facts.[16] The point is well put in a recent philosophy of science introductory text:

> Descriptions of observations cannot be entirely independent of theory either in form or in content. There are no modes of description which remain invariant under all changes of theory. The way in which observations are described changes where theory changes. The accepted way of explaining phenomena enters into the very meaning of the terms used to describe them. It seems to be generally agreed among philosophers, now, that the ideal of a descriptive vocabulary which is applicable to observations, but which is entirely innocent of theoretical influences, is unrealizable.[17]

Lakatos puts it similarly:

> In particular, for classical empiricists the right mind is a tabula rasa, emptied of all original content, freed from all prejudice of theory. But it transpires from the work of Kant and Popper—and from the work of psychologists influenced by them—that such empiricist psychotherapy can never succeed. For there are and can be no sensations unimpregnated by expectations and therefore there is no natural (i.e., psychological) demarcation between observational and theoretical propositions.[18]

'CONVENTIONALIST'[19] FALSIFICATIONISM

Popper's 'falsificationist' philosophy of science does not initiate from the empiricist's empirical basis. Instead, Popper proposes a 'revolutionary conventionalism'[20] where 'observation statements' are accepted only by decision. The decision to accept certain 'basic statements' as potential falsifiers results from the realization that all theories are fallible and that there are fallible theories involved in 'observation'. These 'basic statements' are accepted by 'convention' as part of unproblematic background knowledge,[21] for the purpose of testing the theory at hand; 'he may call these theories ... "observational": but this is only a manner of speech which he inherited from naturalistic falsificationism'.[22] Popper thus substitutes an 'empirical basis' for the empiricist's empirical basis. Popper's own statement of this is:[23]

15 Popper (1972), p. 342.
16 The use of inverted commas to separate that which is 'observational' by decision as opposed to that which observational in the sense of classical empiricism follows Lakatos (1970*b*), pp. 98, 106; and Popper (1963, p. 387.
17 Harré (1972), p. 25.
18 Lakatos (1970*b*), p. 99. For an example of how far rejection of classical empiricism can go, and numerous examples of the 'theoretical' nature of data, see Feyerabend (1975*a*).
19 This includes both 'naive' and 'sophisticated' versions—but the distinction is not germane here.
20 Lakatos (1970*b*), p. 106.
21 Lakatos (1970*b*), p. 106.
22 Lakatos (1970*b*), pp. 106-07.
23 My apologies to the reader for the extreme length of this quote, but considering disagreement on this point (at least among economists) I felt it was necessary.

In introducing the term 'empirical basis' my intention was, partly, to give an ironical emphasis to my thesis that the empirical basis of our theories is far from firm; that it should be compared to a swamp rather than solid ground.

Empiricists usually believed that the empirical basis consisted of absolutely 'given' perceptions or observations, of 'data', and that science could build on these data as if on rock. In opposition, I pointed out that the apparent 'data' of experience were always interpretations in the light of theories, and therefore affected by the hypothetical or conjectural character of all theories.

. . . the process of interpretation is at least partly physiological, so that there are never any uninterpreted data experienced by us: the existence of these uninterpreted 'data' is therefore a theory, not a fact of experience, and least of all an ultimate or 'basic' fact.

Thus there is no uninterpreted empirical basis; and the statements which form the empirical basis cannot be statements expressing uninterpreted 'data' (since no such data exist) but are, simply, statements which state observable simple facts about our physical environment. They are, of course, facts interpreted in the light of theories: they are soaked in theory, as it were.[24]

Thus it can be seen that Popper does not advocate dogmatic falsificationism—that he 'separates rejection and disproof, which the dogmatic falsificationist had conflated'.[25] The decision to 'falsify' is a 'decision'—based on the simple rule which says that when a theory-laden observation is inconsistent with a scientific theory—relegate the observation to unproblematic background knowledge and reject the scientific theory.

Popper does not follow the simple decision rule stated above in his writings on 'sophisticated' falsificationism, only in its 'naive' form. In the sophisticated form Popper develops techniques (based on adhocness) whereby a theory may be protected from falsification by a single conflict between theory and 'observation'. But this sophisticated falsificationism does not eliminate the 'conventional element' in the appraisal of scientific theory, it only reduces it. 'We cannot avoid the decision which sort of propositions should be the "observational" and which the "theoretical" ones'.[26]

It is common practice in elementary delineations of Popper's 'falsificationism' to stress his concern for the method of falsification rather than confirmation—his 'solution' to the problem of induction—and not pursue his epistemology in any depth. This type of cursory examination leads one to believe that Popper's work is adequately represented by what Lakatos calls Popper_0.[27] Popper $_0$ is 'the dogmatic falsificationist who never published a work', as contrasted to Popper_1 the naive falsificationist and 'Popper_2 the sophisticated falsificationist'.[28] Lakatos argues that, 'the real Popper consists of Popper_1, together with some elements of Popper_2.[29] The issue of concern here is not the somewhat ambiguous demarcation of Popper_1 and Popper_2. The issue is that Karl Popper is unambiguously not a dogmatic falsificationist, and never has been.[30]

The MSRP is a scion of Popper's sophisticated falsificationism. It rests on conventionalist decisions of the 'empirical basis' to the same extent that Pop-

24 Popper (1963), p. 387.
25 Lakatos (1970*b*), p. 109.
26 Lakatos (1970*b*), p. 127.
27 Lakatos (1970*b*), p. 181.
28 Ibid.
29 Ibid.
30 The importance of emphasizing what someone did not say as opposed to exegetical discussion of what they 'really meant' is pointed out by Leijonhufvud (1976), p. 85, in regard to his work on the Keynesian revolution.

per's does. 'As it stands, like Popper's methodological falsificationism, it represents a very radical version of conventionalism.'[31]

It is not necessary to delineate the central argument of the MSRP or to define all Lakatos' terms since many summary presentations are available, as argued before.[32] The next step will be an examination of the treatment of this 'conventionalist' element in Popper and Lakatos' work by the contributors to *Method and Appraisal*.

CONVENTION AND APPRAISAL

Most of the contributors to *Method and Appraisal* fail to emphasize this 'conventionalist' aspect of Popper and Lakatos' work.[33] In explaining Popper's work Blaug states:

> He repudiated the Vienna Circle's principle of verifiability and replaced it by the principle of falsifiability as the universal, a priori test of a genuinely scientific hypothesis. The shift of emphasis from verification to falsification is not as innocent as appears at first glance, involving as it does fundamental asymmetry between proof and disproof.[34]

Hutchison and Hicks point to problems of applying Popper's 'naturalist' falsification to economics where empirical data is not available (implying that there is no problem of the empirical basis in physics).[35] Latsis contrasts conventionalism and falsificationism not mentioning the 'conventionalist' element in Popper's work, yet footnoting Popper when explaining falsificationism.[36]

It is worth noting that this dogmatic characterization by economists is not only done by those critical of Popper's methodology; if it were, the argument could be made that Popper_0 is a 'strawman' Popper constructed solely for ease of reprobation. This is decidedly not the case. Those advocating a strict 'Popperian' methodology in economics such as Hutchison argue for the position of dogmatic falsificationism. Hutchison mentions Popper's 'Fallibilism', but it is the fallibility of scientific theories—and therefore the need for a falsificationist rather than confirmationist methodology—which is alluded to; not the fallibility of factual propositions as argued in naive or sophisticated falsificationism.[37]

Throughout the volume falsificationism, usually attributed to Popper, is characterized as a conflict between hard facts and theory. This not only presents Popper and Lakatos as somewhat more philosophically naive than is legitimate, it also leads to misinterpretations of the interanimation of the history of economics and the MSRP.[38]

31 Lakatos (1971*a*), p. 101.

32 See note 5 above.

33 It is immaterial whether this results from a less than precise reading of Popper and Lakatos, or whether the distinction was recognized but not considered germane to their arguments. Regardless of the cuase, the effect is to prejudice the analysis of Lakatos in significant ways.

It is the economic 'conventional wisdom' to view 'falsifying evidence' in a 'dogmatic' way, and also to attribute this view to Popper via Hutchison or incorrectly Friedman (1953). Therefore, the burden of proof is on the authors of this volume to clearly show that they do not follow this erroneous tradition. In this way, 'failure to emphasize' represents an indictment which extends to those not specifically stating in which sense 'falsification' is being used.

34 Blaug (1976*a*), p. 151.

35 Hicks (1976), p. 207; Hutchison (1976), pp. 181, 187.

36 Latsis (1976*b*), p. 14.

37 Hutchison (1976), p. 203.

38 Those familiar with Lipsey and Steiner's introductory text, *Economics* (4th ed.), will be amused by Lakatos' choice of example to demonstrate how Popperians advocate a

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Lakatos' conventionalism regarding the choice of the 'empirical basis' has direct implications on his scientific historiography. A general methodology of science stands in the same relationship to the actual history of science as a scientific theory stands to empirical facts in its domain, according to Lakatos. As theory influences the 'empirical basis' decision for the scientist, methodology influences the 'empirical basis' decision for the historian of science. 'History without some theoretical "bias" is impossible'.[39] The 'History of science is a history of events which are selected and interpreted in a normative way'.[40] Lakatos makes the following note on his first historical example:

> The proposition 'the Proutian programme was carried through' looks like a 'factual' proposition. But there are no 'factual' propositions: the phrase only came into ordinary language from dogmatic empiricism. Scientific 'factual' propositions are theory-laden: the theories involved are 'observation theories'. Historiographical 'factual' propositions are also theory-laden: the theories involved are methodological theories.[41]

Thus it is argued that observations in the history of science are influenced by methodology in exactly the same way as observations are influenced by scientific theory. The historian of science selects the 'facts' to be considered—the history of science is thus a 'decision' and contains the same conventional element as scientific practice. This is an almost 'natural' extension of the Popperian conventionalism discussed above.

Since there is interaction beween the 'facts' of science and the historian's methodological preferences, conflicts between a scientific methodology and the history of science cannot constitute a 'refutation' of the methodology in any simple sense. The appraisal of alternative scientific methodologies is therefore much more complex for Lakatos than simply examining the 'facts'. Lakatos attempts to overcome this difficulty in elegant dialectic style, using what he labels a 'quasi-empirical meta-criterion'.[42]

> ... I first 'refute' falsificationism by 'applying' falsificationism (on a normative historiographical meta-level) to itself. Then I shall apply falsificationism also to inductivism and conventionalism, and, indeed, argue that all methodologies are bound to end up 'falsified' with the help of this Pyrrhonian machine de guerre. Finally, I shall 'apply' not falsificationism but the methodology of scientific research programmes (again on a normative historiographical meta-level) to inductivism, conventionalism, falsificationism and to itself, and show that—on this meta-criterion—methodologies can be constructively criticised and compared. This normative-historiographical version of the methodology of

dogmatic falsificationism which Popper himself is too sophisticated to do. Lakatos (1971*a*) (pp. 129-30, note 93), makes reference to Beveridge's farewell address as Director of the London School of Economics, June 24, 1937, which stresses the influence of the hard facts of the Michelson-Morley experiment on Einstein's subsequent work. Lakatos considers this a distortion of the history of physics by a dogmatic falsificationist. The exact excerpt from Beveridge's speech which Lakatos criticizes adorns the front flyleaf of the introductory text long considered the most philosophically sophisticated of those used in U.S. universities, that is, Lipsey and Steiner (1975).

39 Lakatos (1971*a*), p. 107.

40 Lakatos (1971*a*), p. 108. This statement contains the following note (p. 128, note 69):"Unfortunately there is only one single word in most languages to denote history[1] (the set of historical events) and history[2] (a set of historical propositions). Any history[2] is a theory and value-laden reconstruction of history[1].

41 Lakatos (1971*a*), p. 127, note 60.

42 Lakatos (1971*a*), pp. 109ff.

scientific research programmes supplies a general theory of how to compare rival logics of discovery in which ... history may be seen as a 'test' of its rational reconstructions.[43]

It is immaterial whether Lakatos is entirely successful in this attempt, or if there is a circularity involved as Kuhn has argued.[44] The point is that the MSRP is erected on a certain part of Popperian methodology and that acceptance of this foundation is necessary for the acceptance of Lakatos' programme. This acceptance also implies a certain perspective of the interanimation of scientific methodologies and the history of science. To claim the Lakatosian methodology and hold a view of the history of science which is sycophantic on other earlier methodologies is inconsistent and leads away from the essential core of Lakatos' work. If methodologies could simply be 'tested' by the history of science in a dogmatic way, then theories could be 'tested' by observations. This view may well be held by many, but those who subscribe to this view cannot be advocates of Lakatos' work or those aspects of Popperian methodology from whence it sprang.

Failure to accept Lakatos' conventionalism at the meta-level implies that the MSRP must be rejected, and that there was no need to advance to Popper's work from dogmatic falsificationism. This, of course, does not imply that one must accept the Lakatosian framework, it only implies that acceptance at the micro level entails its acceptance at the macro level, and *vice versa*.

Many of the contributors to *Method and Appraisal* seem to be inconsistent in this sense. They cling to the demarcation between the objective (positive) history of economics and methodological prescriptions about how economics 'ought' to be done (normative), while advocating the MSRP as the vehicle of appraisal for economic theory. Acceptance of Lakatos' view implies that methodological prescriptions describing what scientists 'ought' to do are inexorably intertwined with a putatively 'positive' presentation of what science has done.

Leijonhufvud, for instance, remarks of the 'apparent "drunkard's walk" along and across this sacred line' of normative and positive aspects.[45] He then argues that it is unclear whether an instance in the history of science which is inexplicable in the Lakatosian framework should 'falsify' the MSRP.[46]

From those aspects of Lakatos' work discussed above, it is apparent that the necessity of a 'drunkard's walk' is not just a 'problem', but an essential implication of the MSRP, and that 'falsifying' the MSRP represents the elevation of 'falsificationism' to the status of an accepted general theory of rationality. This amounts to answering the question before asking it. The MSRP can be collated to falsificationism but there must first be a decision regarding the historical 'empirical basis' in question.[47] The MSRP must be compared to falsificationism by Lakatos' 'quasi-empirical meta-criterion'. To approach it in the 'direct' method of Leijonhufvud amounts to allowing a classical empiricism rejected by Lakatos to enter by the back door. This, of course, represents an inconsistency for an advocate of the MSRP such as Leijonhufvud.

Blaug on the other hand recognizes these implications in Lakatos,[48] but analyzes Kuhn's work[49] in terms of the distinction between 'normative

43 Lakatos (1971*a*), p. 109.
44 Kuhn (1971), pp. 142-43.
45 Leijonhufvud (1976), p. 66.
46 Leijonhufvud (1976), p. 67.
47 Cf. Lakatos (1971*a*), p. 110.
48 Blaug (1976*a*), p. 150.
49 Blaug is making reference to Kuhn (1970*a*).

methodology' and 'positive history' where it is no more applicable than it is to Lakatos.[50] Hutchison argues that a 'normative positive confusion' is prevalent in both Kuhn and Lakatos,[51] and yet argues for a 'Popperian view' which is ostensibly free of such confusion. This of course neglects the fact that such 'normative positive confusion' is not 'confusion' at all but an important implication of Lakatos' work resulting from a certain aspect of its Popperian roots.

Overall it seems that the contributors to this volume have not emphasized the 'conventionalist' element in Popper or Lakatos and that this has led them to view the history versus methodology issue in a very non-conventionalist way also. This leads to inconsistencies in many of the essays and a failure to grasp the full implications of Lakatos' work. It seems that these authors are indicative of the economics profession in general, in that they are trying to fit contemporary metascience such as Lakatos and Kuhn into a dogmatic empiricism which these philosophers unanimously reject. These contemporary philosophers of science have developed their work precisely to explain the growth of knowledge without depending on an unacceptable dogmatic empiricism. This does not seem to be recognized by economists in general.

In good conscience it must be noted that some of the essays, specifically those of Coats, DeMarchi, and Latsis, tactfully avoid problems such as those mentioned above, although they still lack an explicit delineation of the 'conventionalist' element and its implications. This would certainly 'complete' these essays. It must also be added that the essay by Simon is sufficiently removed from all the issues raised above to completely circumvent such criticism.

CONCLUSION

In conclusion it must be stated that the above criticisms are not intended to demean the overall quality or importance of any essays contained in *Method and Appraisal*. The criticisms are only meant to elucidate important refinements which must be made in relating the MSRP to economics.

The above criticisms imply that economists who are involved in work such as this should start with a 'deeper' reading of both Popper and Lakatos. The 'conventionalist' influence in both of their works is more than a philosophic subtlety and has implications of a significant nature. The normative-positive distinction long cherished by the economic profession may be applicable in the policy situations where it is usually made, but it is inappropriate for comparing methodologies of science. Used in this way it represents a certain methodological atavism. The above discussion also warns against 'applying' a scientific methodology without an extended historical investigation of its relation to other methodologies.

Even with the above criticisms, I feel that work in the Methodology of Economic Research Programmes made its step from nonexistence directly into a 'progressive' period with the publication of this volume. What is stated above should be taken as a guide to refinement not as an indictment for abandonment.

50 Blaug (1976*a*), p. 152. For Kuhn's comments on this interpretation of him by others, see Kuhn (1970*c*), pp. 233, 237.

51 Hutchison (1976), p. 182.

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