

## Fritz Machlup: A Precursor of Semantic Conception in the Philosophy of Science

### I. Introduction.

Few authors in the Austrian Tradition in Economics are more frequently overlooked than Fritz Machlup, most likely due in part to his intellectual moderation and his proverbial Viennese politeness.

He is still overshadowed today by more well-known economists like Mises or Hayek; in the field of the Philosophy of Science, however, he was one of the most influential authors to emerge from the Austrian Tradition into mainstream scholarship. His discussions with Terence W. Hutchison (about empiricism and verification) and Richard Lester (about firm theory) are still widely studied and quoted, and his methodological position is still considered an interesting alternative to what he once labeled ultraempiricism.

In fact, Milton Friedman's classical position in his *Methodology of Positive Economics* (Friedman 1953) was interpreted by the own Fritz Machlup as very similar to his. Therefore it is still interesting to understand Machlup's position and to continue through his methodological path, a field about which he particularly cared.

Throughout his life, Machlup followed the developments and different currents in the field of the Philosophy of Science with great interest.<sup>1</sup> Thus when he wrote about economic methodology, he was a *rara avis*: a successful practitioner who was also well read in the Philosophy of Science.

He also followed his own rule, which he clearly stated as a piece of advice to every economist:

“I often tell my students that they should not publish any methodological notes, papers or books until they have done years of substantive research in their field and attained recognition for their mastery of its technical aspects” (Machlup 1978, X).

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<sup>1</sup> Most of his articles on this matter are included in Machlup 1978.

Unfortunately, the development of the Methodology<sup>2</sup> of Economics as a separate field made that piece of advice obsolete. In our time, it is nearly impossible to find a successful practitioner and a well-read philosopher in one individual. Machlup was such a person, which is why it is so important to fully understand his position. Because there is such great ignorance of the philosophy of science in nearly every economist's writing about that field,<sup>3</sup> Machlup stands out from his peers.

Machlup himself felt it necessary to explain his "strong taste" for methodology; he did so by pointing to the Viennese intellectual milieu<sup>4</sup> in which he grew up:

"I have often been asked how I developed such a strong taste for methodological discourse. The answer is easy: in the intellectual milieu in which I lived it would have been surprising for any student to remain uninterested in methodology." (Machlup 1978, I).

The Austrian author then gives an impressive list of some of the most relevant individuals in that milieu. The list of renowned twentieth-century intellectuals consists of the following: Menger, Mach, Weber, Schlick, Waismann, Carnap, Neurath, Hahn, Feigl, Wittgenstein, Popper, Gödel, Mises, Kaufmann and Schütz.

Because of Machlup's "strong taste" in methodological discourse, I am particularly interested in discussing his position in relation to some of the most promising recent developments in the Philosophy of Science.

As Bruce Caldwell says:

... it is still clear that Machlup had a better understanding of the philosophy of science that was contemporaneous with his efforts than did Hutchison... Machlup's methodological schooling allowed him to formulate a position consistent with the philosophy of science of his time (Caldwell 1994, 145).

The main danger of the type of reinterpretation proposed in the present paper is to fall into empty scholasticism. To avoid this danger, the emphasis must be placed on the actual

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<sup>2</sup> With a capital "M" notwithstanding McCloskey's criticism (McCloskey 1983).

<sup>3</sup> On the other hand, not many philosophers of science wrote about economics. Caldwell (1994) shows the problems and confusions that this economist's lack of knowledge while writing about Philosophy of Science has caused through the twentieth century.

<sup>4</sup> The classic book about that unique viennese milieu is Janik and Toulmin (1973) and his sequel Janik (2001).

practitioners of Economics and their concerns; they will be interested in interrogating Machlup about his methodological position, and not simply for the sake of his memory.<sup>5</sup> This work intends to show that Machlup's well-known and influential position in Economics Methodology can be better understood in the framework of the Semantic Conception, thereby allowing a better elucidation of economists' practice for the Philosophy of Science.

In the following section (II), I will analyze Machlup's position and its usual interpretation; in section III, I will characterize briefly the Semantic Conception in the Philosophy of Science. Then in section IV, I will attempt to show the similarities and differences between them. Finally, in section V, I will suggest some conclusions for future study.

## II. Fritz Machlup's methodological positions and its usual interpretations

Machlup's first published article on methodological issues (Machlup,1936)<sup>6</sup> stated in its conclusion: "The failure to make distinctions between statements of different order often has serious practical consequences".

Two of the most important features of his position appeared there: his quest for semantic clarity and his concern with practical consequences. For Machlup, methodological debates were important because they had practical consequences.

In his next published contribution to methodological issues (Machlup,1952) Machlup analyzed carefully the concept of "model", stating that its use was not limited to economics.

Instead, models are indispensable for thinking. "We think with the aid of models, whether we know it or not" and "All theorizing involves model building" (Machlup 1978, 76)

This not means that in "descriptive" economics, economists do not use models, it means that while doing this empirical work they use ready-made models, instead of building them.

During his famous discussion with T.W.Hutchison<sup>7</sup>, in Machlup's view, the main problem in Hutchison's position was its naivety in not recognizing different levels of hypotheses. In contrast, Machlup distinguished several levels in a group of what he called fundamental

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<sup>5</sup> Although this concern with practicing economists is an important part of Machlup's legacy.

<sup>6</sup> When I make a general reference to an article I will indent ify it with its original publication date. Of course when I quote it, I will quote the book I have at hand, usually Machlup (1978).

<sup>7</sup> Probably the best account of this discussion can be found in Caldwell (1994).

assumptions: these assumptions are not testable in isolation; they may only be tested indirectly, in a group.

In his classic paper (Machlup,1955) he builds his personal position on the subject, by proposing a “model of the use of analytical apparatus” that he also calls “a machine” and “an engine of pure theory” (Machlup,1978, 149),

To an assumed change regarded as cause, assumed conditions must be added. These assumptions can be of two main different kinds: the fixed ones “that make the machine what it is” that he calls “fundamental assumptions” and the variable, exchangeable ones or “assumptions about the conditions” (Machlup 1978, 148).

These Assumed conditions must be verified empirically "but we not need to be particularly strict about their verification" (Machlup 1978, 150) and can be classified in three different types.

The ones of Type A (or “type of case”) refer to conditions that are normally present. In Machlup’s words: "these refer to conditions that may vary from case to case and influence the outcome significantly, but are sufficiently common to justify the construction of types" (Machlup 1978, 150). Examples of this type include the following: types of goods involved, cost conditions, and elasticity of supply or demand.

Those of Type B (or “type of setting”), in Machlup’s words: "refer to conditions which may change over brief periods of time –say, with a change of government or of the political situation, or during the business cycle- and are apt to influence the outcome in definite directions." (Machlup 1978, 150). Examples of this type include the following: bank credit availability, fiscal and monetary policies, and trade union policies.

Finally the ones of Type C (or “type of economy”), refer to the following: "conditions which may vary from country to country and over larger periods of time, but may be assumed to be settled for a sufficient large number of cases to justify taking these conditions as constant" (Machlup 1978, 151)

Examples of this type include the following: social institutions, private property, and social customs and usages.

Given the fundamental assumptions and assumed conditions of types A, B, and C, a deduced change may be inferred from the interaction of an assumed change with them. Only if this deduced change is repeatedly disconfirmed by experience will fundamental assumptions be considered disconfirmed and overturned through indirect testing.

In later writings, Machlup stated that the only flaw he found in Friedman's classic article (Friedman 1953)<sup>8</sup> was that Friedman forgets that the fundamental assumptions of Economics "should be required to be understandable, in the sense in which man can understand the actions of fellowmen" (Machlup 1978, 153). For Machlup, under the influence of his personal friend Alfred Schutz, this meaningfulness condition was required.

In one of his last published materials (Machlup 1980), Machlup said the following about the discussion of "a priori" models:

You may call any model "a priori" because you can "build" the model according to your own specifications... Construction is always "a priori" even if you construe with some experience in mind. The domain of construction needs constructs and postulated relationships between constructs, but it is itself not the result of observation; it is "a priori"... But it is only a skeleton without flesh (Machlup 1980, 1).

### The usual reading

Machlup is usually considered a Lakatosian precursor. Both Zanotti (1991) and Langlois and Koppl (1991) accept this interpretation.

In support of his own interpretation of Machlup as a Lakatosian precursor, Gabriel Zanotti (Zanotti 1991) quoted an article in which Machlup adopted the same expression as Lakatos when he referred to fundamental assumptions as the "hard core" of a theory. Because the article is from 1974, Zanotti considered Machlup to have explicitly accepted his own past position, developed through his discussions with Hutchison and Lester, as very similar to Lakatos' formulation of Scientific Research Programs<sup>9</sup>

Zanotti does seem to have a valid point, particularly if one considers Machlup's semantic meticulousness (as evidenced, for example, by Machlup's book on semantics) (Machlup 1963)

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<sup>8</sup> Mäki (2009) is a good example of Friedman's article classicism.

<sup>9</sup> Machlup uses the expression "hard core" while referring to the concept of "model".

and his knowledge of the Philosophy of Science. I would add to Zanotti's argument another telling linguistic similarity: in Machlup's famous article on verification (which establishes the foundations of his position), he states that another name that may be given to fundamental assumptions is "heuristic principles". (Machlup 1978, 145). Of course, this is not the same as Lakatos' position, which clearly distinguishes between hard core and heuristic instructions, but it proves that Machlup was not far from that position.

In their article, Langlois and Koppl (1991) stated the importance of understanding "these more recent developments"—they are referring particularly to Lakatos' Methodology of Scientific Research Programs, among others.

"Understanding these more recent developments in philosophy of science is useful for understanding Machlup's methodological views. For, in many respects, his position is precisely an **anticipation** of these modern developments"<sup>10</sup>(Langlois and Koppl's 1991, 88)

Langlois and Koppl suggested that Machlup's compromise between ultraempiricism and apriorism was similar to Lakatos' compromise between Popper and Kuhn. I agree that there is a resemblance between the two compromises, but I believe that it is superficial: Machlup was a practicing economist, with a serious knowledge of the Philosophy of Science, trying to provide some methodological guidance for other practicing economists. Lakatos was a philosopher whose main purpose was to try to find a middle way between other philosophers' positions. Thus, Machlup's main purpose was different: he tried to make better economics, the actual practice of economics was an essential part of his methodological proposals.

Langlois and Koppl noted that Machlup sometimes used the term "model" instead of "theory," but they do not stressed the importance of that change and the reason Machlup gave for it<sup>11</sup>; because of this, I think they missed a very interesting point.

Langlois and Koppl analyzed also Alfred Schütz's influence on Machlup and concluded that his position was basically derived from Schütz's. I do not deny this influence, but I

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<sup>10</sup> Emphasis in the original.

<sup>11</sup> See Section IV.

disagree with the degree to which it influenced Machlup's position. Nevertheless, I concur with one of the authors' conclusions:

“For one thing, Machlup recognized more clearly than Schutz, that our choices are not between individual models, but among theoretical systems -among hierarchical or quasi-hierarchical structures of inter-related models” (Langlois and Koppl 1991, 92)

This is a compelling reason to consider Machlup as a precursor of the Semantic Conception, which can be rightly described as considering theoretical systems as hierarchical structures of inter-related models.

In his article Wenceslao González (González 2009) made a brief reference to Machlup while analyzing Lakatos: quoting Spiro Latsis, he approvingly stated that Machlup's position is closer to Lakatos' concept of the “hard core” of ideas, but he also emphasized that there is nothing in Machlup's writing about the prediction of new facts.

In spite of these arguments, I believe that Machlup's position is better understood within the framework of the Semantic Conception in the Philosophy of Science (as a precursor of this family of thought in the Philosophy of Economics) than as a Lakatosian precursor.

Before making my argument for this belief, I will briefly characterize the Semantic Conception in the Philosophy of Science (for the reader who is not familiar with it).

Therefore, anyone with a working knowledge of the Philosophy of Science can skip the next section and proceed straight to the fourth.

### III. The Semantic Conception

For the purposes of this work, I will follow a standard textbook in the Philosophy of Science by two authors who are part of the Structuralist current (one of the main currents within the Semantic Conception). (Diez y Moulines 1999).

The authors refer to the “family of the semanticists” as including some positions that, although their features allow them to be considered as part of a certain family, also show individuating differences.<sup>12</sup>

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<sup>12</sup> The Wittgensteinian reference is, of course, intended by the authors. In this point, as in many others, they follow Frederik Suppe's classical introduction to the symposium held in Urbana, in 1969 (Suppe 1977)

I will concentrate particularly on those common features that allow a position to be included within the Semantic Conception, as I intend to show that Machlup's work is better understood as a precursor of this particular family in the Philosophy of Science.

Beginning with its chosen name, the Semantic Conception tries to establish its differences from the Received View that emphasizes the syntactic aspects of science. For the Semantic Conception, a scientific theory is formulated by a set of models (rather than by the presentation of a series of axioms) because theories are not only a set of propositions<sup>13</sup>. As a consequence of this enlargement of the concept of theory, a term can be labeled "theoretical" or non-observable only in reference to a theory T: such terms are therefore called t-theoretical. Thus, the Semantic Conception proposes a solution to one of the more difficult questions of the Received View: the definition of terms that were defined only negatively as non-observables. Because the conception of model is fundamentally semantic, the chosen name intends to underline that idea.<sup>14</sup>

A classic work of the Structuralist branch of the Semantic Conception (Balzer, et al. 1987) notes the following:

"The fundamental intuition underlying our approach is that the smallest significant or interesting parts of empirical science –things like empirical laws- are best characterized, not as linguistic entities, but as model-theoretic entities – classes of set-theoretic structures" (Balzer, et.al p.xxi)

Of course there is a question of emphasis because it is not true to say that the Received View has totally overlooked the semantic aspects involved in the Philosophy of Science.<sup>15</sup> That is why the Semantic Conception must be understood as an effort to answer some critiques that originally targeted the Received View.

The common family features in the Semantic Conception can be summarized as three characteristics: a) a theory is a set of models, b) that set of models is identified by the empirical phenomena that it intends to explain, and c) that pretension creates the empirical

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<sup>13</sup> I will use "proposition" as used in modern logic of first order, also called propositional logic.

<sup>14</sup> Other usual way to refer to the Semantic Conception is as the Model-Theoretical Conception

<sup>15</sup> Particularly Rudolf Carnap wrote extensively about semantically aspects of the language of science in Carnap (1942) Carnap (1945), Carnap [1947] (1970) and of course in Carnap [1963] (1997)

assertion of the theory, which is that there is some type of relationship between the models determined by laws and the empirical phenomena that must be clarified.

The first common feature states that theories are not a set of propositions, but a set of models. These models are determined by a set of principles, also called laws or (more commonly) axioms that designate what entities or class of entities they are going to reference. These models delimit the universe of discourse.

The second feature adds the empirical aspect, pointing out the empirical phenomena that the theory attempts to explain. Models are not constructed as self-contained activities, but as explanations for empirical phenomena. These empirical targets are called “intended applications” (Balzer et.al 1987) or “intended scope” (Suppe 1977).

The third feature makes the theory’s empirical intention explicit; this assertion is that there is a relationship between its models and empirical phenomena. This relationship can be of very different types according to different members of the Semantic Conception: it can be one relationship of identity, one of approximation, or one of subsumption. However, the point is that models “fit” the systems the theory wants to explain.

To end this brief characterization of the Semantic Conception, another key consequence of considering a theory to be a set of models rather than a set of propositions is that truth can no longer be predicated about theories, which means that theories can no longer be directly true or false because they are not propositions (and truth or falsehood can only be predicated about propositions). Thus the attribution of truth or falsehood can only be made to a theory indirectly through its empirical assertions, which are propositions.

#### IV. Machlup as a precursor of the Semantic Conception

The key article in which the similarities between Machlup’s position and the Semantic Conception are clearly stated was published in 1960. (Machlup 1960). In that article, in a section entitled “Construct and Model,” Machlup addresses two points that I will analyze in detail: the definition of models and their form and composition.

##### a) The Definition of a Model

The first point that Machlup addresses is the definition of a model. Initially, he condemns the reckless and sometimes sloppy usage of this term. Of course, none of these accusations

can be directed to the Semantic Conception, which shares Machlup's careful attention to detail and precision.

Machlup then provides his own definition of a model (a system of interrelated constructs) and distinguishes it from a theory, stating that a theory is more than a model. His definition of a theory is strikingly similar to the Semantic Conception:

“A theory may be regarded as a model plus a specification of the empirical observations to which it applies”(Machlup 1978, 178).

And later in the same page, he adds:

“Thus, to repeat applicable or applied theory consists of two parts 1) a pure model and 2) a specification of the empirical facts (described ordinarily in terms of real types or operational concepts) whose changes it will explain or predict” (Machlup 1978,178).

Machlup's definition of theory is nearly identical to the definition provided by the Semantic Conception: it includes a model and an empirical aspect. This is not just a linguistic coincidence: Machlup and the Semantic Conception are not simply using the same terms, they are actually saying something very similar.

b) The form and composition of models.

After suggesting a taxonomy of models, Machlup states that the only pertinent classes for his analysis are the verbal and symbolic ones, which he divides between the geometric and the algebraic. However, he states that in any model, its function is to show connections, relationships, and interdependences. In his words:

But in all forms the function of a model is to exhibit connections, relationships, interdependences. There would be no reason for making a model except to show how some things “hang together” or of what “elements” they are composed or how they “work” or are “adjusted” (Machlup 1978, 176)

Again, we can see a similarity between Machlup and the Semantic Conception: both state the key aspect of empirical phenomena in model building. In contrast, however, I must note that some authors within the Semantic Conception (particularly Moulines 2007) have also

repeatedly affirmed the unavoidable intertheoretical aspect of model building. However, this is not a feature that can be attributed to all members of the Semantic family.

Of course, although these references are very clear, they are not of much value if they are isolated. Therefore, I will attempt to show (by analyzing Machlup's work) that this is not the case.

For example, during his famous discussion with Hutchison (Machlup 1956) about Hutchison's problem in understanding the notion of indirect testing, Machlup states that the root of the problem is Hutchison's refusal to accept the existence of a third type of proposition<sup>16</sup> (although truth or falsehood cannot be predicated on such propositions, they still have empirical content).

In Machlup's words:

If so, he rejects a third category of propositions used in most theoretical systems: the heuristic postulates and idealized assumptions of abstract models of interdependent constructs useful in the explanation and prediction of observable phenomena. Such propositions are neither "true or false" nor empirically meaningless. They cannot be false because what they predicate is predicated about ideal constructs, not about things or events of reality. Yet they are not empirically "meaningless" because they are supposed to "apply" or correspond broadly to experienced events. They cannot be "falsified" by observed facts, or even be "proved inapplicable" because auxiliary assumptions can be brought in to establish correspondence with almost any kind of facts; but they can be superseded by other propositions which are in better agreement with these facts without recourse to so many auxiliary assumptions" (Machlup 1978, 496)

Then, in a footnote, Machlup notes that these third propositions were called "procedural rules" by Kaufman, "complex analytical propositions" by Montague, and "constitutive, non-epistemic" by Margenau.<sup>17</sup>

I think that this particular passage (from which I quoted extensively) shows the main problem with syntacticism that reduces the complex social construct of a scientific theory

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<sup>16</sup> I called these linguistic entities propositions after Machlup; however, by definition, an atomic proposition is the smallest unit of meaning on which truth or falsehood may be predicated.

<sup>17</sup> Caldwell (1994) correctly states that this third category is unnecessary if the principle of indirect verification is applied.

to a group of propositions. Maybe that is why Machlup (and, later, many others) started to talk about “models,” although he did not favor this polysemic term.<sup>18</sup>

In a previous work, after stating that models are indispensable for thinking (even though, like Monsieur Jordan in Molière’s famous play<sup>19</sup> we may not be aware of using them), Machlup affirmed that there is no need to use the term “model.”

Again, in his words:

“We think with the aid of models, whether we know it or not... Of course there is no need to use the word “model”. Other words have been used in the same sense –for example “construct” and “schema”(Machlup 1978, 76)

In a later work (Machlup 1961), Machlup cites Richard Braithwaite’s distinction between higher-level hypotheses (which can never be either directly or indirectly verified in isolation) and lower-level generalizations that can be (and are) permanently tested in the social sciences.

Machlup writes:

Only a **whole system**<sup>20</sup> of hypotheses can be tested by deducing from some set of general postulates and some sets of specific assumptions the logical consequences and comparing these with records of observations regarded as the approximate empirical counterparts of the specific assumptions and specific consequences (Machlup, 1978 p.354)

Finally, in one of his last published works on the matter, (Machlup 1978) Machlup makes an erudite comparison between different authors and their methodological positions. When he analyzes Hans Reichenbach’s work, he chooses to quote the preface of “Experience and prediction”; specifically, he cites Reichenbach’s reference to indirect propositions that are only given some degree of probability by their relationship to direct propositions (through reference to physical facts that are immediately observable).

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<sup>18</sup> For Machlup it was a “weasel word” and “catch-all word”, in other words, a term that through its careless and polisemic usage keeps its outside intact but is has no meaning inside, as an egg which was sucked by a weasel. See also Machlup (1963) 73-96, where “structure” is analyzed. About “model” see Machlup (1978) 175

<sup>19</sup> Le Bourgeois Gentilhomme

<sup>20</sup> Machlup’s emphasis

It seems clear that Machlup saw the problems presented by some aspects of the Received View and that he tried persistently to solve them by creating new categories of propositions. The real limitation, he believed, was the syntactic view that considered theories to be propositions.

## V. Conclusions

It is difficult to deny the usual position that Machlup was a Lakatosian precursor, some interesting arguments can be made to support this view, particularly about Machlup's choice of word. And linguistic meticulousity was a very significant issue for Fritz Machlup. However, I believe that Machlup's similarity to the Semantic Conception is deeper than his similarity to Lakatos.

Of course, I do not mean to imply that Machlup was a crypto-semanticist, that will be an exaggerated presentism<sup>21</sup>; I merely wish to state that the well-developed framework of the Semantic Conception in the Philosophy of Science allows for the continuation and expansion of Machlup's work.

These conclusions are not intended to be a scholastic reinterpretation of Machlup's texts; rather, they are an attempt to show that the Semantic Conception is compatible with, and even anticipated by these texts.

The main problems that Machlup confronted in the methodological area originated in the bounds of the Received View. Those problems clearly necessitate the introduction of a third category of propositions (which, although containing empirical content, truth or falsehood cannot be predicated on them). Machlup followed Kaufman and Reichenbach in that conclusion, again demonstrating his mastery of the Philosophy of Science.

The Semantic Conception solves the problem and removes the limitations of the Received View by changing its conception of what scientific theories are—making them complex social constructs<sup>22</sup> rather than just propositions. Machlup demonstrated this point, but with a very important difference: he was part of an individualistic tradition and therefore always

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<sup>21</sup> Although a degree of presentism is unavoidable in every reconstruction, making an historical reconstruction impossible, its exaggeration is usually labeled as "whiggish" after Herbert Butterfield's book. (Butterfield 1965)

<sup>22</sup> Semanticists still share Machlup's search for clarity and content. Their conception of theories as complex social constructs does not give up on these aims.

thought of models as individualistic mental constructs. The Semantic Conception denies that individualistic character and stresses instead the social aspect of constructs. Although Machlup worked intensively in the production and distribution of knowledge, (Machlup 1973) he missed the unavoidable communality of the scientific enterprise (most likely because of his individualistic formation).

It seems probable that Machlup's rare erudition in the field of the Philosophy of Science made him aware of the shortcomings for which the Received View was criticized.

He also dealt with the problem of the aprioristic position of his teacher, Ludwig Von Mises, by stating that models as mental constructs can be called "a priori."

It is clear that although Machlup was against using the term "model" (because of its polysemous character), he still used it to show that it was different from a theory. A theory was, in Machlup's definition, a model plus a specification of the empirical observations to which it applies. This inclusion of the empirical is the Semantic Conception's proposed solution for the famous "theoretical terms problem."

This strikingly similar definition of one the most important elements in the Philosophy of Science – the meaning of "theory" – is the main reason for reexamining Machlup as a precursor of the Semantic Conception.

This reexamination may help to establish a new understanding of Machlup's works and, more importantly, a new elucidation of the practice of today's economists.

## Bibliography

- Balzer, Wolfgang, Moulines, Ulises C. and Sneed, Joseph. 1987. *An architectonic for science. The Structuralist Program*. Dordrecht: D. Reidel.
- Butterfield, Herbert. 1965 [1931]. *The Whig Interpretation of History*, New York:W.W. Norton & Company, Inc.
- Caldwell, Bruce. 1994 [1982]. *Beyond Positivism*. New York: Routledge.
- Carnap, Rudolf. 1942. *Introduction to Semantics*. Cambridge: Harvard University Press.
- Carnap, Rudolf. 1945. Hall and Bergmann on Semantics. *Mind*, 54 (214): 148-155.
- Carnap, Rudolf. 1970 [1947]. *Meaning and Necessity*. Chicago: University of Chicago Press.
- Carnap, Rudolf. 1997 [1963]. *The Philosophy of Rudolf Carnap*. Peru, Illinois: Open Court.
- Freidman, Milton. 2009 [1953]. The Methodology of Positive Economics, reprinted in Mäki (2009).
- González, Wenceslao. 2009. La repercusión de Popper, Kuhn y Lakatos en la Metodología de la economía in *Sobre la Economía y sus métodos*, ed. Juan Carlos García Bermejo. Madrid: Editorial Trotta 327 -354.
- Janik, Allan y Toulmin, Stephen. 1998 [1973]. *Wittgenstein's Vienna*. Trad: Ignacio Gómez de Liano *La Viena de Wittgenstein*. Madrid: Editorial Taurus.
- Janik, Allan. 2001. *Wittgenstein's Vienna revisited*. New Jersey: Transaction Publishers.
- Langlois, Richard N. and Koppl, Roger. 1991. Fritz Machlup and marginalism: a reevaluation. *Methodus*, 3 (2): 86-102.
- Machlup, Fritz 1936 Why bother with Methodology?, reprinted in Machlup (1978): 63-70

Machlup, Fritz. 1952. A note on models in Microeconomics, reprinted in Machlup (1978): 75-99.

Machlup, Fritz. 1955. The problem of verification in Economics, reprinted in Machlup (1978): 137-157.

Machlup, Fritz. 1956. Rejoinder to a reluctant ultra-empiricist, reprinted in Machlup (1978) as Terence Hutchison's reluctant ultra-empiricism: 493-503.

Machlup, Fritz. 1960. Operational Concepts and Mental Constructs, reprinted Machlup (1978): 159-188.

Machlup, Fritz. 1973. *The Production and Distribution of Knowledge in the United States*. Princeton: Princeton University Press.

Machlup, Fritz. 1978. *Methodology of Economics and other Social Sciences*. New York: Academic Press Inc.

Machlup, Fritz. 1980. An interview with Fritz Machlup. *Austrian Economic Newsletter*, 3 (1): 1-3.

McCloskey, Donald N. 1983. The Rethoric of Economic. *Journal of Economics Literature*. 21(2): 481-517.

Mäki, Uskali. 2009. *The Methodology of Positive Economics. Reflections on the Milton Freidman legacy*. New York: Cambridge University Press.

Moulines, C. Ulises. 2007. Model construction, idealization and scientific ontology in *The Courage of Doing Philosophy. Essays presented to Leszek Nowak*, eds. Brzezinski, Jerzy, Klawiter, Andrzej, Kuipers, Theo A.F., Lastowski, Krzysztof, Paprzycka, Katarzyna & Przybysz, Piotr. Amsterdam: Editions Rodopi B.V. 257-272.

Rothbard, Murray 1957. In defence of Extreme Apriorism in *Souther Economic Journal*, 23 (1) : 314-320

Suppe, Frederick. 1977 [1973]. *The Structure of Scientific Theories*. Urbana: University of Illinois Press.

Zanotti, Gabriel. 1991. Machlup: un Puente entre Mises y Lakatos. *Libertas*, 15: 42-50.