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Jarosław Boruszewski*

PROCEDURAL SEMANTICS AND ECONOMIC MODELS[1]

ABSTRACT

The aim of the article is to present issues regarding economic semantics. The key problem is finding a satisfactory view of semantic interpretation for economic models. The author of the article suggests that an adequate version of procedural semantics can provide such a view. This theory combines semantic interpretation with cognitive and practical activities of subjects which use economic models. Philosophical roots of procedural semantics come from the pragmatic and operational theory of meaning. The article contains a detailed discussion of operationalism in economics and a comparison of the specifics of procedural approach to semantics with the philosophical and methodological characteristics of economic models. The version of procedural semantics used in the analysis is Jan Żytkow's semantics of operational procedures, in the light of which Irving Fisher's Cash Loop Model is reconstructed. Directions for further studies of procedural explication of semantics economic models are also outlined, taking into account the methodological specifics of economic sciences.

Keywords: philosophy of economics, economic models, economic semantics, operationalism.

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* Institute of Philosophy, Adam Mickiewicz University, Poznań; e-mail: borjar@amu.edu.pl

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It is obvious that meaning is not just something that either is or isn't,
but that meanings are generated in time and they become
(Bridgman, 1949, p. 258)

1. INTRODUCTION: PROBLEMS WITH ECONOMIC SEMANTICS

The semantics of theoretical constructs of economic science and of economic models in particular is a matter which cannot be thematised easily. How do economic models acquire meaning and refer to things which are external to them? Such questions are difficult to answer. However, they are one of the focuses of contemporary philosophy and methodology of economics (Mäki, 2018, p. 4). One of the sources of problems for the semantics of economic models is – as it is emphasised by Uskali Mäki – the semantic scepticism in economics. In its strong version, it is expressed in a view that "models do not even refer to actual economic reality (but rather to some imaginary fiction)" (Mäki, 1999, p. 307). However, a far-reaching inquiry is needed to overcome this semantic scepticism. Undoubtedly, one should pay special attention to the use of key terms in the economic discourse. The analyses carried out by Fritz Machlup (1991) within the field of economic semantics are especially useful in this respect. However, this does not end here. Contemporary semantics has worked out many theories of meaning. Therefore, it is essential to investigate whether it is possible to use these theories to reconstruct the semantics of economic models.

Traditionally, the dispute in semantics appeared between the followers of descriptivist theory and causal theory (Barnes, 1982). In short, the descriptivists claim that the extension of the term is fixed through identifying descriptions of sets of manifest properties of objects. Conversely, the causalists presume that this extension is set through causal interactions. First, the object is 'baptised' with a given name and then a causal communication chain emerges and links this 'baptism' with another uses of the term. However, we refer to objects not because of their manifest properties but because of their essential relations which occur between the objects within the range of a given term. Both theories encounter serious problems in economics. The problem with the descriptive theory is that theoretical models in economics are not adequate descriptions of economic reality and are not constructed to perform this task. As far as the causal theory is concerned, it is impossible to define exact causal relations between objects in the economic reality and models which would be defined with respect to the essential properties of these objects. It turns out then that there is no single mode of fixing reference (Mäki, 1999, pp. 310–315). These traditional semantic theories, collectively referred to as extensionalism or referentialism, are unable to capture the semantics of economic models adequately. On the grounds of philosophy and methodology of economics, referentialism is opposed to inferentialism which is nowadays treated as a more promising view of economic semantics. Particularly, this refers to causal generalisations in economics (Claveau, Mireles-Flores, 2017), but there are also some suggestions to use inferentialism in the semantics of economic models (de Donato Rodriguez, Zamora Bonilla, 2009). In short, according to inferentialism, the meaning of a given unit depends on its role which it fulfils in all the inferences where it occurs. Inferentialism will be discussed in more detail in the final part of the present article as the main purpose of this article is to propose a view of the semantics of economic models with inferentialism being its special case. The proposal is an attempt to set up a new candidate for a promising account of the semantics of economic models. Procedural semantics seems to fit this purpose and – to the best of the author's knowledge – nobody has considered such an approach before.

From a general philosophical perspective, procedural semantics is "an approach to semantics that views understanding in terms of a set of procedures for deciding whether terms apply to things, or procedures for deciding the truth-values of propositions" (Blackburn, 1996, p. 305). Several philosophical sources of the procedural view of semantics can be pointed out. One of them is the pragmatic theory of meaning by Charles S. Peirce, and especially his famous essay *How to Make our Ideas Clear*. A source which comes second historically and is the most important to the following considerations is the operational theory of meaning by Percy W. Bridgman. The third source can be found in Ludwig Wittgenstein's late philosophy which was dependent on the connection between the meaning of an expression with the way it is used in a language. These three sources are philosophically related, which is often highlighted in the literature (Hands, 2004; Chang, 2017). Operationalism will be the focus of this article for the following reasons:

- this theory has been and still is considered controversial, sometimes unjustifiably;
- this is an account which closely links semantics with methodology;
- operationalism in economics has its own peculiarity and it needs to be emphasised in order to establish certain semantic postulates which should be taken into account in the proposal of the semantics of economic models;
- as compared to Peirce and Wittgenstein, Bridgman more frequently referred to research practice.

There are many procedural semantic theories because "procedural semantics approach is a paradigm or a framework for developing and expressing theories of meaning, rather than being a theory of meaning itself" (Woods, 1981, p. 302). When considering the possibility of the use of procedural semantics in the development of the semantics of economic models, one has to refer to only one version of semantics. In this article, the point of reference is the semantics of operational procedures by Jan M. Żytkow (1944–2001), who was a distinguished methodologist and a long-term collaborator of Herbert A. Simon (e.g. Simon, Żytkow, 1986, 1988). This in a version of procedural semantics in the methodology of sciences,[2] and in this respect Żytkow uniquely modified and developed

[2] Other versions have also been constructed in the semantics of programming languages, logical semantics, cognitive science and, the philosophy of language.

Bridgman's operationalism. Żytkow also directly tackled the problems related to modelling in science, which obviously refers to the subject of this article.

The structure of the article is as follows. The next section presents the discussion of operationalism in economics in 20th century with special attention paid to the questions of semantic nature. The third section presents Jan Żytkow's theory of procedural semantics in the context of the philosophical and methodological characteristics of economic models. The fourth section provides an account of scientific modelling in the procedural perspective and its use to reconstruct a particular economic model – Irving Fisher's Cash Loop Model. The fifth and final section contains selected possible perspectives of the development of procedural semantics in philosophy and the methodology of economics.

2. OPERATIONALISM IN ECONOMICS

Before tackling the problem of operationalism in economics, three important issues have to be considered. The first one is of a general philosophical nature and relates to the fact that operationalism is too easily and frequently conflated with positivism despite the fact that Bridgman was not a positivist (Mirowski, 1998, p. 347). This conflation often results from a selective treatment of Bridgman's work, as well as from a limited presentation of his view based only on his early writings. In this respect, a binding statement for these considerations was made by Wade Hands, according to whom "making social science more 'operational' was, and is, a legitimate goal, but […] supporting such a statement does not require allowing positivism to define the rules of scientific engagement" (Hands, 2004, p. 965). The next issue relates to the development of operationalism in social sciences. It should be noticed that the first reaction of social sciences to Bridgman's *The Logic of Modern Physics* emerged on the grounds of economics. Henry Schultz wrote an article *Rational Economics* in 1928, where he was enthusiastic about operationalism. Unfortunately, this article did not contribute much to the development of operationalism in the methodology of economics. However, operationalism began developing firmly in other social sciences, such as psychology (Stevens, 1935) and (to a lesser extent) sociology (Lundberg, 1939). It was mainly operationalism in psychology that shaped the discussion of operationalism in social sciences. This was a rather unfavourable situation to Bridgman because it was a source of many misinterpretations, distortions and even violations of the methodology and philosophy of operationalism (Koch, 1992). Therefore, this article will not mention any issues relating to operationalism in psychology as it is a separate subject and a problem in itself.

The final issue relates to the scientific work of Paul Samuelson who is considered a representative of operationalism in economics. Strictly speaking, Samuelson was not an operationalist from a methodological perspective. He applied operationalist rhetoric only in his early career and gradually abandoned it. Being unable to clarify this issue at this point (as it requires a separate discussion), let us relate to some statements from literature on Samuelson's alleged operationalism:

- "his operationalism bears no resemblance to Bridgman's thesis […]. It is therefore not surprising that Samuelson's advocacy of 'operationalism' caused little consternation among economists" (Caldwell, 1994, p. 190);
- "[t]his invocation of operationalism is somewhat suspect" (Boland, 2008, p. 3);
- "[i]ronically enough, however, this is not operationalism as that term is usually understood" (Blaug, 1992, p. 87);
- "comparisons between Bridgman's and Samuelson's operationalism often conclude that the latter's *operationally meaningful theorems* were only operational in spirit, but not in Bridgman's sense" (Carvajalino, 2018, p. 2; emphasis in original).

Considering the statements presented above, Samuelson's research will not be the subject of the following considerations. Instead, Donald Gordon and Julian Simon's operational analyses as well as Fritz Machlup and Ben Seligman's criticism of operationalism will be paramount. Gordon's economic operationalism is particularly crucial for the following considerations not only because – as Blaug claimed – Gordon "makes a more promising effort to pin down the meaning of operationalism in economics" (Blaug, 1992, p. 89), but also because his suggestions are useful in contemporary problems of economic models. What is meant here is the problem of informativeness of economic models (Boland, 1975) and the question of their mathematical complexity (Coelho, McClure, 2005).

Gordon's analyses concentrate on functional relationships among observable variables. However, as Gordon points out, this does not mean that either only operational propositions have scientific value or that all operational propositions should have a functional form. In this respect, Gordon raises an important matter:

> "What, if any, is the operational significance of such functions? The answer is that the use of such functions must be interpreted as hypothesizing that they are stable, if they are to have operational significance. […] The operational test for the stability of a function is always its ability to predict changes in the dependent variable from changes in the independent variable" (Gordon, 1955, pp. 151–152).

When we deal with unpredictable shifts of function, this function is at least in a given moment deemed unstable. This can happen in two situations. The unpredictability of shifts can be caused by an unaccounted observable variable or by factors that are in principle unobservable, such as tastes. This differentiation is pragmatic and not purely operational because it refers to the investigator's attitude. These two situations are not clearly distinguishable. The unpredictability can be eliminated when the two missing observable variables are taken into account, which can be difficult to achieve. A starting point as defined by Gordon is undoubtedly quite strict and can make the formulation of valid propositions using functional relationships difficult or even impossible. An operationalist interpretation of economic functions emerges in this respect and this can have far-reaching methodological and semantic consequences for modeling in economics. This interpretation is not based on a presentation of functions with traditional curves but on bands of a certain width. This way of presentation is meaningful in the context of this article and was also used by, for example, Trygve Haavelmo (1939), who in doing so presented not the exact relations, but relations with errors or variations. As a result, additional factors can be taken into account in economic modeling (with its aim of defining the relations between function shifts, e.g. supply and demand), namely threshold and reaction time (Reder, 1952, pp. 185–188).[3] Obviously, the relations between those additional factors are defined on the grounds of particular models. There can be a shift considerable enough to influence something else within a short period of time or a relatively small shift which will make a visible change within a longer period of time, etc. In this way, the economic function does not simply show that if x goes up by A units, then y goes up by B units, provided that w and z are constant. Instead, there is a statement that if x goes up by A units, then y goes up by something between B and D units within the T period of time, provided that w does not change by F units and z does not change by G units.

The conclusion which Gordon drew from the considerations above is paramount. From an operational point of view, only a small number of statements of the same or similar type as above can be linked together. This can be summarized in the following statement: *few versus too many functions*. From a general methodological and philosophical perspective – the mathematical complexity of economic models is inversely related to their operationality. At this point, it may be worthwhile to quote Gordon *in extenso*:

> "the relationship between x and y may be stable long enough for a shift along that function but not stable enough for a shift along that function plus a subsequent shift along another. *With more functions these difficulties multiply in combination*. Given a specified band pertaining to a specified interval of time for each function, *the more functions there are in the system the cruder will be the valid conclusions* – that is, a 'very large' shift in an initial exogenous variable will be necessary to insure an unequivocal increase or decrease in each of the dependent variables. (On the other hand, it may be that the larger the shifts along the function the more difficult will it be, because of the possibly longer time lag involved, plausibly to hypothesize sufficient stability over time.) The important conclusion is that, although *each function in a system may be operational by itself, the combined functions may not be*" (Gordon, 1955, pp. 155–156; emphasis added).

[3] In contemporary mathematical economics, function bands are modelled with, for example, fractal geometry: "In complex systems all relationships are time-dependent and exhibit only 'demi-regularities'. […] Curve is a fractal band that takes up space not a single-valued determinants relationship. The width of the band denotes the variability of the demi-regularity characterizing different complex relationships" (Moore, 2006, p. 312).

On the basis of Gordon's considerations, it is possible to formulate a hypothesis that mathematical complexity of theoretical models is inversely related to their operationality. This hypothesis is currently researched by Philip R.P. Coelho and James E. McClure. In their series of articles (2005, 2008, 2011), they presented content analyses of prestigious economic journals (among others, "American Economic Review", "Economic Journal", "Quarterly Journal of Economics", "Journal of Economic Theory"), and their results support Gordon's hypothesis (or at least they do not disconfirm it). They observed a trend of increase in professional articles including complex mathematical models at the expense of models with operational meaning. One of the indicators of this trend is a frequent use of the term 'lemma' in these publications, and the authors called it the *market for lemmas*. More pressure is put on the inner coherence of the models rather than on their operational interpretation based on current or historical empirical data. Coelho and McClure provide important statements which develop Gordon's hypothesis:

– complex economic models rarely operate in empirical world (Coelho, McClure, 2008, p. 78),
– there is a need for "*the 'appropriate' balance between mathematical complexity and operationalism*" (Coelho, McClure, 2011, p. 213; emphasis in original).

It has to be clearly stated that these analyses are not aimed against the mathematization of economics. The problem relates to the way of using mathematical theories in economic modelling, particularly whether economists intend to confront mathematical models which they have constructed with economic reality. The mathematization of economics *per se* is not a problem as long as it does not lead to theoretical emptiness and separation of economics from real economic problems. On the other hand, theoretical insights produced by economic models should be applied to these problems in a modest way (Hardt, 2016a, p. 284). Tradeoff between operationalism and the mathematical complexity of economic models is also crucial for their semantics. As Jan Żytkow puts it in his article with a meaningful title *Scientific Modeling: Round Trips to Many Destinations*:

> "the process of model creation […] oscillates between solvability of equations and adequacy of description. An acceptable model is simple enough so that the equations of the model can be solved and complex enough to provide an adequate description of the investigated phenomenon" (Żytkow, 1995, p. 179).

The above oscillating character of modeling shows the need to take into account two goals which will not be easily achievable in practice. In Gordon's view of operationalism, this is related to the width of economic function band: "If economic functions are interpreted as bands, the widths must be specified to make them operational […]. [I]ts usefulness will depend upon how narrow the range is, while its accuracy is likely to depend upon how wide it is" (Gordon, 1955, p. 154). An increase of empirical accuracy by taking into account numerous additional factors influencing changes of an economic size ('expanding the band') can make an analysis with the use of a given function useless (Reder, 1952, pp. 188–189). On the other hand, limiting oneself to the condition of solvability leads to the emergence of uninformative economic models with no empirical content (Boland, 1975, p. 27).

Let us proceed to Fritz Machlup's well-known criticism of operationalism. In retrospect, one can say that his criticism did not undermine the validity of operationalism in economics although it uncovered some of its considerable limitations in its original version. Machlup considered a situation outlined with the following question: 'What consequences could be expected from the imposition of a tax on imports?' The concept of tax on import is an operational concept, which is clearly highlighted by Machlup. However, this is not enough to say that the theoretical arguments meant to answer the aforementioned question are operational in nature. This happens because some of the concepts presented in arguments have operational counterparts, and others have not. And so, according to Machlup, terms such as 'domestic prices of imports', 'physical volume of imports', or 'quantity of export demanded' can be operationally defined. Whereas terms such as 'foreign supply of import' or 'domestic demand for imports' cannot be operationally defined because they are purely theoretical. Therefore, the following conclusion can be drawn:

> "[w]hat this exercise has shown is that for *some* of the concepts used in the theoretical argument operational counterparts are available; for *others* they could be obtained if it were really necessary; for a *third group* they could not be obtained even with the greatest expense and ingenuity; but that in the theoretical argument itself *all* concepts were pure constructs, not operationally but nominalistically defined" (Machlup, 1966, p. 65; emphasis in original).

The use of quantifiers is undoubtedly crucial to the meaning of the above statement. However, this refers to the Machlup's general attitude towards operationalism which considerably influenced further development of operationalism in economics. The quantifiers can be summed up in the following way: each/some or – equivalently – only/not only. Of course, this is all about the firm standpoint of classic operationalism, according to which only operational terms are legitimate in science or that every scientific term should have an operational meaning. However, such a standpoint can no longer be sustained and even such a declared economic operationalist as Gordon did not support it. Still, there seems to be a consensus that "for *some* of the constructs empirical counterparts will have to be suggested" (Machlup, 1960, p. 572; emphasis in original). If so, then is it not a bit premature to state that in the theoretical argument all concepts were pure constructs? This was suggested by Bruce Caldwell (1994, p. 192) who claimed that "such a conclusion is not a necessary consequence of his argument; other conclusions are possible." In time, Machlup's criticism does not question the validity of operationalism but indicates the limitations of its early versions.

Moreover, Machlup at least partially agreed with Bridgman's basic and controversial semantic postulate:

> "the rule 'different operations – different concepts' would indeed be a nuisance. […] The trouble, I submit, lies in the failure to distinguish metric concepts – measurable physical or statistical quantities – from concepts of sensory or imagined objects. In the case of numerically determinate quantities it is perfectly proper to insist that different metric operations yield or imply different concepts. But in all other cases different operations may point to, or identify, the same object" (Machlup, 1960, p. 560).

Machlup did not question the rule of 'different operations – different concepts', although he limited its use. This rule is to be used in measurement and statistical procedures, which is undoubtedly meaningful to the semantics of economic models. Moreover, Machlup expands on this topic and comes up with another statement of paramount importance in economic semantics:

> "the constructs of the model which have *any* operational counterparts usually have *several* such counterparts, each deficient in some way, deviating from the exact (ideal) construct for which it can be only a poor analogue" (Machlup, 1960, p. 573; emphasis in original).

Another piece of evidence which proves the importance of Machlup's criticism in the development of operationalism comes from the fact that Julian L. Simon used operational analysis in his semantic considerations on economics. Simon not only paid scientific homage to Machlup but also incorporated Machlup's criticism in his own version of economic operationalism. Simon applied operational analysis to study the meaning of many economic terms, such as 'utility', 'causality' and 'product differentiation'. In the course of the analysis of the last term, Simon reached some interesting conclusions pointing out to the fact that the use of this term in economic discourse is often confused and misleading. He states that the definition of product differentiation that leads to a preference for one variety of the product over another is defective from an operational point of view. Such a definition of product differentiation shows only one way to check operationally if the product is differentiated:

> "is a test of whether there is consumer preference (*i.e.,* whether a change in the seller's offering is accompanied by a change in preference). If differentiation and preference are equivalent in the sense that there is only one measurement for the two concepts, then preference and differentiation are, for all scientific purposes, equivalent. […] the *same* measurement constitutes the definition of both" (Simon, 1982, pp. 675–677; emphasis in original).

Thus, J. Simon inversely applies Bridgman's dictum: 'different operations – different concepts', hence 'the same operation – the same concept'. Therefore, this is yet another variation of Peirce's pragmatic maxim: "there is no distinction of meaning so fine as to consist in anything but a possible difference of practice" (Peirce, 1878/1955, p. 30). Precisely speaking, Bridgman's line of reasoning was as follows: if there are different operations defining the same term (e.g. 'length'), then there are different concepts and different terms should be used. However, according to Simon's analysis, if there is one operational definition applied to two different terms, then the concept is the same and, as a result, one of the terms is redundant. Due to operational equivalence and synonymity of the terms 'product differentiation' and 'consumer preference', the former should no longer be used as there is less basic one. The use of the term 'product differentiation' may lead to confusion (Simon, 1982, p. 676). However, it is not yet clear whether Simon fully agreed with Bridgman's dictum 'different operations – different concepts'. Such a conclusion can be drawn from the following statement:

> "there are several possible measures of price or cost, and it is not obvious which is the best one to look at – cost in man hours, total expenditure as a proportion of GNP, price relative to wages and price relative to other goods. The choice should depend upon one's purpose, but *luckily all of them tend to show much the same result*" (Simon, 1982, p. 696; emphasis added).

Obviously, the last fragment of the statement above is intriguing as the use of the word 'luckily' is slightly enigmatic. But if the above statement is juxtaposed with Machlup's one in the perspective of economic semantics, then important semantic postulates can be obtained:

– the elements of an economic model which have *any* operational counterparts usually have *several* ones (Machlup);
– (luckily) *all* of them tend to bring much the same results (Simon).

As it will be presented in the next section of this article, these postulates are realized in procedural semantics. However, before this happens at the end of this section, it would be worthwhile to mention some crucial points of Ben Seligman's criticism of operationalism. The key idea of his criticism is the reference to Niels Bohr's principle of complementarity as "perhaps the most useful antidote to the self-assurance of operationalism" (Seligman, 1967, p. 155). The aim of complementarity is to oppose bringing economics down to the field of what is measureable. Apart from the operational dimension, there is also the complementary interpretational dimension which is equally important. Operational analysis has to be complemented with understanding and comprehension. Methodological complementarity postulated by Seligman is quite important in the discussion of operationalism in economics and can be applied to the aforementioned Machlup's premature conclusion. From the complementary perspective, one can say that Machlup made economic concepts too theoretical, and the conclusion regarding the complementarity of operational concepts and theoretical constructs would be consistent with the arguments of the author of *Economic Semantics*. Moreover, this type of complementarity is postulated on the grounds of Żytkow's procedural semantics, which will be discussed in the next part of this article.

However, Seligman investigates complementarity not only in the methodological aspect but also in the epistemological and semantic one and he calls this 'ambiguity of complementariness'. Seemingly contradictory explanations of economic phenomena are then allowed. In this respect, Seligman refers to Nicholas Georgescu-Roegen's dialectic concepts:

> "[a] vast number of concepts belong to this very category; among them are the most vital concepts for human judgements, like 'good', 'justice', 'likehood', 'want', etc. […] *they are surrounded by a penumbra within which they overlap with their opposites.* […] we must accept that *in certain instances* at least, 'B is both A and non-A' is the case" (Georgescu-Roegen, 1966, p. 23; emphasis in original).

By referring to the argument above, we could ask whether we deal with 'B is both A and non-A', or 'B is neither A, nor non-A'? The latter option is closer to the operationalist perspective. As far as empirical predicates are concerned, there will always be a certain *region of indeterminateness* (Carnap, 1936, p. 445) as there are cases when neither a given predicate nor its negation can be attributed. These can be situations relating to the past, where there is a lack of accessible data concerning certain events. We can therefore talk about semantic complementarity without having to engage in contradictory concepts.

3. PROCEDURAL SEMANTICS: AN OUTLINE OF BASIC IDEAS

Jan Żytkow's intention behind his construction of procedural semantics was to overcome the notorious ambiguity which appears on the grounds of classic operationalism – different operational procedures define different concepts and one should use separate terms. In Żytkow's semantic construction, an empirical term is defined by a collection of operational procedures instead of a single one. If different operational procedures are related to a given scientific concept, then it is possible to use the same term as long as the collection of these procedures is coherent. Otherwise, the defined concept becomes ambiguous (Żytkow, 1984, p. 488). In this way, one can avoid the problematic 'concept proliferation' which can lead to an excess of mutually unrelated concepts and make the language of science too complicated. In the context of economic semantics, it would be valuable to compare the starting point of Żytkow's methodological-semantic strategy with Julian Reiss's diagnosis of the possibility to apply operationalism to the index-number problem. According to Reiss, operationalism:

> "contains an important grain of truth relevant to the index-number problem. Different measurement methods tend to have idiosyncrasies each of their own, and it is a fallacy to think methods measure the same concept just because we attach the same name to them […]. Operationalism overshoots its target because it tells us to regard concepts associated with dif-

ferent measurement procedures as different independently of whether or not the different procedures yield the same results. But it is right to warn us not to unwittingly use conflicting procedures unless we have investigated the behaviour of these procedures" (Reiss, 2008, p. 71).

Thus, Reiss appreciates the critical potential of operationalism, and the aforementioned 'overshooting of the target' definitely relates to classic operationalism without touching upon Żytkow's semantics. Let us recall Bridgman's controversial remark included in his famous considerations regarding different measure procedures interpreting separate concepts of length (tactile length and optical length). When two procedures of measuring length are available, then using identical names in both cases is practically justified as long as it is not discovered that these procedures do not bring different results within the known scope of their application. Within the margin of error, two different procedures should bring the same results in their common range of application. Yet Bridgman claimed that principally when procedures are changed, a conceptual change also takes place and the use of identical terms for different concepts over the entire range is required for the sake of convenience that can be achieved at cost too high in terms of unambiguity (Bridgman, 1927/1948, pp. 16, 23). In this respect, Reiss was right in saying that classic operationalism overshot the target. A similar thought was expressed by Carl Gustav Hempel, according to whom classic operationalism directly "overemphasizes the need for an unequivocal empirical interpretation of scientific terms" (Hempel, 1966, p. 93). This remark can also be applied to the index-number problem. As Reiss puts it:

"We may, of course, investigate the empirical behaviour of COLI and HICP measures and find that *they do not systematically differ. Or we may find that they do differ but not enough to matter in the context of specific inquiry*. But this is a question regarding a contingent fact about the world and hence requires empirical investigation" (Reiss, 2008, p. 71; emphasis added).

In this respect, Reiss and Żytkow's views somehow overlap. *A limine*, one can declare neither accordance nor discordance of results of different operational procedures, let alone the degree of accordance or discordance. Semantics cannot *a priori* settle the identities of concepts. Some more 'empirical job' is required in this case. Żytkow expresses this matter in the following way: "not putting up with the limitations of a set of considered concepts, not giving up too soon the search for hidden connections between subranges" (Żytkow, 1984, p. 492). Before we move on to detailed analyses of semantics of economic models from a procedural perspective, it is important to mention some of Mary Morgan's general philosophical and methodological features of economic models which advocate the accuracy of their interpretation. Żytkow's accomplishments are important in this respect, as well as the work of other followers of procedural semantics.

In her famous study of economic models, Morgan distinguishes their several specific features. First, it is always important to remember that economic models exist in the world and as so for the users they are modes of action. In this respect, economists work with models and even "they move to a point where they no longer use those models to interpret the world, but they see those models at work in the world – the point at which model-designed interventions seem natural" (Morgan, 2012, p. 406). In such a way, a proposition of semantics of economic models should take into account this activistic aspect of economic models. Procedural semantics fulfils this task. Jan Żytkow and Andrzej Lewenstam highlight this matter strongly in an article on modeling in analytical chemistry. In this respect, they oppose semantic reductionism according to which all concepts should be brought down to primitive concepts, and primitive concepts should be reduced to basic laws which act as axioms. However, this leads to a basic problem of the interpretation of formalism – the question of setting the intended interpretation and eliminating the unintended ones. Żytkow and Lewenstam offer an alternative view in the form of defining scientific concepts in terms of operational definitions based on actions and observations:

> "[t]hese types of definitions, which are usually called operational procedures, are equally important and *complementary to theoretical definitions*. Rather than reducing other terms to the primitive terms of basic theories, operational definitions reduce scientific concepts to the basic classes of actions and observations that the cognitive agent, the experimenter, is able to carry out" (Żytkow, Lewenstam, 1990, pp. 226–227; emphasis added).

The advocates of semantics of operational procedures do not treat procedural interpretation as competitive to theoretical one as they are both complementary. Undoubtedly, this remains in line with the interpretation of operationalism in economics set forward by Seligman. Another important feature of economic models pointed out by Morgan refers to their degree of abstraction or detail. In this respect, she assigns an intermediate status to economic models.

> "Modelling […] provides economic science with lots of 'middle level stuff': in-between, generic-level accounts of what economists take to be typical in economic life rather than descriptions of particulars or very general accounts. Models result both from dividing general accounts and gathering particular empirical cases together" (Morgan, 2012, p. 394).

This important feature of economic models has a crucial influence on the construction of their semantics. This means then that this semantics can neither be a formal abstract account nor can it be a fully detailed description of particulars. In this respect, procedural semantics turn out to be promising because operational procedures have an intermediate and hybrid character. The key claim of the advocates of procedural semantics is that referential semantics is too general and abstract to provide satisfactory view of the mechanism which links the language with perceptive activities and non-verbal actions. A semantic interpretation which is founded upon and abstract function of denoting (as in referential semantics) is treated as a 'black box'. There is no access to the internal mechanism of semantic interpretation and only the number of inputs and outputs is available. The procedural approach is meant to remove the limits of the referential approach because procedures have a fixed structure. Therefore, semantic interpretation is based on the concept of procedure instead of the concept of function. According to the set-theoretic approach, function is a mapping, and the same mapping can be obtained through a potentially unlimited number of procedures (Woods, 1981, pp. 329–331; Duží, Materna, 2010, p. 218). In this respect, the abstract referential approach does not fulfil the tasks of semantics for models. On the other hand, an overdetailed approach can also be undermined. As far as procedural issues are concerned, the problem refers to the question of the *specification of procedures*. This problem is also present in classic operationalism. One of Bridgman's most important postulates described in *The Logic of Modern Physics* was a statement that every procedure should *in principle* be *uniquely specified* (Bridgman, 1927/1948, p. 10). This postulate has become controversial both in the past and more recent literature on this subject. The main concern is that by realizing this postulate, one can get involved in an infinite process of specification of a given procedure, which can be unacceptable in practice. Thus, it is necessary to stick to one general characteristics of procedures (Chang, 2004, pp. 222–223). In his later writings, Bridgman paid attention to the fact that no procedure can be unambiguously specified in every detail – it is not possible to come up with an absolutely precise and unambiguous specification of procedures. One should assume that apart from a certain *degree of refinement*, a further specification of procedure is not required. Some details are treated as insignificant (Bridgman 1950, p. 10). Therefore, an important conclusion can be drawn for the development of procedural semantics. Resignation from an interpretation based on an abstract denoting function – a certain marginal view of semantic interpretation – cannot lead to a completely opposite situation, i.e. a statement that each detail of the procedure of interpretation is an element of the meaning of an interpreted term. Fully specified procedures are idiosyncratic – private and unique. As a result, special attention is paid to the fact that on the grounds of procedural semantics procedures are abstract, which means some of their elements from the lower level are treated as insignificant to the meaning of a given term (Woods, 1986, p. 59).

In Żytkow's procedural semantics, the matter discussed in this article is handled in a very precise way. He distinguishes two types of procedures:

- type-? – their use to a given number of objects implies a yes/no answer or a certain numeric result;
- type-! – their use creates an object or some state of affairs.

On the other hand, the concept of procedure is understood as a finite string of instructions. More specifically, *it is a set of commands and questions* which end with a terminal instruction determining the result of the procedure. Procedures are carried out on data – the realization of procedures is a transition from initial data to the final ones and it is possible due to specific instructions. Therefore, one can distinguish a set of initial data D_0, whereas realized procedures bring sets of final data D_t. An important point for this discussion is distinction:

- procedure-scheme;
- procedure;
- realization of the procedure (Żytkow, 1982, p. 173).

The *scheme of procedure* defines types of instructions (questions, commands, conditional and terminal instructions) and their order. *Procedure* contains particular functions and operations, but there are also variables, while in the *realization of procedure* is supplied with constant and particular numeric values in place of variables present in the procedures. As it can be seen, operational procedures are intermediate and stretched between abstract schemes of procedures and their detailed realizations. Naturally, it is possible to go beyond the scheme of procedure in the process of abstraction up to the point where an abstract, 'bare' denoting function with no internal structure is obtained – a function which is a pure mathematical mapping. Similarly, it is possible to reach such a degree of detail in the specification of procedure realization where this becomes absurd (e.g. researcher's shoe size or culinary preferences). All in all, there is no preferable and definite solution to this problem. When 'middle level stuff' is handled, then the localization of this level between the outlined extremes will frequently be determined by pragmatic factors.

As it has already been mentioned, a set of operational procedures defines the meaning of interpreted terms on the grounds of Żytkow's procedural semantics. Żytkow assigns this set with the following conditions:

- *consistency* (or empirical equivalence) – two procedures are consistent if and only if, they apply to the same objects and they give the same results within the limits of error;
- *coherence* (or *nondisjunctiveness*) – set of procedures is coherent if and only if, all its subranges are connected with one another by sequences of overlapping subranges (Żytkow, 1984, p. 482).

In such a way, basic concepts of procedural semantics are obtained: procedures, data and conditions assigned upon sets of procedures. The fact that a set of procedures interprets a given concept agrees with Machlup's aforementioned statement: *if some, then several*. However, it is crucial here to define the relations between the results of different procedures. But this is already an empirical issue and although it may turn out that – as J. Simon claimed – those results will appear luckily consistent, where the word 'luckily' can be understood as a contingent of circumstances.

What if 'unluckily' there is a situation of inconsistency or incoherence? Let us return to the question of indices. The problem of inconsistency emerged in axiomatic index theory. It turns out that Irving Fisher's criteria called 'tests' and set upon indices cannot be fulfilled simultaneously because this would lead to inconsistency. A circular test which lead to inconsistency appeared to be crucial, so in order to obtain consistency one had to stop using it (Boumans, 2012, pp. 399–401). But even when faced with inconsistency, Fisher did not reject this test and explained this in the following way:

> "the important question is: *How near* is the circular test to fulfilment in actual cases? If very near, then practically we may make some use of the circular test as an approximation even if it is not strictly valid" (Fisher, 1922, p. 276, emphasis in original).

> "In short, while *theoretically* the circular test ought not to be fulfilled, and shifting the base ought to yield inconsistencies, the inconsistencies yielded are so slight as *practically* to be negligible" (ibidem, p. 303, emphasis in original).

Thus, Fisher's attitude to index numbers is different from the axiomatic one, and therefore Marcel Boumans calls it instrumental. It is about "finding the best balance between theoretical and empirical requirements, even if these requirements are incompatible" (Boumans, 2001, p. 316). This points out to a quite crucial aspect as to what extent empirical inconsistencies are acceptable – to what number and degree? Perhaps we are simply doomed to inconsistencies, as Bridgman suggested: "I personally do not believe that there is any consistent method for dealing with the complete situation, but that we are forced to a *spiralling approximation or to operation on different levels*" (Bridgman, 1959, p. 77; emphasis added). This issue is definitely very important for methodology of economics. However, it goes beyond the scope of this article.

4. ECONOMIC MODELLING IN PROCEDURAL PERSPECTIVE: THE CASE OF FISHER'S CASH LOOP MODEL

Żytkow was particularly interested in scientific modelling. He also simultaneously worked on the development of his version of procedural semantics and its applications. He considered modelling and construction of operational definitions as distinct scientific activities yet not independent from each other. The construction of operational definitions along with experimenting *supports* scientific modelling (Żytkow, 1999, p. 311). In order to illustrate this fact, Żytkow used Galileo's reflections upon the movement of a ball on an inclined plane. The key element of Galileo's experiment was the fact that he was unable to measure the exact time when the ball would touch the ground. However, he could indirectly measure the final velocity of the ball. To be able to do this, Galileo attached a 'launching pad' to the bottom of the inclined plane. The ball would reach the edge of the pad and next fall to the ground and reach the *p* point. One could then measure the distance between the *p* point and the *q* point – the bottom of the slope. Galileo used this result to calculate the velocity of the ball at the bottom of the slope and came up with a model in the shape of an adequate empirical equation. For Żytkow, this is an example of an operational definition (in this case used for the concept of velocity) and at the same time an instance of the application of the model (*ibid.*, p. 315).

Similar situations can be found in economic sciences. To show this, it will be useful to outline Żytkow's explication of modelling perceived as a *multilevel feedback process*. He distinguished five steps in modelling. When dealing with an object, a process or a phenomenon *O*, one should:

(i) "make a listing of objects, properties and processes present in *O;* decide which empirical parameters *P* of *O* we want to explain and which we can measure in *O;* a modelling task depends on how much of the modelled objects we want to represent in the model" (*ibid*., p. 317);

(ii) create a model-diagram that captures interaction in *O*;

(iii) construct a model-formalism corresponding to the model-diagram;

(iv) simplify and augment the equations until solvable;

(v) verify the solution against empirical data for *P* measured for *O*.

It is important to mention that one does not deal here with a simple one-way sequence of stages. Modelling goes through many feedback loops. This happens when a solution to a problem specific for a given stage requires corrections on a preceding one. Obviously, failures at the last stage need corrections at the earlier stages. It is important to mention that there are models which do not contain formal components and instead come (only) as diagrams. They can be then referred to as *informal qualitative models* (Gordon, Sleeman, Edwards, 1995).

Let us use Żytkow's explication of modelling to reconstruct Irving Fisher's Cash Loop Model (CLM). In Fisher's original article from 1909, three stages of modelling are distinguished:

– first approximation;
– the complete formula;
– statistical application.

In the first (*nomen omen*) approximation, we can say that Fisher's first stage is equivalent to (i) and (ii) in the above explication; the second stage corresponds to (iii) and (iv), and the last two stages are mutually equivalent. What is important is that there are many loops in CLM construction, which will be presented further. At that moment, two essential remarks have to be made and they refer to the outermost stages of modelling in the context of the methodology of economics. Let us begin with stage (v), where a well-known and difficult problem of testing economic models emerges. However, when the last stage of CLM construction is taken into account, then in the context of the present problematics the pressure is put not on testing the model itself (which is frequently infeasible), but on the *test of its application*. As Francesco Guala claimed:

> "[w]hat you can do, though, is to test an *application* of a model, a hypothesis stating that certain elements of a model are approximately accurate or

good enough representations of what goes on in a given empirical situation. […] A model is always useful to a degree, as long as it is applicable to *some* situation […] The fact that a model turns out not to work under certain circumstances does not count as a refutation of the model but only as a (failed) test of its applicability in a given domain" (Guala, 2005, pp. 219–220, emphasis in original).

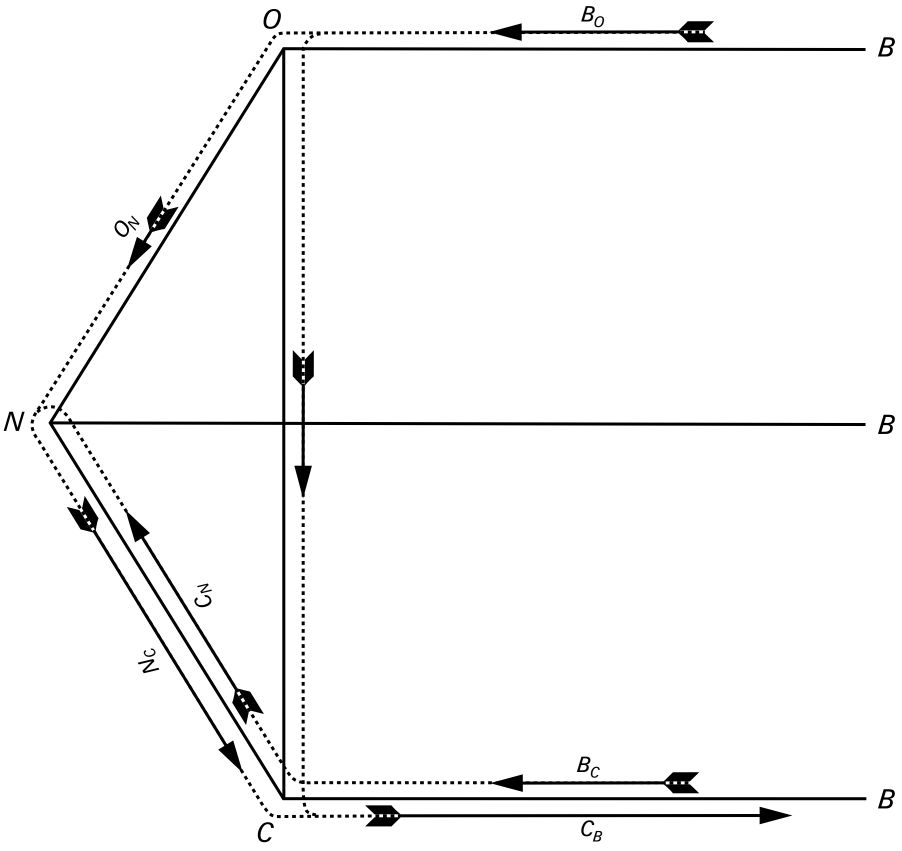
If we consider the fact that in the procedural approach there is both application and operational definition, then the above statement is in line with Bridgman's approach. He claims that if operations are used for a particular purpose, then instead of talking about good or bad operations, one should describe them as either useful or non-useful (Bridgman, 1945, p. 248). When it comes to Żytkow's first level, important observation should also be made. When researchers make inquiries into empirical phenomena, the phenomena are not directly given to the researchers as parts of the empirical reality, but they are given as objects of inquiry and as such they are always investigated with the help of a certain conceptual apparatus. A prior conceptualization of the field of research appears (Wójcicki, 2002, p. 33). The result of conceptualization is *conceptualized empirical system* which cover measurement points, time lapses and parameters used to describe a given phenomenon. There is *range*, *period* and the *characteristics* of an empirical system. The parameters chosen to describe a phenomenon can be both quantitative and qualitative. As Ryszard Wójcicki points out, the conceptualization of a phenomenon:

- restricts area of interest and makes it definite;
- suggests certain way of conceiving the phenomenon (Wójcicki, 1979, p. 38).

Strictly speaking, it is the conceptualization which defines the object of research from the formal point of view. The same object of interest can be conceptualized in different ways. On the other hand, two separate phenomena conceptualized in exactly the same way are considered identical. In the methodological perspective, conceptualized empirical system is the target and it is modelled. One could ask a question: what is the point of constructing theoretical models if systems already conceptualized are their targets? An important answer comes from Paweł Zeidler: "When the empirical system is conceptualized, then the construction of its model consists in a *determination of the relationships between the parameters* used for the conceptualization" (Zeidler, 2013, p. 61, emphasis added). This determination can take up a form of adequate equations, but it can also become iconic and presented through diagrams.

Let us proceed directly to the CLM reconstruction. The aim of Fisher's modelling was "tracing the circulation of money, and measuring it by bank records" (Fisher, 1909, p. 605). On the conceptual level, he divides people into three groups: commercial depositors C (e.g. firms, companies), other depositors (e.g. proprietors) O and non-depositors N (wage earners). However, he also admits that this division is not comprehensive as it does not include the subjects whose part in money circulation is negligible (e.g. street traders). Fisher also introduces a demarcation line between *C*'s and *O*'s, which separates business self from personal self (e.g. John Smith Shop, John Smith). Transactions happening between these two 'selves' are treated the same as normal transactions taking place between companies and different people. As Fisher clearly admits: "Where such a person withdraws money from his till and puts it in his pocket, we may say his business self has paid his personal self some dividends of the business" (*ibid*., pp. 606–607). Therefore, this is a clear conceptualization of the target of research. The last element of conceptualisation are banks which are observation posts – places where payment flows are registered (Morgan, 2007, p. 117). Thus, it is possible to say that banks become measuring points.[4] Next, Fisher moves on to stage (ii) and introduces Cash Loop Diagram.

Figure 1. Cash Loop Model Diagram



Source: Figure 1 from I. Fisher *A Practical Method of Estimating the Velocity of Circulation of Money* (1909), p. 608.

[4] For the sake of completeness, it is necessary to mention the metaphorical aspect of Fisher's conceptualization. Banks are homes for money and its circulation is a temporary excursion beyond. To learn more about metaphorizing in economic modelling, see: Hardt, 2016b.

In the next step, there is first feedback and first precise clarification of conceptualization:

> "As already indicated, money may be said to circulate only when it passes in exchange for goods. Its entrance into and exit from banks is a flow but not circulation. In the diagram the horizontal arrows represent such mere banking operations, not true circulation. The arrows along the sides of the triangle, on the other hand, represent actual circulation" (Fisher, 1909, p. 608).

As a result, there are four types of circulation, i.e. exchanges of money against of goods or services: O_C, O_N, C_N, N_C. Together with the remaining non-circulative (B_O and C_B) they create three circuits:

- B_O, O_C, C_B, B_O;
- B_O, O_N, N_C, C_B;
- B_O, C_N, N_C, C_B.

An important issue here is the fact that money circulates in the first circuit once out of bank, then circulation O_C is essential and it is not present in the remaining circuits, and the whole cycle is closed with C_B. Whereas in the two remaining circuits money circulates twice out of bank, because it is paid for wages. The role of intermediaries in the form of non-depositors is crucial here. Payments to *N*'s flow straight to commercial depositors, so money circulates twice before it returns to bank. It is very important for Fisher's first approximation:

> "the total circulation exceeds the total flow from and to banks by the amount flowing through 'Non-depositors'. In other words, the total circulation in the diagram is simply the sum of the annual money flowing from and to banks and the money handled by 'Non-depositors'. The quotient of this sum divided by the amount of money in circulation will give approximately the velocity of circulation of money" (Fisher, 1909, p. 609).

At this point, there is a transfer to the third stage of Żytkow's modelling, which is creating an equation for the time being presented in an ideational form. At the same time, there is feedback towards final conceptualization. There are two types of measuring points: the number of *B*'s transactions and *N*'s transactions.

In the part called 'complete formula', one can find Żytkow's stages (iii) and (iv), which are perforce connected with feedback. There is also feedback to stage (ii) because Fisher introduces a certain additional variant of an initial diagram. This stage of Fisher's modelling has three main components: the formulation of complete and exact formula of total monetary flow (*F*) in exchange of goods (all possible transfer within given community), the formulation of an algebraic form of the first approximation and – which is crucial – their mutual comparison which "will express the error of the first approximation, and will suggest a method of transforming the exact formula into a shape more suitable for statistical application" (p. 610). This is stage (iv) – 'simplify and augment the equations'. Formulas for F and the first approximation (F') have got the following shape:

$$F = O_c + C_O + N_c + C_N + O_N + N_O + C_c + O_O + N_N,$$
$$F' = C_B + O_B + N_B + N_C + N_O.$$

After adequate transformations and amplifications, Fisher establishes the remainder $r = F - F'$, which first and foremost sums up the following:

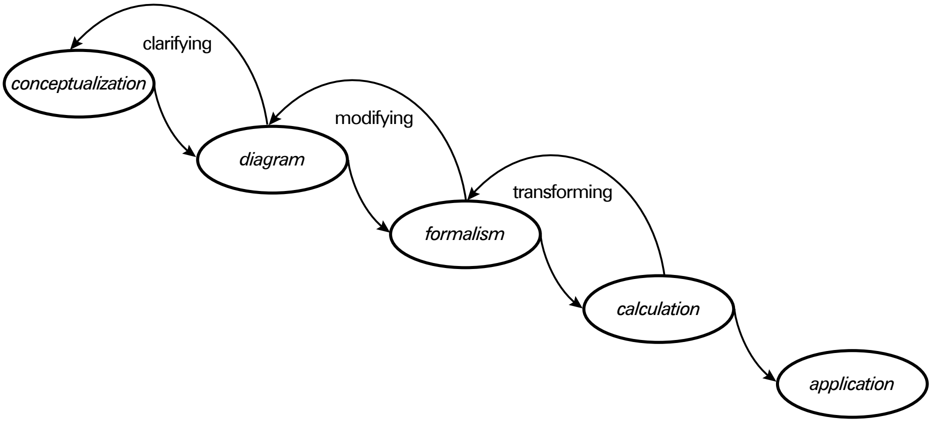
- $[(C_O + C_N) - B_C]$, C's till-paid commercial expenditures, not withdrawn from bank;
- $[(C_O + N_O) - O_B]$, O's money receipts pocketed, not deposited in bank;
- $(C_c + O_O + N_N)$, intraclass monetary flaws.

Fisher estimates that in his times the share of remainder r in the USA is rather insignificant and it contributes less than 10% of the total. In this way, an equation for the velocity of circulation V is obtained, where M stands for the amount of money in circulation:

$$V = \frac{\left(C_B + O_B + N_B\right) + \left(N_C + N_O\right) + r}{M}.$$

It is crucial that the part of numerator (denoting bank deposits) is, as compared to others, measureable in the greatest extent. The second part denoting expenditures of non-depositors is also measureable but to a lesser extent, whereas the remainder r is conjectural. At the last stage (v) there is an application of the formula for V. Fisher used statistical data from 1896 available in the USA and calculated that velocity of circulation was 18.6 times a year. Whereas V for the year 1909 was 21.5 times a year. In other words, it was once in 17 days (Fisher, 1911, p. 289).

Figure 2. Fisher's modelling as a multilevel feedback process



Source: own elaboration.

As it can be seen, Fisher's modelling can be reconstructed as a multilevel feedback process. Moreover, there is also an application of a model together with its operational definition as it was in Galileo's case. Galileo was unable to measure the final velocity of the ball because exact and direct procedures of measuring time were not available. He measured the velocity indirectly on the bases of the distance between the point where the ball fell and the bottom of the slope. Obviously, Fisher was also unable to measure the velocity of currency payments directly. Of course, this is a very basic problem. When M_d (demand deposits) is available, then estimating velocity V_d is possible, and M_d and V_d are treated as variables which are in principle observable. As far as currency is concerned, only M_c (stock of currency) is documented well, whereas the estimation of velocity of currency circulation – V_d – poses difficult and serious problems (Cramer, 1989, pp. 328–330). Fisher's model provides an operational procedure used to estimate the value of velocity of circulation of currency, although it is *rough and indirect* (Boeschoten, Fase, 1989, p. 319). In this respect, Morgan synoptically describes Fisher's way of research and his modeling as:

> "concerned with classifying all the relevant payments that he wanted to make measurable and then relating them, mapping them, in whatever ways possible, to the payments that he could measure using the banking accounts. He used the visual model to create the mathematical equation for the calculation using the banking statistics, and this in turn used the flows that were observed (and could be measured) in order to bootstrap a measurement of the unobservable payments and thus calculate a velocity of circulation" (Morgan, 2007, pp. 117–118).

The four types of circulation are: O_C, O_N, C_N, N_C. In order to estimate the value of velocity, one had to "add the amount of money annually withdrawn from bank to the annual money wages" (Fisher, 1911, p. 474) and correct this sum with the conjectural value r. Wages are a sum ($O_N + C_N$), while the first component is ($C_B + O_B + N_B$) which is a sum of flows which are not circulations. This is the most measureable part of the formula for V thanks to which unobservable payments, i.e. ($O_C + N_C$), are estimated. Therefore, Fisher point out to the sums of these circulations indirectly – through measureable flows which are not circulations. If we compare this with Galileo's example which was analyzed by Żytkow as a benchmark, we will see considerable similarities because there is an application of the model together with an operational definition. In Fisher's own words: "entrance and exit of money at banks, being a matter of record, *may be made to reveal* its circulation outside" (Fisher, 1909, p. 604; emphasis added).

A model alternative to CLM was proposed by Robert D. Laurent in 1970s and was based on using the redemption rates of worn-out banknotes of different denominations. The basic principle behind this model is that "all notes of currency are redeemed if, and only if, they have performed a constant number of transfers" (Laurent, 1972, p. 1173). The obtained results then point out to the fact that the velocity of circulation increases with the decrease of denomination. Several years later, Fisher's much older model was reused. Interestingly enough, the combination of CLM model and Laurent's model used by different researchers brought results for the same country which were "surely comparable" (Boeschoten, 1992, p. 37). The following estimations of currency velocity were obtained in the Netherlands: 15.7 in 1977 (J.S. Cramer–Fisher's model), whereas in 1980 W.C. Boeschoten and M.M.G. Fase (combination of Laurent's and Fisher's models) obtained 15.2. Thus, this can be interpreted as obtaining a consistent set of operational procedures interpreting the concept of velocity of circulation. Moreover, results obtained by Fisher and his followers may lead a quite optimistic conclusion: "These results suggest that currency velocity is a constant, as if it were set by physical limitations to the speed of currency circulation, and that it lies between 15 and 20" (Cramer, 1989, p. 331). Obviously, on account of the aforementioned problems, this does not eliminate the difficulties with estimating the value of V_c. However, this provides grounds for Fisher's results to be treated as partial definitions of the velocity of circulation. An obvious reason for this is the fact that pioneering research of the velocity of circulation has established certain canonical ways how to use this term. However, the meaning of this theoretically and practically intriguing term does not end in the operational procedures of estimating its value. Fisher was fully aware of this fact. The theoretical context is crucial here and it is set by the equation of exchange.[5] The complementarity of the theoretical and operational meaning emerges here and, in this respect, Hasok Chang's perspective of interpretation of operationalism can be applied.

> "Instrumental measurement operations always need to be placed in the larger context of the assortment of various operations which give to each concept its full meaning. […] We may choose to use instrumental measurement operations to define a concept, thereby privileging them over other kinds of operations, but even then we must keep in mind that a definition is only a criterion by which we *regulate* the uses of a concept, not the expression of the *whole* meaning of the concept" (Chang, 2017, p. 29, emphasis in original).

Procedural semantics does not fully exploit the meaning of interpreted terms. The range of concepts is always open. However, operational procedures can serve as partial regulatory definitions, which offer an undoubtedly valuable support for the economic modelling. Semantic theories which postulate uniquely defined meaning can easily be undermined from the perspective of research practice. Additionally, the criticism of such maximalist theories can easily lead to a false dilemma: either there is a uniquely defined meaning or not: "That there

[5] "To those who have faith in the *a priori* proof of the equation of exchange the real significance of the remarkable agreement in our statistical results should be understood as a confirmation, not of the equation by the figures, but of the figures by the equation" (Fisher, 1911, p. 298).

is not one unique determinate meaning to any given proposition or rule does *not entail that it can mean anything, but that it has many meanings*, and 'many' does not equal 'any'" (Tyfield, 2008, p. 75; emphasis added). Procedural semantics as outlined in this article is resistant to such a dilemma.

5. CONCLUSIONS: TOWARDS REGIONAL SEMANTICS OF ECONOMIC MODELS

The aim of the article was to juxtapose referential semantic interpretation with procedural interpretation. The former is expected to define denotation of a term, whereas the latter is expected to tell how the term is to be used. This distinction is very important in philosophy and methodology of economics. Let us take into account the idealizations which often occur in the economic discourse (e.g. perfect competition, complete information, zero transaction cost). How can idealizational concepts be semantically interpreted? As Wójcicki emphasises (using the physical point as an example): "[i]f the interpretation *is not merely an abstract one*, it may not be available" (Wójcicki, 1995/96, p. 510; emphasis added). If we want to treat idealizational concepts literally, we must state that they denote nothing. As Wójcicki claims further: "As long as we are not able to define the denotations of the term of the analysed object language the interpretation offered is not referential in the right sense of the word. One way or another *it involves some procedural elements*" (*ibid*., p. 512; emphasis added). This proves the necessity of at least some elements of procedural interpretation of economic models. If idealizational concepts denote nothing in the empirical world, then one should point out the situations when, for example, information can be treated as complete, i.e. how this term can be used. Let us repeat the following: if semantic interpretation is not merely abstract as the advocates of procedural semantics point out, abstract interpretation is a pure projection with no inner structure and cannot as such be operationally analysed. As it has been mentioned before, we do not come down to fully detailed procedures. There is just a partial analysis of interpretation: "'level of operation' may be roughly characterized by the things we leave unanalyzed" (Bridgman, 1959, p. 7).

From the foregoing considerations we know that procedures operate on data. But what can be done in a situation when required data are inaccessible? There are many such situations in the real world (e.g. unregistered data referring to past events, confidential or destroyed data). This does not mean that semantics is then useless. Because procedures are represented as a structured entity, they can be used as bases for the inference of what its outcomes would have been in certain circumstances.

> "As a consequence of these accessibility limitations, it is clear that if procedures are to be taken as explications of meanings, one cannot expect to

just blindly execute them. Rather, in some (most?) cases, an intelligent inference component is required in order to *deduce useful information from the procedural specification*. This in turn dictates that the procedural specifications must be useful for more than just execution as 'black box' procedures with input-output conditions. They must have *internal structure that is accessible to inferential procedures*" (Woods, 1981, p. 325, emphasis added).

At this point, procedural semantics meets inferentialism. Interestingly, the followers of inferentialist semantics in economics (in relation to economic causal generalisations) underline the fact that meaning does not diminish in inferentialist relations. Entries and departures transitions are also crucial:

- evidential connection – what the data are expected to say when the statement is accepted;
- policy and research implications – connections with recommendations, imperatives and eventually actions (Claveau, Mireles-Flores, 2017, p. 387).

A semantic proceduralist will solve this matter easily. The first situation refers to the application of type-? procedures – measuring or diagnostic, in other words data which can be expected through the realisation of a specific procedure on data initially available. The second situation relates to the use of type-! procedures which command or recommend the realization of a specified state of affairs. Interestingly, inferentialism refers to data and actions only when the 'semantic power' of inference runs out. Conversely, procedural semantics refers to inference (inferential procedures specifically) only when data are inaccessible. In a sense, inferentialism and procedural semantics are two sides of the same coin. However, procedural semantics has a considerable advantage and, in this sense, inferentialism is its unique variety (in a situation when data cannot be reached). Particularly, this refers to transfers between targets and models. These transfers are described in the inferential view of economic models in the following way: "what the model has allowed us to do in the end is to derive some conclusions about the empirical system, starting from information extracted *from this same system*" (de Donato Rodriguez, Zamora Bonilla, 2009, p. 103; emphasis in original). Let us remind that empirical systems are subject to prior conceptualization and only then it constitutes the target. As Zeidler observes: "only a previous conceptualization of the investigated empirical systems allows to identify individual – theoretically interpreted – empirical data (measurement results) as representing specific properties of this object" (Zeidler, 2013, p. 81). We do not begin extracting information from an empirical system because information here is the point of arrival and not the starting point. This happens only when both the applications of the model and its operational definition are successful (Galileo and Fisher's models). The construction of a model includes feedback loops and also those which can influence initial conceptualization. Then the statement that this is ‘this same system’ becomes senseless from the procedural point of view. The problem of initial conceptualization, therefore, appears to be crucial to modelling in economics because no conceptualization can be taken for granted: “note that *there may not be any obvious way* to associate *Π* [phenomenon] with a specific singular empirical system” (Wójcicki, 1994, p. 133; emphasis added).

Summing up, it is necessary to mention a problem which requires separate and thorough studies – the specifics of operational procedures in social sciences and particularly in economics. This specification is crucial for the extraction of semantic and methodological peculiarities in economic sciences. Hasok Chang and Nancy Cartwright have come up with some statements which are paramount in the context of the measurement specification in social sciences.

> “For the purpose of comparisons, measures and measurement procedures are required that can be applied across locations, populations, economies, and cultures. This often results in measures that lose information – measures that are far from the best procedures that could be devised in the separate groups – and the *more local measures often give dramatically different results from the more universal ones*. Also, for theory-testing we need separate procedures that measure the same univocal concept, but *for practical use we generally need a variety of purpose-specific concepts, each with measurement procedures appropriate to it*” (Chang, Cartwright, 2008, pp. 373–374; emphasis added).

Semantic interpretation is *universal* when there are no limits set upon the particular uses of the interpreted terms. In other words, this interpretation is not limited to any particular application. Whereas semantic interpretation which is narrowed down to particular uses becomes a *local* interpretation and as a result terms will be interpreted differently in various applications. The terms will refer to different objects, although as the author of this important distinction points out, “the difference does not necessarily mean that the general ideas underlying such interpretation must be different” (Wójcicki, 1995/96a, pp. 392–393). Universal procedural semantics could prove to be remote from modellers’ purposes. However, this would be against the relevance of its application in economic modelling because – as Boumans observes – assessment of economic models as measuring instruments depends on their *validation*. The validity of the model is understood as its usefulness with respect to some purpose (Boumans, 2012, p. 420). Moreover, in empirical sciences other than physics fundamental theories are rarely at disposal, so models are built on different theories from different fields of science. Due to the lack of fundamental theoretical background, it is not possible to create a universal semantic interpretation: “interpretation of at least key terms formulating a given model […] is strictly determined by the *procedures related to the application of this model*” (Zeidler, 2013, p. 45; emphasis added). As it can be seen, the close relation between the application of models and operational procedures comes to the foreground.

Does this mean that procedural semantics should have a purely local character? It can be local, although if we consider the middle-level status of economic models and also Żytkow's semantic construction together with statements by Simon, Machlup and Reiss as well as methodological reconstruction CLM, we can conclude that there is a third possibility. Analogously to regionalist studies, we can call it *regional interpretation*. The ranges of particular operational procedures can be their own subsets, or they can overlap or also separate. The analogy may be as follows:

- the ranges of particular procedures are *regions*[6];
- the union of these ranges (regions) gives a *generalized region*;
- intersections of ranges (regions) create *subregions* (Golledge, Amadeo, 1966, pp. 15–17).

The pertinence of this analogy is supported by the fact that Żytkow uses the term 'distant' when comparing subranges of concepts.

> "Subranges are often distant, and their belonging to a range of one concept is determined by scientific theories in which this concept plays an important role. Exactness of formulation and a large degree of empirical conformation of these theories justify a *cognation of distant subranges*. […] An equivalent of scientific knowledge in a natural language can be defined as common knowledge, *social experience*, or social beliefs […]. So much weaker common knowledge can *connect distant subranges*" (Żytkow, 1984, p. 489; emphasis added).

The regional approach makes it possible to come up with a double insight into procedural semantics. On the one hand, it has an integrative character – it gives the possibility to look for connections between particular realizations of procedures, establishing relations between ranges and comparing measurement results. On the other hand, procedural semantics has a differentiating character which relates to the comparison of operational procedures in applications across cultures. Then it is necessary to look into some 'constants' specific to particular cultures. This was highlighted by Gordon who claimed that, for example, 'taste' cannot just be a variable because it would be difficult to imagine a society with no stability in this respect. Taste can then be part of the common social experience. Some parameters are treated as relatively stable, and as a result the number of available functions is limited in a given theoretical model. According to Gordon, mathematical complexity is inversely related to operationalization. Although this may *prima facie* seem counterintuitive, the necessity of taking into account certain 'cultural constants' still supports and does not limit the operationalization of models. In this respect, Gordon was critical of the economists who 'are apt to take *varying constants* for meaningful *functional variables*' (Gordon, 1955, p. 161; emphasis added). By engulfing these

[6] "two regions A and B may have the following relationships to each other: (1) they may have no common territory; (2) they may intersect; (3) A may be part of B; (4) B may be part of A; (5) they may be identical" (Rodoman, 1968, p. 45).

'varying constants' – which cannot be treated as 'fixed variables' – one makes procedural semantics even less universal. However, it can be regional, and this argument speaks for and not against its soundness.

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SEMANTYKA PROCEDURALNA A MODELE EKONOMICZNE

STRESZCZENIE

Artykuł dotyczy zagadnień z zakresu semantyki ekonomicznej. Głównym problemem jest dostarczenie zadowalającego ujęcia interpretacji semantycznej dla modeli ekonomicznych. W artykule wysuwa się propozycję, że teorią, która może dostarczyć takiego ujęcia jest odpowiednia wersja semantyki proceduralnej. Teoria ta wiąże interpretację semantyczną z poznawczo-praktyczną aktywnością użytkowników modeli ekonomicznych. Semantyka proceduralna jest filozoficznie zakorzeniona w pragmatycznej i operacyjnej koncepcji znaczenia. W artykule szczegółowo przedstawiono dyskusje nad operacjonalizmem w ekonomii oraz porównano specyfikę proceduralnego podejścia do semantyki z filozoficzno-metodologiczną charakterystyką modeli ekonomicznych. Konkretną wersją semantyki proceduralnej, która jest wykorzystana do szczegółowych analiz, jest semantyka procedur operacyjnych Jana Żytkowa. W świetle tej teorii zrekonstruowano wybrany model ekonomiczny – model transakcyjnego obiegu pieniądza (*Cash Loop Model*) Irvinga Fishera. Wskazano także na dalsze kierunki badań w zakresie proceduralnej eksplikacji semantyki modeli ekonomicznych, które uwzględniają specyfikę metodologiczną nauk ekonomicznych.

Słowa kluczowe: filozofia ekonomii, modele ekonomiczne, semantyka ekonomiczna, operacjonalizm.

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