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THE METHOD OF ONTOLOGICAL UNIFICATION IN ECONOMICS. SOME NEGLECTED ASPECTS¹

INTRODUCTION

One of the great successes of logical empiricism was its official theory of explanation presented by Hempel (1962; 1965; 1966) and commonly known as the covering law model. The idea underlying this model is relatively simple: “(...) explanation is derivation. When a scientist explains a phenomenon, he derives (deductively or inductively) a sentence describing that phenomenon (the explanandum sentence) from a set of sentences (the explanans) which must contain at least one general law” (Kitcher, 1981, 507). However, from the very beginning that model has suffered from many shortcomings, e.g., explanatory asymmetries and irrelevancies (see, Woodward, 2011). Many researchers put their efforts in overcoming these deficiencies, e.g. W. Salmon (1971) in his statistical relevance model. Unfortunately, they did not spend enough time on developing the “unofficial” model of explanation which many leading empiricists, including Hempel himself, clearly treated as a promising approach to explanation. In his 1966 paper he explains it as follows:

“What scientific explanation, especially theoretical explanation, aims at is not [an] intuitive and highly subjective kind of understanding, but an objective kind of insight that is achieved by a systematic unification, by exhibiting the phenomena as manifestations of common, underlying structures and processes that conform to specific, testable, basic principles” (Hempel, 1966, 83).

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In other words, the rule of explaining much by little should be treated as a virtue in science. That is the case also in economics as it was clearly stated by P. Samuelson in his *Foundations of Economic Analysis* (1947/1983, 7):

“Most economic treaties are concerned with either the description of some part of the world of reality or with the elaboration of particular elements abstracted from reality. Implicit in such analyses there are certain recognizable formal uniformities, which are indeed characteristic of all scientific method. It is proposed here to investigate these common features in the hope of demonstrating how it is possible to deduce general principles which can serve to unify large sectors of present day economic theory”.

There is interesting literature on various forms of unification in economics, mainly the contributions by U. Mäki (e.g., 1990; 1992; 2001; 2009c). What my study aims to do is not to review Mäki’s papers but rather to comment on some interesting issues concerning the method of unification and its use in economics in particular; the issues that are somehow neglected or are out of the ordinary treatment in the literature. Also, I do not aim to offer an in-depth and extensive study of each of them but rather to shed some light on these issues and hence make them known to a wider audience. Therefore, in the following section of the paper I make references only to these Mäki’s contributions that can serve as starting points for my own reflection. What I see as somehow neglected in the literature on unification in economics are: 1/ the issue of the structure of ontologically unified theories and the process of their formation (e.g., how explanandum sentences are *in real* derived from explanans if we assume that the mechanism of deductive inference is not in place); 2/ the problem of the nature of ontologically unified theories, i.e., their relation to their predecessors; 3/ the ethical issues concerning the method of ontological unification, including its relation to the concept of truth (in its correspondence version). Here one may ask not only whether more unified theories offer better explanations, but also whether they are closer to the truth. After briefly surveying the literature touching these issues, in the later parts of the paper I focus on the three topics just raised.

BETWEEN ONTOLOGICAL AND THEORETICAL UNIFICATION

Unification reflects the ratio of explanandum elements to the ones in explanans. As Feigl (1970, 12) put it: “The aim of scientific explanation throughout the ages has been *unification*, i.e., the comprehending of maximum of facts and regularities in terms of a minimum of theoretical concepts and assumptions”. In such an approach no one asks about the relative importance of particular elements in the explanans. Having in mind that the “unofficial” idea of unification came from the empiricist camp whose skepticism towards anything unobservable was legendary, it is justifiable to claim that in this perspective the subjects of unifying processes

were just the particular theories and not the real, underlying processes responsible for phenomena put under investigation. Therefore, the Hempel's view on unification was theoretical in nature, i.e., while making references to unification, he put emphasis on theoretical unification (see, Hempel and Oppenheim, 1948). U. Mäki defines that kind of unification in the following way:

“Logical unification is brought about when more and more statements within a discipline become derivable from the same set of axioms, or when the same set of statements becomes derivable from a smaller set of axioms” (Mäki, 1990, 331).

Since the process of logical unification operates on theories I call it theoretical unification. The role of researcher using that form of unification is therefore to build a new theory explaining all the phenomena elucidated by its predecessors. Kitcher (1989, 432) puts it as follows: “explanatory unification is a matter of establishing the best trade-off between minimizing the number of premises and maximizing the number of conclusions of explanatory arguments”. Unification of that form was used by Samuelson who in his *Foundations* (1947/1983, 7) claims the following:

“The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and *unifies them* with respect to those central features. It is the purpose of the pages that follow to work out [the implications of this fundamental principle] for theoretical and applied economics ... seemingly diverse fields – production economics, consumer's behavior, international trade, public finance, business cycles, income analysis – possess striking formal similarities. Only after laborious work in each of these fields did the realization dawn upon me that *essentially the same inequalities and theorems appeared again and again*” (italics added by Mäki in his citation of Samuelson).

Aumann offers a similar view on theoretical unification: “the validity of utility maximization does not depend on its being an accurate description of the behavior of individuals. Rather, it derives from its being the underlying postulate that pulls together most of economic theory” (1985, 35). What strikes me in Aumann's words is his insistence on the potential of a given theory to unify various explanations under one more general theory combined with his negligence of seeking truth. I do not agree with such an opinion. That is not to say that for me theoretical unification is not a virtue in science, but rather I claim that it should be combined with a desire to accurately explain the workings of a socio-economic system. If not supplemented with such a desire, economists risk of building completely fictional worlds or model worlds, namely the ones “not constructed by starting with real world and stripping out complicating factors: although the model is *simpler* than the real world, the one is not a *simplification* of the other” (Sugden, 2002, 131). Such an approach to model building is quite

close to the one used by constructivists: “the true constructivists, do not even pretend to be primarily in the business of representing any real target system. For them the models come first” (Knuuttila, 2009, 67). For sure, in the case of theoretical unification one cannot say that the more unified a given theory is, the higher explanatory power it possesses. However, there is a promising alternative to constructivists’ ideas, namely the isolationist approach where “[m]odels are considered true if they correctly isolate a real factor, and their epistemic contribution lies in this decomposition and identification of causal factors and their ways of operation” (Grüne-Yanoff, 2009, 2). In such an approach truth matters. As Mäki (2001, 498) puts it:

“[O]ntological unification is based on the referential and representational capabilities of theories, while derivational [theoretical] unification is based on their inferential capabilities (Mäki, 1990, 331). Ontological unification is a matter of redescribing apparently independent and diverse phenomena as manifestations (outcomes, phases, forms, aspects) of one and the small number of entities, powers, and processes”.

The idea of isolation has a long history and has been advocated by economists like J. S. Mill and Alfred Marshall (see, Grüne-Yanoff, 2009, 2). U. Mäki (1992) calls that method a fundamental one in economics. What one seeks while using it is to find a crucial set of elements responsible for a given explanandum. Using Mill’s terminology, the aim of economics should be to find *grater causes* of events. Or, as Mäki (1990, 319) puts it:

“[E]xplanation is or should be conditioned more by objectively real than subjective factors. It is the way the world is which ultimately should determine the limits to conditions for correct explanations. Scientific explanation is primarily an attempt to reveal some essential relationships in the real world”.

Mäki (1990) cites here Menger’s statement that the aim of economics can be defined as “investigations into the nature of the commodity, into the nature of economy, the nature of value, of price and similar things” (1963, 37). That statement is quite essentialist in nature, especially if we compare it with the opinion of another leading Austrian economist, precisely Mises, who claims that “scientific reasoning [should] not stop until it reaches a point beyond which it cannot go” (1933 [2003], 29). Here I disagree. Following Mäki, I agree with Popper (1963, 103) that scientific theories should describe the “essence” of things lying behind the appearances. It is important to note that Popper puts here essence in inverted commas. “Essence” does not mean the final explanation which does not require further investigations; “essence” is just the most important explanation accessible in a given moment. That follows from Popper’s rejection of essentialism, namely his denial of “the doctrine that science aims at ultimate explanation; that is to say, an explanation which (essentially, or by its very nature)

cannot be further explained, and which is in no need of any further explanation” (1972, 105). Not only Austrian economists, like Menger and Mises, have fallen into the trap of essentialism but also in writings of many contemporary theoreticians one can find an essentialist spirit, e.g., Coase while claiming that the aim of theorizing in economics is “to get to the essence of what [is] going on in the economic system” (1988, 68) or Williamson (1985, 31) that “any issue that can be formulated as a contracting problem can be investigated to advantage in transaction cost economizing terms. Every exchange relation qualifies” (1985, 17). The essentialist spirit in the works of Coase and Williamson does not mean that they are essentialist in the sense of Mises and Menger, more probably they are “modified essentialists” sharing Popper’s view that:

“I do not think that we ever describe, by our [conjectural] universal laws, an ultimate essence of the world. I do not doubt that we may seek to probe deeper and deeper into the structure of our world or, as we might say, into properties of the world that are more and more essential, of greater and greater depth” (Popper, 1972, 196).

Here it is clear that Popper rejects classical empiricism. That is important for economics which had been marked by the empiricist/positivist spirit since Hutchison’s 1938 paper *The Significance and Basic Postulates of Economic Theory*, where he claims that “The essence of science [economics] is the appeal to fact”. His crucial problem was how to distinguish economics (science) from pseudo-economics (pseudo-science). Finally, he found the answer in Popperian falsificationism. However, due to changes in economic methodology as such (e.g., the demise of Hutchison’s legacy) and in philosophy of science in particular (problems with falsificationism due to the so-called Duhem-Quine theses, popularization of the “unofficial” Hempel’s unificationist approach, etc.) that took place in the 70’s, the reasoning in terms of observables (if x , then y), quite popular in economics, started to be questioned. That is important for our discussion about unificationist approach to economic modeling since in its original version it shared with Hempel’s “unofficial” model of explanation the claim that the explanans is formed by observable facts (“antecedent conditions” to use the terminology of Hempel and Oppenheim) combined with a set of general laws. Therefore, the shift towards explaining in terms of unobservables which took place in philosophy of science in the 70’s should have the impact on the way we understand the unificationist account of explanation and the nature of explanans in particular. For example, should the explanans be formed by unobservables? Moreover, if we are to accept the view that in economics we do not have the laws of nature as the ones postulated in Hempel’s model then what about the second pillar of explanandum – if not laws, then what? These are the question that do not occupy enough attention in philosophical reflection on economics. I focus on these issues in the next section of the paper.

THE ELEMENTS OF EXPLANANS AFTER EMPIRICISM

Mäki claims that the explanans of ontologically unified theories consists of “entities, powers, and processes” (2001, 498), and then he specifies it as follows: “ontological as opposed to mere derivational unification gives priority to entities rather than sentences; reference representation rather than inference and derivation; discovery rather than imposition” (499). That sounds very close to W. Salmon’s statement that: “Casual processes, casual interactions, and casual laws provide the mechanisms by which the world works: to understand *why* certain things happen, we need to see *how* they are produced by these mechanisms” (1984, 132). Therefore, we have two interconnected questions: what are the elements of explanans and how they produce explanandum phenomena. In other words, Mäki’s accounts of ontological unification give a nice picture of a general structure of ontologically unified theories (e.g., his 2004 study on theoretical isolation and explanatory progress in transaction costs economics) but lacks a philosophical reflection into the nature of explanans and the ways it “reacts” with explanandum. Luckily, such a reflection is present in philosophy of economics and can nicely fit with Mäki’s approach. Here I would like to make references to T. Lawson transcendental realism and N. Cartwright accounts of capabilities and causes. Let me start with Lawson.

According to Lawson, economic explanations should be transcendental in nature: “It is a movement, paradigmatically, from a ‘surface phenomenon’ to some ‘deeper’ casual thing” (1997, 24). Here he makes reference to the so-called transcendental realism, namely “a metaphysical theory constituting an account of what the world must be like before it is investigated by science, and for scientific activities to be possible” (1998, 504). That question is very Kantian in nature. Thus, for Lawson, explaining is digging deeply:

“[S]cience aims at uncovering casual factors, that is, it is concerned with identifying structures, mechanisms, and the tendencies they ground, which produce, govern or facilitate phenomena at a different level. And if the aim of science is to illuminate structures that govern surface phenomena then *laws* or *law-statements* are neither empirical statements (statements about experiences) nor statements about events or their regularities (whether unqualified or subject to *ceteris paribus* restrictions), but precisely statements elucidating structures and their characteristic modes of activity” (Lawson, 1997, 24).

Thus, according to Lawson, we have structures (explanans) and surface phenomena (explanandum) and the goal is to explain the observables by “hidden” mechanisms. That is a clear depart from empiricism also because Lawsonian realism has a huge metaphysical load. Therefore, the science should dig deeply and should analyze not only the manifestations of explaining items but their nature; if not, then concentration on observables only would lead to analyzing theories as such, and therefore ontology would be reduced to epistemology (cf.

Backhouse 1994, 13). However, if we agree that we have only processes and mechanisms not producing perfect regularities, then how can one explain them? Lawson states that the alternative to empiricist *if x, then y* is not a random and unsystematic “emergence” of explanandum phenomena, but the so-called partial regularities that are reproduced systematically over restricted areas of time-space. To illustrate such demi-laws he gives the example of women in the UK that are *concentrated* in secondary sectors of labor markets and also the case of productivity growth in the UK that has *frequently* been slower than in other industrial countries (Lawson, 1997, 204). The use of italics indicates here that these statements are less than universal and that they refer to actualization of mechanisms or tendencies. If we are to apply Lawson’s analysis to unificationist approach to explanation, then we should finish with explanans formed by processes unobservable on empirical level of reality that in a given context of time and space may produce given outcomes (explanandum). Therefore, in a sense, explanans reduces to one set of ingredients, i.e., mechanisms, that *per se* may produce explanandum phenomena. We do not have empiricist laws of nature, but a transcendental inference, i.e.,

$$\begin{aligned} & [\text{mechanisms}] (\textit{explanans}) \rightarrow \text{given context} \rightarrow \\ & \rightarrow (\text{may produce}) \text{given outcomes} (\textit{explanandum}) \end{aligned}$$

The central idea introduced by Lawson is the one of the actualization of a mechanism – in a given context a particular mechanism produces a given outcome, however, it is not clear from Lawson’s accounts whether the same mechanism in the same context always produce the same outcomes. Having in mind his insistence on less than universal character of regularities in economics, one may assume that the answer is simply *no*. The reason for that, according to Lawson, is that the context is constantly changing and cannot be controlled. However, such an argument assumes that the actualization of a given mechanism is external to it. I find such a reasoning problematic since it is quite essentialist. Let me explain. If one claims that the actualization of a given mechanism is totally external to it and depends only on context, then one in fact claims that a given mechanism does not have any hidden structures or determinants of its functioning. If Lawson subscribes to such an essentialist thinking, he contradicts himself since he rejects essentialism due to his claim that a scientist should dig deeply. Therefore, the central issue now is to explain the ways mechanisms “actualize” themselves and henceforth produce explanandum phenomena. I try to do it below by referring to the N. Cartwright’s philosophy of economics.

The Lawsonian “actualization of mechanism” can be restated in terms of mechanism’s nature, and then the question is the following – what is in the nature of a given mechanism to produce? That is a typical Aristotelian question. Therefore, what is important is not the set of elements of the explanans as such but the nature of each of them. The aim of science (here: economics) is to acquire

knowledge about natures of explaining items. N. Cartwright summarizes it as follows:

“Our most wide-ranging scientific knowledge is not knowledge of laws but knowledge of *natures* of things, knowledge that allows us to build new nomological machines never before seen giving rise to new laws never before dreamt of” (Cartwright, 1999, 4).

In her path-breaking book *Nature's Capacity and their Measurement* (1989) she summarizes her views in a similar way: “The generic casual claims of science are not reports of regularities but rather ascriptions of capacities, capacities to make things happen, case by case” (2–3). For Cartwright capacities equal natures and therefore the analyses of natures can lead to a better understanding of phenomena put under investigation. Hence, not surprisingly she claims “I maintain that the use of Aristotelian-style natures is central to modern explanatory progress” (1999, 81). In Cartwright’s opinion there are no better examples of particular sciences with capacities playing the most important role in explaining than physics and economics. Let me focus on economics; here she claims:

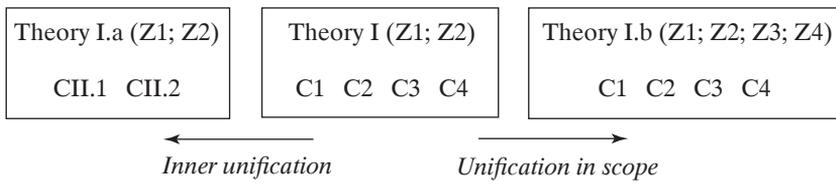
“Mill believed that the laws of political economy and the laws of mechanics alike are laws, not about what things do, but about what tendencies they have. This thesis should be familiar. Substituting the word ‘capacity’ for Mill’s word ‘tendency’, his claim is exactly what I aim to establish in this book” (Cartwright, 1989, 170).

What Cartwright wants is to supplement the method of idealization (e.g., the one used in economics as described by Mäki) with a strong reference to natures of things: “Together idealization and the inference to natures form a familiar two-tiered process that lies at the heart of modern scientific inquiry” (1999, 83). Here we arrive at answering the question about the ingredients of explanans after empiricism, namely after rejecting the view that only observable facts can account for a given explanandum. What accounts for explanans are natures of things, and that lead to a reformulation of unificationist approach, and hence the method of ontological unification became a search for the nature of processes and mechanisms that are primarily responsible for explained items. By making reference to natures, the process of ontological unification gains also a metaphysical dimension. Summing up my reflection in this section of the paper I would like to stress that, in respect to the philosophical studies on unificationist approach as used in economics, the contributions by Mäki, Lawson, and Cartwright can be treated as complementary rather than mutually exclusive frameworks.

In the next section of the paper I move to the issue of the nature of ontologically unified theories, i.e., their relation to their predecessors. Therefore, I am not to focus on the elements in explanans/explanandum as such, but rather I try to analyze the interplay between theories in different stages in unificationist processes, i.e., with different level of unification.

THE DYNAMICS OF ONTOLOGICAL UNIFICATION

Unificationist reasoning is dynamic in nature (Kitcher, 1981, 509). In trying to explain much by little we experiment with different sets of explanans and explanandum. What we aim to do is to reduce the ratio between the number of elements present in explanans and the ones forming the explanandum. However, that can be done in two ways. First, one can add new elements to explanandum while keeping explanans unchanged. Second, one can reduce the number of explaining items (explanans) and keep explanandum unaltered. The former method is quite popular in economics and accounts for its expansionism defined by Mäki “[as] a matter of a persistent pursuit to increase the degree of unification provided by an economic theory by way of applying it to new types of phenomena” (2009c, 9). Let me call the first method *unification in scope* and the second one *inner unification*. Differences between them are schematically represented below:



The starting point is Theory I which is formed by two explained items Z1 and Z2 (explanandum) and four explaining elements, i.e., C1, C2, C3, and C4 (explanans). In order to unify it, one can search for determinants of explanans, here CII.1 and CII.2 that produce explanandum of Theory I, and hence Theory I.a is more unified than Theory I. Alternatively, one can arrive at more unified theory by using the explanans of Theory I to account for not only Z1 and Z2 but also Z3 and Z4, and hence can finish with Theory I.b. Since we are interested in digging deeply, let me focus on the interplay between Theory I and Theory I.a. Can one say that Theory I reduces to Theory I.a, i.e., CII.1 and CII.2 produce Z1 and Z2 by producing C1, C2, C3, and C4? Or, maybe, Theory I.a is ontologically new and there is a direct production of Z1 and Z2 by CII.1 and CII.2? If the answer is yes to the first question, then we are quite close to reductionism. However, reductionism is untenable if inference is in terms of capacities, natures, tendencies, and mechanisms. Therefore, my answer to the first question is NO and hence moving from TI to TI.a is a transcendental voyage that fits well with a beautiful saying by Marcel Proust: “The real voyage of discovery consists not in seeking new landscapes, but in having new eyes”. We still have the same landscape (Z1 and Z2) but totally new eyes (CII.1 and CII.2). Butts (1968,163) writes in this context the following:

“There is always a new conception, a principle of connexion and unity, supplied by the mind, and superinduced upon the particulars. There is not

merely a juxtaposition of materials, by which the new proposition contains all that its component parts contained: but also a formative act exerted by the understanding, so that these materials are contained in a new shape”.

How to find a totally “new conception”? The answer is not by using deduction nor induction, but by applying the transcendental reasoning aiming at uncovering the deep determinants of explanandum phenomena, i.e., the ones present in the (Lawsonian) real domain of the reality. There is no better way to explain transcendental reasoning than by referring directly to Lawson (1997, 24):

“[T]he essential mode of inference sponsored by transcendental realism is neither induction nor deduction but one that can be styled *retroduction* or *abduction* or ‘as if’ reasoning. It consists in the movement, on the basis of analogy and metaphor amongst other things, from a conception of some phenomenon of interest to a conception of some totally different type of thing, mechanism, structure or condition that, at least in part, is responsible for the given phenomenon. If deduction is illustrated by the move from the *general* claim that ‘all ravens are black’ to the *particular* inference that the next one seen will be black, and induction by the move from the *particular* observation of numerous black ravens to the general claim that ‘all ravens are black’, retroduction or abductive reasoning is indicated by a move from the observation of numerous black ravens to a theory of a mechanism intrinsic (and perhaps also extrinsic) to ravens which disposes them to be black”.

The goal of economics should be to search for such mechanisms and as Hahn (1984, 332) claims: “I do not wish to deny that there are empirical regularities of economic behaviour awaiting discovery. But I claim that these will be, as it were, much deeper down...and much closer to the form in which axioms are postulated”. For him the example of axiom is the economic concept of consumer preferences as such. He also postulates to dig deeply in explaining economic phenomena and he puts it as follows: “Axioms are not plucked out of thin air and far from distancing the theorists from what somewhat mysteriously is called the ‘real’ world they constitute claims about this world so widely agreed as to make further argument unnecessary” (Hahn, 1985, 5). Again, I disagree with the last part of Hahn’s claim since I do not believe in essentialism. Here I subscribe to a famous Hilbert’s (1902, 438) saying that “As long as a branch of science offers an abundance of problems, so long is it alive”. It is interesting that the above words by Hilbert are used as an opening phrase in Acemoglu’s opus-magnum *Introduction to Modern Economic Growth* (2009) where some elements of the method of retroduction can also be found as the following sentence from its preface indicates:

“[T]o understand why some countries grow and others fail to do so, economists have to move beyond mechanics of models and pose questions about the fundamental causes of economic growth” (xv; underlined by L.H.).

The kind of models Acemoglu rejects are mainly phenomenological models, precisely those that represent only observable proprieties of their targets (cf. Frigg, 2006). The postulated *move beyond mechanics of models* can be therefore understood as the move beyond the mechanics of wrong kind of models. For sure, Acemoglu in his own models of economic growth does not reject the usefulness of idealized models with a set of negligibility assumptions. Thus, even at the research frontier of modern mainstream economics one can find a transcendental reasoning.

Let me summarize. In the introduction to this paper I posed the following question: how unified theories refer to their predecessors? As I have tried to show, there is no easy answer to such question. For sure, a unified theory is not the result of some kind of applied reductionism. Also, it is not the result of inductive/deductive reasoning, namely it is not the case that somebody is not stopping in applying deduction/induction methods after finding Theory I. and that in doing so he is to arrive at Theory I.a. More likely is that Theory I.a. is to be found by using Lawsonian abductive reasoning with transcendental spirit.

What remains to do is to refer to the third issue raised in the introduction, namely whether ontologically unified theories are closer to truth. Also, whether one can say that better explanation positively correlates with truthlikeness of theories. It is important to notice that *truthlikeness* measures degree of *being similar to the truth* and *probability* measures the degree of *seeming to be true*. Here we focus on truthlikeness since *being similar* concerns the objective facts about similarity or likeness, and *seeming to be true* concerns the appearances (see, Graham, 2008). Since the method of ontological unification is realist in nature, it is no surprise that the right issue is to ask about truthlikeness of ontologically unified theories.

TRUTHLIKENESS OF ONTOLOGICALLY UNIFIED THEORIES

Robert Lucas once said that “[a]rtificial economic systems [models] can serve as laboratories in which policies (...) can be tested” (Lucas, 1980, 696). Nancy Cartwright (1983, 4) adds the following: “route from theory to reality is from theory to model, and then from model to phenomenological law”. In a similar vein, Morgan (1998, 318) claims that: “[models] are at the same time models of theory and models of phenomena because the model is where these two elements fit together”. Therefore, if we are to investigate the truthlikeness of ontologically unified theories, we should analyze the truthlikeness of (economic) models. For the sake of simplicity I treat here theory as a set of logically connected models (Suppes, 1974). Mäki describes a typical model in economics as follows:

“Agent *A* uses object *M* (the model) as a representative of target system *R* for purpose *P*, addressing audience *E*, prompting genuine issues of resemblance between *M* and *R* to arise; and applies commentary *C* to

identify the above elements and to coordinate their relationship” (Mäki, 2009a, 75).

In such a conceptualization, the model can be viewed as pragmatically (due to the presence of *P* and *E*) and ontologically (the importance of resemblance between *M* and *R*) constrained representation. However, the issue is more complicated since models represent in two ways. First, one can say that a model is a *representative* of some target, i.e., *M* is *R*'s surrogate. Second, *M* can *resemble* the target system *R* if one can learn about *R* by examining *M*, i.e., *resemblance* should be in “suitable respects and sufficient degrees” (Mäki, 2009b, 32). If we are to subscribe to abductive reasoning then we should concentrate on resemblance and hence we should value most surrogate models rather than totally fictional ones (substitute systems). Therefore, resemblance matters and the higher level of resemblance, the *more similar to the truth* a given model is. So, resemblance comes with truthlikeness. In order to conclude I would say that representation does not require resemblance². One can have *M* as *R*'s surrogate but that *M* is not always guarantying that one can learn from *M* about *R*. E.g., *M* can be a map with a scale nearly 1:1, but representing nearly 100% of a given area is not guarantying that it says something about what is the most interesting in this very area and what is in 1% of unrepresented territory.

From the above discussion we know that ontologically unified theories can have a high degree of truthlikeness, since that kind of unification requires theories (models) to infer to ontologically important explaining mechanisms and processes. If a given theory contains such elements, then it offers high degree of resemblance. However, one question still needs to be answered – why explaining in terms of searching for truth should be the goal of science, including economics? Here I have to come back to the fact that ontological unification presupposes scientific realism – we have the world which is external to the observer (here: economist) and that world can be comprehended. Next, if the virtue of science is to be objective, then as Artigas claims:

“If we recognize scientific objectivity as an important value in itself, and not only because it leads us to obtain results important for technical purposes, then we are dealing with the *search for truth* as an *ethical value*, and this is hardly compatible with the scientist perspective that considers empirical science to be ‘the *only* authentic source of truth’. We cannot prove that the pursuit of truth is an ethical value using science alone” (Artigas, 2001, 253–254).

Therefore, being objective requires accepting realism; but accepting realism leads to ontological kind of unificationist projects. If one, as myself, subscribes to such a realist perspective then it leads to the denial of the vision of economics nicely described by Machlup (1955, 5) in the following way:

² For further discussion, see Mäki U. (2009b) and a set of papers in the special issue of *Erkenntnis* (Vol. 70, No. 1).

“[E]conomic science is a system of a priori truths, a product of pure reason, an exact science reaching laws as universal as those of mathematics, a purely axiomatic discipline, a system of pure deductions from a series of postulates, not open to any verification or refutation on the ground of experience”.

It is not true that *economic science is a system of a priori truths*; on the contrary, truth is to be discovered using the method of transcendental inference (abduction). It is also not true that economics can reach *laws as universal as those of mathematics*; on the contrary, in Lawsonian perspective economists search not for universal laws but for Mill’s tendencies and processes. Therefore, economics is not *a system of pure deductions from a series of postulates*; rather it is a never ending process of explaining deeper and deeper. What in a sense economists a priori assume are the postulates of scientific realism, however, these postulates are *out-of-economics-postulates*. Bunge explains:

“Scientific knowledge contains no philosophical assumptions. From this is often concluded that scientific research has neither philosophical presuppositions nor a philosophical import, whence science and philosophy would be water-tight compartments. But this is a hurried conclusion. Philosophy may not be found in the finished scientific buildings (...) but it is part of the scaffolding employed in their construction (...) scientific research does presuppose and control certain important philosophical hypotheses. Among these the following stand out: the reality of the external world, the multilevel structure of reality, determinism in an ample sense, the knowability of the world, and the autonomy of logic and mathematics” (Bunge, 1967, 291).

These *out-of-economics-postulates* or presuppositions of economic theories are fundamental for the deep understanding of economists’ practice. Schumpeter called that fundamental assumptions *a vision*, i.e., an ontological window through which economic theories reach the economic realm. Buchanan claims that this very ontological window offers *a constitutional framework* for economic research which is “rarely, if ever, challenged by those scholars who work inside the intellectual tradition. These central elements are taken as presuppositions, as relative absolute absolutes, and, as such, they become, themselves, the constraints (the constitution) within the scientific discourse is conducted” (Buchanan, 1991, 13–14).

What I tried to propose in this paper is that if economics is to explain, then it should adopt a scientific realism and hence it should value most ontologically unified theories. In this perspective, explanation should be superior to prediction and description and thus I subscribe to Coase’s words that “[f]aced with a choice between a theory which predicts well but gives us little insight into how the system works and one which gives this insight but predicts badly, I would choose the latter” (1988, 64). In order to give good insights economists should not only construct appropriate theories but invest their time also in analyzing ontological windows as such. Therefore, economics needs philosophical reflection.

CONCLUSIONS

The method of unification is central in science, including economics. Therefore, in order to understand how economists explain one should analyze that way of doing research. In this paper I tried to show that the kind of unification we should value most in economics is ontological one, which is based on the referential and representational capabilities of models. Next, I claimed that the official model of explanation by Hempel and Oppenheim, although very useful in offering the structure in which we can conceptualize economic theories (explanans vs. explanandum), does not capture the important specificities of economics. First, in economics we have mostly laws in a sense of Mill's tendencies rather than universal laws of nature. Second, in ontologically unified theories explanans consists of mechanisms and processes of unobservable nature that operate on the real level of reality. Haavelmo, for instance, puts it as follows: "In scientific research – in the field of economics as well as in other fields – our search for "explanations" consists in digging down to more fundamental relations than those that appear before us when we merely "stand and look"" (1944, 38). Third, deductive inference is not in place in ontologically unified theories in economics. As Lawson shows in his *Economics & Reality*, the inference is usually transcendental one (*digging down deeply*). That kind of inference combined with the idea of the actualization of a mechanism gives us the way explanans produces explanandum phenomena. Moreover, what is important in this perspective is not the set of explaining items as such but the nature of each element in the explanans. Fourth, I showed that a unified theory is not the result of some kind of applied reductionism. Precisely, there is always something (ontologically) new in a given unified theory that was not present in its predecessors. Fifth, the resemblance aspect of ontologically unified theories usually leads to higher truthlikeness of such theories. And last but not least, the central role of explanation in science, including economics in particular, follows from the fact that ontological unification is realist in nature and realism is, to use the idea from quantum physics, in entanglement state with truth in its correspondence version. Therefore, the more a given theory is ontologically unified, the better explanations it offers.

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METODA UNIFIKACJI ONTOLOGICZNEJ W EKONOMII. WYBRANE ASPEKTY

STRESZCZENIE

Artykuł omawia kwestie metodologiczne związane ze stosowaniem metody unifikacji ontologicznej w ekonomii. Tekst rozpoczyna się od krótkiego przeglądu literatury dotyczącej tego zagadnienia, aby następnie zająć się tymi aspektami unifikacji ontologicznej, które nie są szczegółowo omawiane w literaturze. Artykuł opisuje więc kwestię struktury zunifikowanych ontologicznie teorii ekonomicznych, a także proces ich formowania, w tym ich relację do teorii, które podlegały procesowi unifikacji. W analizie nie pomija się również fundamentalnego zagadnienia związanego z tym, na ile bardziej zunifikowane teorie oferują lepsze wyjaśnienia, a także czy postępujący proces unifikacji ontologicznej w ramach danej dziedziny wiedzy (tutaj: ekonomii) musi oznaczać zbliżanie się do prawdy.

Słowa kluczowe: filozofia ekonomii, unifikacja ontologiczna i teoretyczna, realizm w ekonomii, wyjaśniania.

ABSTRACT

The paper focuses on the method on ontological unification in economics. It briefly reviews the literature dealing with various understandings of unificationist projects in economics. Next it puts emphasis on some interesting issues concerning the method of unification that did not attract much attention in the literature. Thus, the paper deals with the issue of the structure of ontologically unified theories and the process of their formation. It also analyses the problem of the nature of ontologically unified theories, i.e., their relation to their predecessors. Moreover, it raises the ethical issues concerning the method of ontological unification, including its relation to the concept of truth (in its correspondence version). The paper finishes by asking the question whether more unified theories offer better explanations and also whether they are closer to the truth.

Keywords: philosophy of economics, theoretical and ontological unification, realism in economics, explanations.

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