Polanyi's Ontology *from Inside*: A Reply to My Critics

Tihamér Margitay[1]

Abstract Key words: David W. Agler, John V. Apczynski, Walter B. Gulick, Mihály Héder. Gergely Kertész, Kyle Takaki, Michael Polanyi, emergence, layered ontology, tacit knowing.
In this essay, I first sketch Polanyi's three arguments for layered ontology and the arguments against them which I put forward in Margitay (2010). I then discuss two recurrent themes in the several comments on my essay in this issue of Tradition and Discovery. *The balance of the paper gives detailed responses to each comment.*

I begin with a personal remark to provide a context for both my 2010 paper and my reply to the comments. Polanyi's multi-layered ontology and his theory of emergence are very appealing to me. Besides intuitive appeal, however, other merits are needed in philosophy, like good arguments, comprehensive and consistent accounts, support from a wide range of human experience, etc.—at least this is how I understand philosophy. In my 2010 paper, I wanted to probe into Polanyi's arguments in order to find out whether they can provide good reasons to believe in his ontological system. These arguments proved mostly to be less than convincing to me. This was a disappointing result. So I have always been on my critics' side and kept my fingers crossed for their success in defending Polanyi's ontology.

I am most grateful for my critics' comments not only because they devoted precious time to consider my paper but also because they helped me to reconsider the whole issue, to see the weaknesses of my points, to appreciate the strengths of Polanyi's *and* of *their own* arguments to find Polanyi's ontology more convincing. This exchange of ideas was extremely fruitful from my perspective. I have learnt much from the comments and they gave me further inspiration to refine the arguments *for* and against the Polanyian ontology. I am also deeply indebted to Phil Mullins and Walter Gulick for initiating and organizing this discussion in TAD.

Summary of My Arguments

In order to make explicit certain interpretations and to reinforce the common ground of discussion, let me first sketch my 2010 paper. I identified three arguments in Polanyi's works that were invoked to give reasons why we should accept his stratified ontology. The first one rests on his Correspondence Thesis (CT) and it runs like this (I cite Polanyi in this argument because his language and mine become an issue in later discussion):

> 1. Knowing (comprehension) has a hierarchical emergent structure. The knowledge (comprehension) of an entity is emergent on the tacit knowledge (comprehension) of certain clues and their integration.
> 2. Polanyi discusses in detail an example—the understanding of someone's skillful performance—in which there is a correspondence between the ontological structure of the comprehensive entity (the person's skillful performance) and the structure of another person's comprehension of the same entity. (The "comprehension of this real entity has the same structure as the entity which is its object" [TD, 33].)
> 3. Then this correspondence is generalized over "*all other instances of tacit knowing the correspondence between the structure of comprehension and the structure of the comprehensive entity which*

is its object. And we would expect then to find the structure of tacit knowing duplicated in the [ontological] principles of all real comprehensive entities" (TD, 33-34).
4. Based on this example, the structural correspondence consists in that a lower level in the structure of knowing of a particular comprehensive entity corresponds to the lower ontological level of the same entity; and, similarly, a higher level in knowing corresponds to the higher level in the ontology.
5. "[T]the structure of tacit knowing determines the structure of comprehensive entities" (TD, 55).
6. Therefore the ontological structure of *any* comprehensive entity is hierarchical because the structure of knowing the entity is hierarchical.
7. Therefore the world has a hierarchical ontological structure.

I raised three objections against this argument:

i. There are different kinds of cognitive achievements regarding a particular comprehensive entity (we recognize it, identify it, use it, understand it, etc.). All have the same emergent structure, yet we rely on different clues, and these clues are differently related to the ontological parts of the comprehensive entity.
ii. Knowledge of an entity is always emergent on the clues integrated into it, even in the case of those entities that are not ontologically emergent according to Polanyi. There is emergence in knowing without the ontological emergence of the object known.
iii. Knowledge of a comprehensive entity always relies on clues that are at an epistemologically lower level compared to the integrated whole, but some of these clues come from an ontologically higher level (e.g., our body and tradition, etc.) than the comprehensive entity (e.g., in the case of the recognition of a machine or a frog).

It follows from (i) and/or (ii) and/or (iii) that, given the relevant epistemological and ontological claims of Polanyi, there is no systematic correspondence between the ontological levels and levels of knowing. The CT simply proves to be false as a universal statement. In other words, it follows from (i) or/and (ii) or/and (iii) that, given the CT, what Polanyi says about the knowing of particular comprehensive entities is inconsistent with what he says about the ontological structure of those entities.

The argument from dual control is the second one invoked by Polanyi to support his hierarchical ontology.

8. Lower level (physical and chemical) laws are not able to determine/control the behavior of certain entities (i.e., emergent entities) fully.
9. Therefore higher level laws/principles are necessary to do this in terms of boundary conditions.
10. Higher level laws/principles and their concepts are irreducible to lower level concepts and laws.
11. Therefore these entities are ontologically emergent.

This simplified version is enough to formulate my two interrelated objections against (8) and, thus, against dual control:

iv. Emergent entities (e.g., machines) are also physical entities and as such they are subject to laws of physics. In the case of certain non-emergent entities (like the solar system), physics alone can completely determine (account for) their physical structure and physical parameters. (Quantum

indeterminacy is not an issue here.) Why cannot physics determine the *physical* structure and the *physical* parameters of an emergent (macroscopic) entity? Polanyi fails to explain what the difference is *in physics* in the two cases.

v. According to physics, physics is complete in the sense that, for *all physical bodies/particles*, physical laws and prior physical parameters of particles account for (determine) *all the subsequent physical parameters* of the particles. Therefore higher level principles cannot determine the physical shape, arrangement and working of emergent entities (that is, the physical parameters of, e.g., a machine) though they are supposed to do that. Therefore standard physics is inconsistent with the theory of dual control.

The third Polanyian argument for ontological emergence is the argument from identification. Polanyi developed its specific version for machines.

12. Identification is necessary for something to be an entity.
13. Higher level concepts and principles (e.g., operational principles, functional concepts etc.) can identify machines. (They are *sufficient* to identify them).
14. A *kind* of machine (e.g., pendulum clocks) can be realized—in theory—by infinitely many different physical-chemical configurations of particles.
15. Physics and chemistry do not have the higher level concepts and laws.
16. Therefore (from 14, 15 and 10) machine-types cannot be identified by their physical-chemical topography.
17. Therefore higher level concepts and principles (e.g., operational principles, functional concepts, etc.) are *necessary* to identify machines.
18. Therefore machines are emergent entities.

I challenged the steps from 14 and 15 to 16:

vi. For *identification*, no complete theory-reduction (like the supposed reduction of thermodynamics to statistical mechanics) is necessary. A weak conceptual reduction is enough in which the concepts of the higher level theory are identified with or defined in terms of lower level concepts (i.e., without the derivation of higher level laws from the lower level ones). By means of such a weak conceptual reduction, the higher level technical description of a kind of machine can be translated into a physical-chemical description and it can identify the kind. Industrial standards were cited as extremely important practical examples of weak conceptual reduction. Industrial standards define—by convention—machines (higher level concepts) like fittings and wrenches in terms of physical-chemical descriptions. Therefore there are machines that can be identified by means of lower level concepts, thus they are not emergent entities. Therefore, Polanyi's argument is insufficient to establish the ontological difference.

The overall conclusion of my critique is that Polanyi's arguments do not support his theory about the emergence of not knowledge-like entities and about their hierarchical ontological structure. The emergence of knowledge-like entities can be established by Polanyi's epistemology, but the general stratified ontology cannot be built on this ground.

Let me make explicit a methodological point: I meant to apply internal criticism in my paper. I analyzed all the arguments (that I know of) Polanyi brought up to back up his stratified ontology and I argued

that all of them are less than convincing because of internal tensions, inconsistencies and *non sequiturs.* Of course *internal* criticism of an argument—strictly speaking—is possible only very rarely. Primarily I had to rely on my own reconstructions. Some of them rested directly on citations, while others—I think—can be plausibly attributed to Polanyi on the basis of his commitments. Anyway, they all involve my interpretation. Even word by word citations can be questioned as to whether they do represent Polanyi's position properly (see below).

Common Themes in the Comments

Before giving detailed replies to comments, I reflect on the overall picture emerging from them. Two themes—two charges—appeared in most of the comments, and both of them are related to my methodology of internal criticism. Firstly, my reconstruction of the Correspondence Thesis elicited disapproval (Apczynski, Gulick, Héder, Takaki). My readers thought that the cited passages and their use do not represent correctly Polanyi's theory on the issue, thus they are misleading. Though my critics offered different explanations as to what the problem was with these texts, their remarks might have a common core that Phil Mullins formulated particularly clearly in personal correspondence (and analyzed in detail in Mullins 2006). He contends that

> it is a Polanyi bridge idea that affirms that you cannot split ontology from epistemology. I suggested [in Mullins 2006] that Polanyi's one unfortunate slip into the language of "correspondence" in TD is a single instance that all the other language about ontology elsewhere ("ontological aspect") makes clear he is not arguing for any sort of simple correspondence theory since this pulls ontology and epistemology apart and presumes that we can speak about the nature of real things apart from acknowledging our commitment that this is the way things are (an unsigned check). (Mullins, personal correspondence).

Consequently, using these TD passages as a starting point—so the first charge says—I interpreted Polanyi in an uncharitable way and this is why my criticism misses the point. Basically, I set up a straw man and argued against it.

This complaint struck me as odd because I fully agree with what Mullins says. Ontology is indeed inseparable from epistemology. The onto*logy* of a comprehensive entity is nothing but our theory about its ontological structure. Ontology can be discussed only as the structure of the known/knowable world. Our epistemology has a direct bearing on what our world can be like, and our ontological commitments bear on the kind of knowledge we have. And if you bear in mind this, then the citations from TD should not be misleading arguments. The TD-based form of the Correspondence Thesis is just shorthand for the correspondence between the structure of our knowledge of a comprehensive entity and what we know about the ontological structure of the entity. I have been taking CT this way. Anyhow, nothing hinges on *my* understanding of CT in my arguments since they are to show the inconsistencies within what *Polanyi* says about the structure of knowing and the ontological structures of things as they are known, obviously, by him.

Yet, it is true, I adopted the language of the passages cited from TD, the language I considered innocent abbreviation, but I was wrong. This language in my mouth was misleading

I took Polanyi's language literally and innocently as a basis for my critique. But I was wrong not to have examined the passage in light of a broader consideration of Polanyi's philolsophy.

The second charge recurrent in the comments is that I criticize Polanyi from some objectivist and reductionist standpoint relying on hidden and outdated assumptions that have been rebutted successfully by Polanyi and many other philosophers in the twentieth century. This charge probably ensues mainly from the previous one. If I had considered ontology as independent of any knower, from a God's eye perspective, then it would have implied objectivism indeed.

I was a bit embarrassed when I found myself accused of objectivism and reductionism again and again. I went through my arguments and tried to find the hidden objectivist and reductionist assumptions, but I could not—neither do I need these assumptions as it is clear from the outlines of my arguments. The only place I refer to reductionism is in (vi) but even there the notion of weak conceptual reduction serves only as *a theoretical explication* for *how* industrial standards identify standardized machines. My argument would pose the same challenge to Polanyi's also without this, if standardized machines were used merely as counterexamples. If the first four sentences are deleted from (vi), we get the following argument.

> vii. Industrial standards define—by convention—machines (higher level concepts) like fittings and wrenches in terms of physical chemical descriptions. Therefore there are machines that can be identified by means of lower level concepts. Thus they are not emergent entities. Therefore, Polanyi's argument is insufficient to establish the ontological difference.

I think I can show point by point (and I will do it in due course in the answers below) that I am not guilty of the charges of objectivism and reductionism, and that these charges rest on misunderstanding. However, there is something alarming in recurrent misunderstandings. Furthermore, misunderstanding *simpliciter* is not a particularly fruitful explanation in a discussion, especially not in the present case. My commentators are excellent Polanyi scholars and they are sympathetic readers of my paper. There must be some substantial reason behind this rife misunderstanding. It cannot be the obscurity of my paper either, because I am sure they read it carefully and with charity to counterbalance all the imperfections of my style and disentangle all the obscurity of my words. What can be then the explanation for this misunderstanding?

My commentators might have an important truth behind their (mis)understandings. Assuming that the second charge results mainly from the first one, the problems spring from the language of CT and my critique. If you think that this language reveals and carries both epistemological and ontological commitments that are inconsistent with the spirit and words of the rest of Polanyi's works, then the attribution of objectivism and reductionism seems to be rational even if the literal reading of my paper runs contrary to the attribution of such positions. True enough, the language of the passages I cited and I used seems to separate ontology from epistemology. However, as I have suggested above, this language can be viewed as a mere abbreviation carrying no metaphysical or epistemological commitment at all. But why should it be so? What is the role of this language in Polanyi's argument and in my criticism? Is this parlance really so innocent? The first lesson to be drawn from the comments is that the burden of proof is on me in the discussion to show that Polanyi's argument and my criticism can really be formulated in a purely epistemological language without losing their force, in a language that explicitly excludes objectivist and reductionist assumptions. This project goes far beyond the scope of this reply. Thus I will take up this challenge in a forthcoming paper. (Its rather stale working title is Polanyi's Ontological Arguments Revisited.) For the time being, I will keep in mind carefully these lessons in my replies.

I must apologize that my replies are not proportionate to the comments. It is partly because I would like to answer a problem thoroughly at one place (at the place of its first occurrence) instead of giving fragmented replies every time when it comes up.

1. Reply to Walter Gulick

In point 1, Gulick correctly observes that it follows from my internal critical approach that my arguments cannot show the untenability of stratified ontology. If convincing, they can show at best that the stratified ontology cannot be like Polanyi says it is, or, at least that we need other reasons to accept this ontology.

Another consequence of this strategy is that I do not criticize Polanyi from an objectivist standpoint, and so I do not rely upon objectivist assumptions. Gulick is right when he claims that

> [t]he distinction between the first person and the third person perspective, made use of by Margitay (139), is often relied upon by philosophers in the analytical tradition to distinguish subjective from objective statements (Gulick, 10-11).

But then forgetting about the contingent "often", he goes on:

> Margitay's distinction (139) between the validity of ontological claims made from a first person in contrast to third person perspective seems to restrict the personal dimension of knowing to the former perspective only. He thus departs from a basic point in Polanyi's epistemology (Gulick, 11).

This represents a serious misunderstanding. The distinction between the first and the third person perspective does not entail objectivism or the rejection of Polanyi's epistemology. The distinction between the two perspectives can be made within Polanyian epistemology, and I make it in a Polanyian vein. For example, my perception is personal knowledge and the cognitive scientist's observations about my perceptual processes are also part of her personal knowledge.

The distinction between a first person's and a third person's personal knowledge came up in my paper where I explored the preconditions of the emergent status of knowledge-like entities. I argued that the relationship between the first person and the third person's personal knowledge is of extreme importance in Polanyi's ontology because the emergence of the knowledge-like entities depends, in part, on Polanyi's theory about how we can have access to other's knowledge (Einfühlung or indwelling[2]). Furthermore, the emergence of other entities—indeed the whole layered ontology—rests on the emergence of knowledge-like entities. So the emergence of the knowledge-like entities and, thus, the Polanyian theory of Einfhülung/indwelling are the cornerstones of the whole hierarchical ontology.

To see why our access to another's knowledge is important, let us begin with the emergence of knowledge-like entities. According to Polanyi, knowledge-like entities (perceptions, observations, discoveries, etc.) are emergent because, while integrating them into a focal whole, I cannot focus on the particulars and the integration process as they contribute to the focal whole. Their (functional, ontological and semantic) *role* in the whole is inaccessible for me because in order to make them work I have to focus on the focal whole; while to see them work, I should have to focus on their working. However, I can have only one focus at a

time (on the focal whole) and this is the first reason why the particulars and their integration are unspecifiable. Even if I could have a second focus, I could not observe the particulars as particulars having the appropriate functional role with respect to the whole but only as focal wholes by themselves. Turning the focus of my attention to the particulars changes their status. They cease to be particulars of the focal whole and become independent entities focally observed on their own rights, and this is the second reason for their unspecifiability. In principle, however, a cognitive psychologist (with her personal knowledge) can have access to my particulars and can observe them in their contribution to my focal whole. In principle, the whole process of *my* selecting the relevant particulars and integrating them into a focal whole could be the focal object of a third person and, in principle, she could lay bare and describe it. Of course, these observations will be part of the cognitive psychologist's personal knowledge. The description of the particulars and their integration of my perception would require that the psychologist be focally aware of my cognitive process (just as physicists can observe the process of the formation of crystals from single molecules or as biologists can observe the synthesis of protein). She need not be *focally* aware of my focal whole. It is enough that I assure her that, say, I watch the same object that she watched a moment ago. This possibility—that would demolish the emergent status of knowledge-like entities—is excluded, on Polanyi's view, in that the psychologist can know what I do only by means of Einfühlung/indwelling, copying my own process of knowing. Consequently, her focal observation of my process of knowing cannot lay bare my process of knowing for the same reasons that my focal observation of the particulars cannot reveal their contribution to the focal whole I integrated out of them. In short, the fact that a third person can have access to my knowledge only by dwelling in what I am dwelling in myself is part of what makes knowledge-like entities emergent. This line of thought presupposes that the third person has the same kind of personal knowledge as the first person.

In order to partially save CT, Gulick brings Polanyi's distinction (PK 105) between reversible and irreversible knowing (understanding, etc.) into play (see point 2-5, Gulick, 11-12). He agrees that "Polanyi's postulated correspondence fails to obtain in reversible recognition and identification" (Gulick, 11) as, e.g., in the recognition of a watch but "at least Margitay's argument seems established with respect to reversible cases of recognition; he does not analyze the correlations involved in the heuristic act of scientific discovery" (Gulick, 11). Gulick maintains that CT does hold in the cases of irreversible knowing, that is, in the cases of scientific discovery, invention, learning, etc. I cannot agree, I am afraid. My arguments (i), (ii) and (iii) do not depend on reversibility. If they work for reversible knowing, then they should work for irreversible cases too. And they do, indeed. Take for instance the great discoveries of particle physics, the discoveries of *elementary* particles from the photon to the latest Higgs boson. In particle physics, an elementary particle is a particle having no substructure. It is not made up of smaller particles or of any other constituents. In the Standard Model of particle physics (that is, according to the accepted theory of particles), the elementary particles include electrons, photons, neutrinos, bosons, etc.[3] These particles are *known*—and were known in Polanyi's time—to be ontologically simple entities according to physics, therefore they are not emergent entities in the context of physics and in the context in which they were discovered in physics. The existence of non-emergent particles in the ontology of physics obviously undermines Gulick's attempt to cast doubt on the existence of non-emergent entities. He writes:

> there is a problem with speaking of "non-emergent entities," because the frame of reference is all-important. While the path of a planet may be completely determined by the laws of physics and chemistry (132), the discovery that a certain patch of light is a planet can be seen as an emergent epistemological event for the discoverer. And there are many ontological levels in the dynamo-physical world; the planet is likely emergent from interstellar gases and debris (Gulick, 12).

This passage can be understood, I suspect, in three ways. First, we can read it as concluding that there are no non-emergent *knowledge-like* entities because the discovery of even a physical object (planet) is an emergent knowledge-like entity. This is in perfectly harmony with what I say in the first premise of argument (ii). "Knowledge of an entity is always emergent on the clues integrated into it, even in the case of ontologically non-emergent entities" (Margitay 2010, 133). So knowledge-like entities are emergent entities, but then, in this interpretation of Guilck's passage, there is no "problem with speaking of 'non-emergent entities.'" Secondly, the quotation above can be construed as concluding that there is no non-emergent entity in our ontology. It is a mistake in the light of how the ontology of physics describes elementary particles. Of course Gulick may dispute the ontology of standard physics, but it needs robust arguments and evidences. And they can hardly come from Polanyi's philosophy since he believed in physics. Thirdly, this paragraph may be taken as warning that the two sides of the correspondence should be in the same frame of reference when we substitute the structure of a particular instance of comprehension and the structure of the comprehensive entity which is its object into the correspondence relation. Gulick can be right about that depending on what the frame of reference and the sameness criteria mean. Anyway, the identity of this general frame of reference is secured intuitively when we compare the structure of someone's discovery of an elementary particle in the context of a theory *with* the ontological structure of the particle described by the same theory and accepted by the discoverer.

With these preliminaries we can reformulate the arguments (i), (ii), and (iii) for discoveries:

i'. Strictly speaking the original version of argument (i) does not apply to a non-emergent entity, but it can be modified in two different ways to work for elementary particles too. There are different kinds of irreversible cognitive achievements regarding an elementary particle (one discovers certain features of the electron, one develops its theory, one uses it in a novel scattering experiment to detect other particles, etc.). They all have the same emergent structure, yet these physicists rely on different clues, and these different clues are unrelated to the ontological parts of the elementary particle since it has no ontological parts at all. And this argument can also be formulated in a counterfactual way: There are different kinds of irreversible cognitive achievements regarding an elementary particle, they all have the same emergent structure, yet physicists rely on different clues, and these different clues would be obviously differently related to the selfsame ontological parts of the elementary particle if it had an ontologically complex structure.

ii'. The discovery of an elementary particle is always emergent on the clues integrated into it, but the entities discovered are not ontologically emergent. There is emergence in irreversible knowing without the ontological emergence of the object discovered.

iii'. Discovery of an elementary particle always relies on clues (like the theoretical model, mathematics, the design of experimental apparatus, etc.), that are at an epistemologically lower level compared to the integrated whole (i.e., the discovery of the elementary particle), but some of these clues come from an ontologically higher level (e.g., from physics, from the body of the experimentalist, from instruments, etc.) rather than the ontological level of the non-emergent elementary particle.

It follows from (i) and/or (ii) and/or (iii) that there is no systematic correspondence between the ontological structure of an elementary particle and the epistemological structure of its discovery in the light of Polanyi's other relevant ontological and epistemological claims and in the light of his commitment to physics. Consequently CT does not hold for irreversible knowing and understanding either.

In point 8, Gulick suggests a "non-rigorous version of the Correspondence Thesis … : the suggestive particulars of current knowledge can be indwelt and function as subsidiaries to higher level discoveries. Subsidiaries and focal insight of the knower are parallel to lower and higher (more general) levels in reality" (Gulick, 12-13). In this interpretation, CT is a kind of heuristic device (Margitay 2010, 133-134). CT makes only an analogy to explain how the lower and the higher ontological levels are related *provided that* you have already accepted the theory of hierarchical ontology. In this form, CT does not support the claim that higher level discoveries reveal more general levels of reality and that the more general levels of reality are ontologically distinct and constitute higher ontological levels. Furthermore, this analogy has, at best, only a restricted scope[4] as it is clear from the examples of elementary particles and from the analysis of the watch example (Margitay 2010, 132), that is, from the argument (i) and (iii).

In point 10, Gulick fleshes out a very intriguing ontological approach (Gulick, 13). He attributes it to me, but unfortunately it has never come to my mind, but I would be proud if it did! He suggests that we can use different ontological theories, externalist or internalist theories, depending on the circumstances perhaps, on our knowledge state. It is like what physicists do. They use different theories according to their purpose. The parameters of an accelerator are controlled on the basis of classical theories while the parameters of the accelerated and scattering particles are calculated by quantum theories. The proposal of a similar pragmatic use of ontological theories is a new and excellent example of Gulick's local pragmatism.

Point 13 is an attack on my argument against the possibility of dual control:

> A further assumption offered by Margitay also seems problematic: that the laws of physics are "complete" (135-136) and leave no indeterminacy so that any emergence must be controlled by these laws. The notion of such a deterministic universe is an expression of an objectivist perspective that is seriously at odds with Polanyi's emphasis on the many indeterminacies that limit what we can claim with certainty about ultimate reality (Gulick, 14).

Unfortunately, completeness is not my assumption. (I would be ready to discard it immediately.) It is a theorem of physics that physical theories account for all the physical parameters covered by their laws. (Think over how odd the opposite of the completeness theorem would be. Newtonian particle theory would be nonsense if it admitted that the fundamental law, F=ma, is true for some particles on some occasion, but for other particles, or on other occasions, it is not. And there is no physical criterion to separate the two cases because there is no *physical* difference between the two systems. So the completeness theorem is not the product of some outdated objectivism, instead it has a profound epistemological reason).

I fully subscribe to Gulick's reconstruction concerning operational principles (Gulick, 13-14). Operational principles determine the physical shape, arrangement and working both of the components of a machine and the whole machine; furthermore it specifies the purpose of the machine. So operational principles are also supposed to determine physical parameters (e.g., the motion of the arms of a clock). How is it possible that they can do this when these physical parameters are completely accounted for by physical theories and prior parameters of the system?[5] My argument (v) concludes that standard physics is inconsistent with the theory of dual control. It is not true that the notion of such deterministic (classical) physics is "seriously at odds with Polanyi's emphasis on the many indeterminacies". Polanyi points out many indeterminacies but, to my best knowledge, he never questions the deterministic nature and the completeness of classical physics. And he *merely postulates* the possibility of the control by operation principles over classical physical parameters.

Other indeterminacies revealed by Polanyi[6] do not help here, or at least, something more needs to be said about how they can make physics compatible with dual control.

I hope my detailed reply convinces Gulick that I am not "reverting to a kind of reductionistic objectivism" (Gulick, 14) and that I do not reject the basic objectives of post-critical philosophy. Instead, I am still working with him for these objectives.

2. Reply to Kyle Takaki

Takaki devotes his reply mainly to the problem of the Correspondence Thesis. Proceeding from a lucid figure in which he summarizes my reconstruction of CT, he analyses two issues that are the crucial components of my argument against CT.

He concedes that there is no systematic correlation between the structural elements of knowing and the structural elements of the object known. Therefore there can be no systematic correspondence, and therefore, contra Polanyi's thesis, the structure of knowledge cannot determine the structure of entities. He points out that this refutation of Polanyi's argument from CT rests on a strong notion of determination. As an alternative to this, in order to save Polanyi's argument, Takaki develops a witty (and Kantian) idea about how knowledge can determine the structure of the object known.

> [T]he practices of various sciences use tools to probe reality, there is an unavoidable need to create models … Tacit knowing primarily "determines" the structure of these models, which then are taken in some capacity to represent aspects of Nature. Since there is no direct access to Nature except by way of our models (theories, data, etc.), the degree to which we peer into the order of things is unavoidably mediated by the quality of the models we employ. Thus what tacit knowing "determines," via *connoisseurship*, are models (theories, etc.) that presumably gain some access to Nature. (Takaki, 36)[7]

This is a very sensible and persuasive determination relation. I know only certain methods of weaving fishing nets. The structure of my nets determines what sorts of fish I can catch. The fish that I catch determine what I know of them, and what I know of them determines my ontology of fishes. Hence my knowledge determines the structure of objects. However, this determination relation by itself cannot help Polanyi out for two reasons. First, remember Polanyi wants to argue for the layered structure of comprehensive entities by virtue of the layered structure of knowledge. For this, the emergent *structure* of knowledge should determine the emergent *structure* of things. Takaki's determination relation is far more general: some kind of knowledge *content* determines some ontological *content*. Why should the emergent structure of knowing result in such models that catch only emergent entities? It can happen only by chance not by determination. The second problem springs from the fact that my arguments (ii) and (iii) can be deployed *mutatis mutandis* against this determination relation as well. For example, our theorizing and discovery is always emergent even when our model describes non-emergent entities like elementary particles (see above). Consequently, Takaki's determination relation is very plausible, but not strong enough to support an inference from the hierarchical structure of knowing to the hierarchical structure of the object known.

As a complement to this determination relation, Takaki interprets Polanyi's ontological emergence in a radically realist way.

> I think what Polanyi is struggling to account for is the sense that hierarchical emergence is in some sense real—which one can glean by moving "up" the hierarchy of the sciences—and not merely an artifact of what embodied realism projects by way of scientific models (theories, etc.). For beyond projection lies the radical nature of tacit knowing: a faith invested in a reality that reveals itself in unlimited ways, thereby affectively fueling the pursuit of truth (Takaki, 37).

I must admit that this interpretation can be defended by numerous citations. However, there are other references that indicate a different version of realism, probably a version that Takaki rejects. For example:

> [S]trictly speaking, it is not the emerged higher form of being, but our knowledge of it, that is unspecifiable in terms of its lower level particulars. We cannot speak of emergence, therefore, except in conjunction with a corresponding progression from a lower to a higher *conceptual* level. And we realize then that conceptual progression may not always be existential, but that it becomes so by degrees (PK 393-394).

We cannot resolve this dilemma here, for sure. Anyway, because of the inseparability of epistemology and ontology, it is hard to sort out whether realism or projected realism is a better interpretation here—if they can be distinguished in Polanyi's theory at all.

Takaki comments on the possibility of inter-theoretic reduction. He has doubts about whether the standard example of this kind of reduction is a successful one, that is, whether phenomenological thermodynamics can be fully reduced to statistical mechanics. There is no need to convince me. I do not believe that it or indeed any alleged inter-theoretic reduction is a satisfactory one. I mentioned the standard inter-theoretic reduction (and its example) in my paper only to contrast it with what I called weak conceptual reduction. I wanted to state clearly that the standard inter-theoretic reduction is not necessary for identification. Much less than that—namely weak conceptual reduction—is enough.

In the final section of his paper, Takaki proposes ingenious variations and alternatives to Polanyi's arguments to save the layered ontological structure, and then he anticipates the possible replies by which I could defend my position. He does it so correctly—I can add hardly anything to them except that I highly appreciate his impartiality that generated sometimes better replies than I could have given. He gives rise to so many intriguing and coruscating links to Polanyi's layered ontology that I cannot discuss them within the framework of this reply. They could only be properly talked over in long afternoons in a quiet garden.

3. Reply to Mihály Héder

In his first and second sections, Héder discusses my arguments against CT and dual control. He raises objections apart from a single exception that are very similar to the ones raised by Gulick and I answered them in the reply to him. Thus they need not be treated here. One point not yet treated is the following:

> Claiming that the watch is determined as much by fundamental laws of physics as a planet also implies that the human act of designing things (using one's knowledge) is determined and there is no freedom or human creativity. I think this is in contradiction with Margitay's final conclusion about the ontological status of knowledge-like entities, or, at the least, he

> weakens his case for knowledge-like entities by denying their causal effect on the physical world (Héder, 32).

Héder is right about that. If the completeness of physics is in contradiction with the theory of dual control, then it also excludes the special form of dual control when knowledge-like entities, reasons, interest, and values would control the motion of a human body through higher level principles. Héder rightly points out that the completeness of physics is in direct contradiction with the causal efficacy of knowledge-like entities on the physical parameters of human bodies. In short, if physics is complete, then knowledge-like entities cannot influence our motion, what we do, how we act. That is, the completeness of physics excludes the possibility of human freedom in the sense that we commonly understand freedom. This is an age old problem much discussed in the philosophy of freedom. However, the lack of causal efficacy of the knowledge-like entities does not refute my final conclusion. Knowledge-like entities can be emergent entities without causal power. Yet it is clear enough that we (i.e., Polanyi and his readers, including me) would find little comfort in the emergence of physically inert knowledge, desires and values that cannot have any effect on our actions.

The situation is even worse than this because the completeness of physics gives rise to questions concerning the epistemic emergence of knowledge, that is, concerning the basis of the ontological emergence of knowledge-like entities.

Although this paper is about the analysis of Polanyi's arguments and not about my personal views, if, none the less, I may voice a personal view, I would suggest that in order to resolve the tension, we should do something about the completeness theorem of physics. However, we cannot dismiss it with simple hand-waving for, as it was pointed out above, the completeness theorem rests on serious epistemological considerations.

In the third part of his paper, Héder develops counterarguments against the possibility of identification by industrial standards. He starts this section with the reconstruction of my argument:

> [H]e goes on to consider technical standards, which in his view are able to fully identify machines. I think this is the least convincing part of his article. True, it might be logically possible that a standard is able to completely describe a machine, but in general, this is not the case. Moreover, exhaustive "physical" description is not even the goal, but only a side effect of standardization (Héder, 32).

However, this is *not* my point of view. I do not think that standards can give or aim at complete description of machines. Moreover—and this is the most important in the present context—full or exhaustive physical description is not necessary for my argument (vi). I deeply regret this misunderstanding because he attacks the straw man's position later on, and he does it in a competent and knowledgeable way.

At one point, he writes:

> The goal of standardization in general is to enable technical collaboration and prevent vendor monopoly. Therefore, a standard should leave as many properties indeterminate as possible without breaking the functionality, so that a standard-compliant artifact can be manufactured in multiple ways and by multiple manufacturers (Héder, 32).

I agree that standards exclude some possible physical realizations of a kind of machines but still leave open the possibility of infinitely many realizations. Furthermore, if the first part of the second sentence is correct, then Héder may put Polanyi's argument from identification in an even more difficult position than my own argument. Provided that the physical-chemical description in a standard specifies a machine to the extent that it is sufficient to guarantee functionality (it is commonplace), *and* it specifies possibly *only up to the extent that is necessary* for this purpose (this is what Héder's sentence seems to imply), then this would mean that physical-chemical descriptions in standards identify not only machines but also their functionality when standardization is successful, when a standard ideally fulfills its role.

In fact, complete description is not necessary for my argument to be effective against Polanyi's. It is enough that standards can specify bolts, wrenches, *bearings*, etc. (contra Héder [32], who thinks specification is possible only for machines having no moving parts) *in physical-chemical terms* such that if an object meets the physical-chemical specs then it is (such and such) a bolt. It is a proper identification of something as something by virtue of its parameters, and only this is required for ontology (By the way, quality controllers, salesmen, transporters, shop assistants, etc. identify, for example, bearings by specs without knowing what they are for or how they can operate).

One might counter that this kind of identification by sufficient conditions has all sorts of semantic, etc. problems. But this is not the point (about this, see Margitay 2010, 138). The point is that the identification by physical-chemical specs is *as good as* the identification by operational principles and higher level concepts. Since the latter can provide identification also only by sufficient conditions, thereby it is just as vulnerable to the semantic, etc. objections as the identification by specs.

To challenge Polanyi's ontological argument from identification, my argument does not require that all machines be identifiable by physical-chemical specifications. Nor does it require that living organisms that are akin to machines, be identifiable this way (see Héder, 33). It is not necessary either that all standards provide physical-chemical descriptions that can successfully identify machines. What my argument does require to be sound, however, is that *some* machines can be identified by physical-chemical descriptions without higher level concepts and operational principles. This much is enough to show that Polanyi's argument from identification cannot establish the emergent status of machines.

4. Reply to David Agler and Gergely Kertész

I treat Agler's and Kertész' brilliant comments together because both have many interesting and valuable things to say about the semantic argument from identification. The two papers put forward various suggestions that can fortify Polanyi's arguments enormously, and these proposals nicely supplement each other to make a really strong case for layered ontology.

Both Agler and Kertész identify two types of semantic arguments by Polanyi. The first one is the Multiple Realizability (MR) argument that is also called by Agler the Many-One argument. In a fine grained reconstruction, it runs like this.

19. A higher level type (a machine type) can be realized by infinitely many lower level entities.
20. Therefore a finite range of the description of lower level tokens (a finite range of physical description of specimens of a machine type) cannot identify a class of machines.

22. The lower level entities that can realize a higher level type belong to *different lower level types*, because a class of machines of the same kind includes specimens of different size, often of different materials, and with an infinite range of other variations (cf. KB 175).
23. Therefore a higher level type corresponds to several heterogeneous lower level types.
24. Therefore a higher level type cannot be identified by a lower level type or by any of their sensible conjunctions. In other words, there can be no *unified* physical-chemical account of what a machine is or how it operates. (Agler, 23 and Kertész, 17)[8]

They both claim that something like this is a correct reconstruction of what Polanyi says, but I think their accounts are definitely stronger than what is literally in Polanyi.

Both Agler and Kertész seem to accept that the argument (vi) from standards is effective against this Polanyian version (Kertész has some reservations that can be dispelled, I think, by what has been said in my previous replies).

Then they take different paths. Agler turns to the reconstruction of the second semantic argument (I shall come back to that later) while Kertész sets out to improve this one.

As a first step, Kertész points out that, though the descriptions provided by standards are enough for identification, they do not give us an explanation. An explanation would require a complete inter-theoretic reduction. A weak conceptual reduction does "not explain in the terms of the lower level *the higher level order* that is grasped by higher level concepts" (Kertész, 19, my italics). Why do we need this explanation? Kertész turns to Fodor's theory for the answer. Stronger reduction is necessary because—says Kertész (Kertész. 19-20)—the unity of the lower level kinds and the higher level laws by which they can be covered are simply invisible from a lower level perspective. The set to which lower level types belong can only be constructed by the use of higher level concepts and criteria.

Kertész believes that this improved version of the MR argument stands firm against the counterexample of standards. However, I am not fully convinced *yet*. Identification is, for sure, necessary for something to be part of an ontology, and industrial standards can provide us identification. But why do we need unity and the explanation of unity for an ontology? The purely ontological answers to this question generally refer to pragmatic (or aesthetic) reasons. We need unity, because we need ontological simplicity to get a manageable ontology. We should use Occam's razor because the simpler an ontology is the better it is. One could counter that industrial standards involve a very easily manageable and simple ontology. So I do not think that the Polanyi-Fodor-Kertész version of the MR argument really can block my argument from standards.

It seems likely that there is no ontological answer for the question why unity and explanation are necessary for an ontology. However, the Polanyian epistemology can answer this question, and this is the point where the inseparability of epistemology from ontology plays a crucial role. I suggest that we should add one more premise to Kertész' argument: We cannot comprehend an ontological category without unity and without the explanation of this unity. The comprehension of a whole requires that we see its parts in a comprehensive unity. Thus the comprehension of a machine type, that is, a class of machines as an ontological whole presupposes that we see its different type of physical-chemical realizations *in unity as they are integrated into the machine type*. That is we should understand the different lower level kinds in unity *and* we should also understand that this particular unity as the one that makes up (integrated into) the machine type. The

explanation of the unity in terms of the higher level principles unifying the different lower level kinds is part of the latter. So according to Polanyi's theory of knowing, *conceptual* comprehension of an ontological whole presupposes both the unity of the lower level types that realize the higher level whole and the explanation for this unity in terms of higher level principles. With the help of this premise, it follows that if we can conceptually comprehend a machine type as part of our ontology, then we can treat it only as an emergent entity in the lack of a complete inter-theoretic reduction of the machine type to its physical-chemical particulars. And since we do not possess such inter-theoretic reduction and still can comprehend machines as parts of our ontology, therefore, they are emergent entities.

This sketch needs some more work to become a fully-fledged argument. However it is already clear that this result can also help me to reformulate the Correspondence Thesis in a more tenable form.

The Context-Dependence (CD or One-Many) argument is the second semantic argument that is treated by both Agler and Kertész although with a different emphasis on it. Kertész only mentions part of it and calls it the Underivability argument while Agler develops it into an independent argument for the emergence of machines.

Agler proceeds from the Polanyian tenet that higher level laws and properties that determine machine types, cannot be derived from physical-chemical configurations and from their lower level laws. This makes it possible that "a one-many relation exists between physical-chemical particulars and higher-level types wherein the same physical-chemical particulars can be realized in several different machine types" (Agler, 24) or in non-machine types. Thus it is not the object by itself that makes something to be a machine, but rather a particular machine-human relationship. Agler invites us to consider a scenario in which there are two physically identical objects. One of them is "the result of human invention and plays an instrumental role in human life" (Agler, 25) while the other emerged naturally by chance and has nothing to do with human beings.[9] A physicist using *only* physical observations could not tell the difference between them, though intuitively they are different. Their difference lies in their different relationship to human beings, or rather on the other way round; they are different because *we* have different relationships to them. This contextual factor makes the distinction. From this, Agler concludes that "[t]here is thus no one-to-one reduction between a unitary description of physical-chemical particulars and a machine type as machines only exist relative to the role they play in intelligent activity" (Agler, 25). This is an ingenious and a very impressive argument, I *must* admit. I really seem to be compelled to accept it because I am committed to the emergence of the knowledge-like entities. And, according to Agler, ultimately certain knowledge-like entities make one object to be a machine while the other is only a natural object.

5. Reply to Apczynski

In his succinct essay written in a personal tone, Apczynski says basically that I fundamentally misunderstood Polanyi, or, putting it in a less harsh way that fits better to his own gentle style, there is another interpretative frame neglected by me that could provide a more fruitful reading of Polanyi. I asked the wrong questions, especially with respect to the Correspondence Thesis, and therefore got wrong answers. Had I approached the text differently, I might have found what I was looking for: the insight that the world really *has* a layered ontological structure. Apczynski's approach is startling like a koan, like some teaching methods of Buddhist masters.

I approached Polanyi's corpus as part of the theoretical philosophical discourse, and asked what arguments he has to support for his far-reaching theory of stratified ontology. These arguments were scrutinized and they turned out to be defective.

However, Apczynski protests against this method and he is particularly dissatisfied with my reconstruction of the Correspondence Thesis. According to him, "Polanyi does not propose a 'correspondence thesis' as Margitay presents it" (Apczynski, 28). Consequently, Polanyi cannot build an argument on it. The strong claims from TD (in my compilation) do not reflect accurately "Polanyi's position on the structure of tacit knowing and the hierarchical structure of comprehensive entities" (Apczynski, 28). He criticizes, probably, not so much my reconstruction of Polanyi's words because Polanyi's texts were taken literally and closely followed in the course of the reconstruction of CT and his arguments, but rather my reading of these texts, my use of these passages. I focused on the text, and looked for the explicit reasons for the layered ontology.

To give me a hint about what I could have done as an alternative to this, Apczynski tells me a parable, about how Polanyi tried to illuminate his new insights to his dogmatic contemporaries. According to this parable, to find the right meaning of the cited passages, I should look at reality *through* them. I should use them as clues to see the ontological structure of reality. His very Polanyian criticism is that I relied on the explicit, focal meaning of these texts but their true meaning is what they have as clues in the focal meaning of the stratified ontology. Perhaps, instead of calling upon Polanyi to give explicit reasons in his texts, I should see the Gestalt of the layered ontology of reality dwelling in his texts.

I would be fully persuaded if he said that I should do both (i.e., look *through* and *at*). Argumentation, conceptual theorization and other methods used in the philosophical tradition are part of Polanyi's adopted arsenal; he uses them extensively, so it is not unfair to call upon him to use them rigorously in such a crucial question. Apczynski grants that "Margitay seems to acknowledge this [the alternative interpretation he proposed] when he suggests that we may take the Correspondence Thesis as something like a heuristic device" (133-34—seeApczynski, 28), but then I have not developed it into a real alternative. Again, I should agree.

Apczynski's comment points to a further possible conclusion too. Polanyi develops a philosophical system which includes a systematic account of knowledge, ontology and human values. However, his works can also be construed as edifying philosophy (cf. Rorty 1980), and included in his works—in the theory of tacit knowing—is how they are to be read as edifying philosophy. In his comment, Apczynski gave a masterful example how to do this Polanyian edifying philosophy in practice.

Endnotes

[1]While working on this paper, I was supported by the TÁMOP 4.2.2.B-10/1-2010-0009 project.

[2]It is not an entirely happy expression but its meaning is clear from Polanyi's description of how one can have access to another person's knowledge:

> [W]e consider the way one man comes to understand the skillful performance of another man. He must try to combine mentally the movements which the performer combines practically and he must combine them in a pattern similar to the performer's pattern of movements. Two kinds of indwelling meet here. The performer co-ordinates his moves by dwelling in them as part of his body, while the watcher tries to correlate these moves by seeking to dwell in them from outside. He dwells in these moves by interiorizing them (TD 29-30).

> Man's skillful exercise of his body is a real entity that another person can know, and knows only by comprehending it, and that comprehension of this real entity has the same structure as the entity which is its object (TD 33).

[3]See, e.g., Griffiths 2008 about the development of particle physics and about the Standard Model. Cf. also "Elementary Particle", Wikipedia http://en.wikipedia.org/wiki/Elementary_particle#cite_note-1 (12.12. 2012.)

[4]Actually, it seems that it is even more restricted than the original Polanyian version. There are numerous examples in which discoveries relied on a wide range of historically accumulated clues, yet what was discovered was quite particular. The recent discovery of the water on Mars could be an example for this.

[5]Putting the matter this way indicates that the indeterministic nature of Quantum Theory does not matter here. First, because machines are macroscopic objects that behave deterministically in practice in accordance with classical physics. Second, because even if machines were microscopic objects that are subject to quantum indeterminacy, it would be quantum theory and the prior state of the system that would determine the probabilistic behavior of the system and *not* an operational principle.

[6]I discussed them in Margitay 2007.

[7]It is worth comparing Takaki's and Giere's (1990) concept of model and its role in science.

[8]In fact, I cheated in the reconstruction of the argument. I have made it up from the two reconstructions given by Agler and Kertész.

[9]This scenario resembles the one described by Putnam (1980 1-4) in which an aunt draws the picture of Churchill by randomly crawling around.

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