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40 years of ZFN – the flexibility of youth and the experience of age

Michał Heller

Różne rocznice skłaniają do historycznych refleksji. Wprawdzie czterdzieści lat od jakiegoś wydarzenia nie zwykło się specjalnie świętować, ale w przypadku naukowego czasopisma jest to już na tyle długi okres, że warto mu poświęcić chwilę zamyślenia.

Zagadnienia Filozoficzne w Nauce zaczynały skromnie, ale ambitnie: od zgrzebnego formatu typu „samizdat" (bibułowy papier, powielacz), poprzez pierwsze przymiarki do komputerowego druku (nadal bibuła), aż do postaci naprawdę drukowanej. Kolejne numery *Zagadnień* są wiernym świadkiem stopniowych postępów w polskiej, pokomunistycznej sztuce drukarskiej: najpierw druk oszczędny, na miarę dostępnych technik, potem stopniowe ulepszenia, by wreszcie dojść do wysmakowanego, nawet trochę snobistycznego, układu graficznego.

Tytuł *Zagadnienia Filozoficzne w Nauce* od samego początku zapowiadał pewien filozoficzny program. Że ma to być coś o wzajemnych relacjach nauki i filozofii – było oczywiste, ale akcent nie był położony ani na „zagadnieniach filozoficznych", ani na „nauce", lecz na przyimku „w". Filozofia nauki jest – i była już wtedy – dobrze rozwiniętą dyscypliną filozoficzną. Miała swoje liczne szkoły i odmiany. Najbardziej wpływową do dziś pozostaje filozofia nauki

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zwana (nie całkiem merytorycznie poprawnie) anglosaską lub analityczną, ale analizy metodologiczne wywodzące się z Filozoficznej Szkoły Lwowsko-Warszawskiej także cieszyły się – i nadal cieszą się – niemałym poważaniem. Założycielom czasopisma chodziło o coś innego, o to jak tradycyjne pytania filozoficzne są obecne w badaniach naukowych i ich rezultatach. Tradycyjna filozofia, stworzona przez Greków i przetworzona przez europejską myśl średniowieczną nie tylko wydała z siebie nowożytne nauki empiryczne, lecz również wycisnęła na nich swoje ślady. Odczytywanie tych śladów i odcyfrowywanie ich znaczeń jest pasjonującym zadaniem, którego zaniedbanie byłoby niepowetowaną stratą dla współczesnej kultury. Z czasem dla tego typu uprawiania filozofii przyjęła się nazwa „filozofia w nauce" – nie „filozofia nauki", lecz właśnie ten mały przyimek „w".

Jest rzeczą oczywistą, że do realizowania w ten sposób zarysowanego programu niezbędne jest wykorzystywanie środków poznawczych i metodologicznych narzędzi wypracowanych przez filozofię nauki. To jednak nie wszystko. Ślady filozoficznych pytań rzadko leżą na powierzchni wytworów nauki. Nie jest więc tak, że aby je odczytać, wystarczy przywołać znajomość tradycyjnej filozofii i zastosować odpowiednie metodologiczne narzędzia. Filozoficzne tropy prowadzą często w głąb naukowych teorii, ich inspiracji i wniosków. Do tego niezbędna jest dogłębna znajomość samej nauki (najlepiej, jeżeli wynika ona z twórczego jej uprawiania). Tylko wnikając głęboko w tkankę naukowych teorii, można zidentyfikować ich filozoficzne uwarunkowania i poddać je trafnej interpretacji

Potrzebne jest także jeszcze inne wsparcie. Historia nauki nie tylko wiąże naukę, poprzez jej rodowód, z tradycją filozoficzną, lecz także bardzo skutecznie naprowadza na filozoficzne pozostałości w dzisiejszych naukowych dokonaniach. Dlatego też „filozofia w nauce" ściśle wiąże się z historią nauki. W tym mariażu historia nauki nie sprowadza się do odtwarzania dziejów naukowych odkryć, lecz staje się aktywnym narzędziem badania.

Nawet pobieżne przejrzenie spisów treści poszczególnych numerów *Zagadnień Filozoficznych w Nauce* przekonuje, że wśród autorów tego pisma pojawiają się: filozofowie, filozofowie nauki, historycy nauki, matematycy, fizycy, astronomowie, biologowie i przedstawiciele innych nauk. Dobre czasopismo to nie tylko kolejne numery drukowane na papierze lub pojawiające się w Internecie, lecz także środowisko, jakie wokół niego się skupia – kształtuje je i samo jest przez nie kształtowane. W Krakowie, przynajmniej od końca dziewiętnastego wieku, żywe są tradycje „filozofujących uczonych" i dialogu między przedstawicielami różnych nauk a filozofami. Kluczowymi pod tym względem są takie postacie jak: Tadeusz Garbowski, Władysław Heinrich, Joachim Metallmann, Marian Smoluchowski, Władysław Natanson… *Zagadnienia* wpisują się w zapoczątkowaną przez nich tradycję i pozwalają jej promieniować poza Kraków.

Czasopisma tym różnią się od książek, że przeczytaną książkę po prostu odkłada się na półkę, a czasopismo odradza się z każdym nowym numerem i, jeżeli jest dobrze wrośnięte w środowisko, mimo iż przybywa mu lat, zachowuje sprężystość młodości i wzbogaca ją doświadczeniem dojrzałego wieku.

Kraków, 22 lutego 2019 roku

Emergence of the Classical

Independent quantum systems and the associativity of the product of quantum observables

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Abstract

We start from the assumption that the real valued observables of a quantum system form a Jordan algebra which is equipped with a compatible Lie product characterizing infinitesimal symmetries, and ask whether two such systems can be considered as independent subsystems of a larger system. We show that this is possible if and only if the associator of the Jordan product is a fixed multiple of the associator of the Lie product. In this case it is known that the two products can be combined to an associative product in the Jordan algebra or its complexification, depending on the sign of the multiple.

Keywords

quantum systems, quantum observables.

1. Introduction

In quantum theory, the (real valued) observables are self-adjoint elements of a complex associative involutive algebra. This structure is quite different from the classical case where the observables form

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a Poisson algebra, i.e. an algebra over the reals with a commutative and associative product and a Lie product inducing derivations for the commutative product.

As emphasized by Niklas Landsman in his book (1998), the structure in the quantum case can be formulated in an analogous way by equipping the selfadjoint part of the algebra with the Jordan product (i.e. $\frac{1}{2}$ times the anticommutator) and a Lie product defined as $\frac{i}{\hbar}$ times the commutator. Both products have a physical motivation quite similar to the classical case. In particular the induced derivations of the Jordan product by the Lie product are motivated by their interpretation as infinitesimal symmetries, and the Jacobi identity for the Lie product may be understood as a consistency condition on symmetries. Both products are non-associative, and the associator of the Jordan product is $\hbar^2/4$ times the associator of the Lie product.

The question we want to analyze in this paper is whether the latter relation between the associators can be physically motivated. Mathematically it implies that both products can be combined to an associative product in a complexification of the algebra. This algebra has an antilinear involution, and its self-adjoint part is the original Jordan algebra with the Lie product given in terms of the commutator.

To answer this question we add the requirement that independent physical systems can be considered as parts of a larger system, such that the properties of the subsystems are not influenced by the embedding into the larger system. We show that the validity of the Jacobi identity in the composed system implies that the associators of the Jordan products are proportional to the associators of the Lie product, with a proportionality constant which is independent of the system. If the constant is positive, one obtains an associative product in the complexified algebras, and the composed system arises as the self-adjoint part of the tensor product of the associative algebras.

The idea to derive the associative product of quantum physics from the composibility of systems was first discussed in the paper of Grgin and Petersen (1976) and reconsidered more recently by Kapustin (2013) and Moldoveanu (2015). A related but independent result applying to the infinite dimensional case can be found in (Hanche-Olsen, 1985), see also the book (Hanche-Olsen and Størmer, 1984). Contrary to these works we do not make any *a priori* assumptions on the way the larger system can be built from the subsystems.

2. Jordan-Lie algebras

A Jordan algebra is a real vector space A equipped with a commutative product $\circ$, i.e. a bilinear map

$$A \times A \to A, (a, b) \mapsto a \circ b$$

with $a \circ b = b \circ a$. This product is not necessarily associative, instead only the weaker relation

$$(a^2 \circ b) \circ a = a^2 \circ (b \circ a) \tag{1}$$

holds, where $a^2 = a \circ a$. Jordan introduced this concept in order to describe the structure one can expect for quantum observables. Indeed, the linear structure may be motivated by Ehrenfest's Theorem stating that expectation values add as in classical physics (see e.g. Arodź, 2019); labeling of measurement results in terms of real numbers may be redefined by applying a mapping $\mathbb{R} \to \mathbb{R}$, so in particular squares of observables can be defined, and a commutative product can be introduced by

$$a \circ b \doteq \frac{1}{2}((a + b)^2 - a^2 - b^2) \ .$$

The condition (1) follows from the requirement that powers are well defined,

$$a^n \circ a^m = a^{n+m}$$

where $a^1 = a$, $a^{n+1} = a^n \circ a$, under the additional positivity condition

$$\sum a_i^2 = 0 \ \Rightarrow a_i = 0 \ . \tag{2}$$

(See (Jordan, Neumann and Wigner, 1934); such a Jordan algebra is called formally real.) Finite dimensional Jordan algebras can be classified. Besides the standard case of selfadjoint subalgebras of associative involutive algebras over $\mathbb{R}, \mathbb{C}$ or $\mathbb{H}$ (the quaternions) one has a few exceptional cases. We only consider unital Jordan algebras, i.e. there is an element $1 \in A$ which satisfies the relation

$$1 \circ a = a \forall a \in A \ . \tag{3}$$

For finite dimensional Jordan algebras the existence of the unit is a consequence of the positivity condition (2).

In addition to the Jordan product of observables one has in quantum theory a Lie product in terms of commutators which describes the dual role of observables as generators of infinitesimal symmetries. The standard example is Heisenberg's equation of motion characterizing the time evolution, and it corresponds directly to the Poisson bracket of classical physics, as first observed by Dirac. The arising structure has been analyzed by Landsman (1998). He defines a Jordan-Lie algebra as a Jordan algebra $(A, \circ)$ with a Lie product, i.e. a bilinear map

$$A \times A \to A \ , \ (a, b) \mapsto [a, b]$$

which is antisymmetric

$$[a, b] = -[b, a]$$

and satisfies the Jacobi identity

$$[[a,b],c]+[[b,c],a]+[[c,a],b]=0\ . \tag{4}$$

The Lie product is related to the Jordan product by two relations. The first is the Leibniz rule

$$[a\circ b,c]=a\circ[b,c]+[a,c]\circ c\ . \tag{5}$$

This rule is motivated by the interpretation of the map $A \ni a \mapsto [a,c]$ as an infinitesimal symmetry. The second relation involves the associators. Denote the associator of the Jordan product by

$$[a,b,c]\doteq(a\circ b)\circ c-a\circ(b\circ c)$$

and the associator of the Lie product by

$$[[a,b,c]]\doteq[[a,b],c]-[a,[b,c]]\equiv[[a,c],b]\ .$$

Then the relation is

$$[a,b,c]=\frac{\hbar^2}{4}[[a,b,c]]\ . \tag{6}$$

One then can introduce a product $\cdot$ on the complexification $A\otimes\mathbb{C}$ of A, by

$$(a\otimes z)\cdot(b\otimes w)=(a\circ b)\otimes zw+[a,b]\otimes\frac{i\hbar zw}{2}, \tag{7}$$

which turns out to be associative due to (6). One thus obtains the standard structure of the algebra of quantum observables. It remains open whether the relation (6) between the two associators has a physical interpretation.

We therefore introduce the concept of a q-algebra where the condition (6) is not imposed. We also do not require the Jordan condition (1) and the positivity relation (2)

Definition 2.1. A q-algebra is a real vector space equipped with a commutative product $\circ$ and an antisymmetric product $[,]$. It contains a unit for the commutative product (3) and satisfies the Jacobi identity (4) and the Leibniz rule (5).

3. Independent subsystems

Let A, B and C be q-algebras. To model the requirement that A and B represent independent subsystems of the larger system represented by C we require the following relations:

Definition 3.1. Let $\alpha : A \to C$ and $\beta : B \to C$ be monomorphisms of q-algebras. The pair (α, β) is called an embedding of independent subsystems if the following conditions are satisfied:

1. the map
$$A \times B \ni (a, b) \to \alpha(a) \circ \beta(b) \in C$$
extends to an injective linear map $\alpha \otimes \beta : A \otimes B \to C$.
2. the infinitesimal symmetries implemented by elements of A act trivially on B and vice versa,
$$[\alpha(a), \beta(b)] = 0 \; \forall a \in A, b \in B, \tag{8}$$
3. the $\circ$-product with an observable of one of the subsystems does not affect the $\circ$-product in the other subsystem (the observables from A are compatible with the observables from B in the context of Jordan algebras (Hanche-Olsen and Størmer, 1984))
$$(\alpha(a)\circ(\alpha(a')\circ\beta(b)) = \alpha(a\circ a')\circ\beta(b)\,, (\alpha(a)\circ\beta(b))\circ\beta(b') = \alpha(a)\circ\beta(b\circ b')\,, \tag{9}$$

In the following we omit the symbols α and β by identifying A and B with their embeddings in C. Moreover, we delete the symbol $\circ$ for the commutative product and replace it by juxtaposition. We first determine the antisymmetric product in the image C_0 of $\alpha \otimes \beta$:

Lemma 3.0.1. The antisymmetric product in C_0 is given by

$$[ab, a'b'] = [a, a'](bb') + (aa')[b, b'] \,, a, a' \in A,\ b, b' \in B \,. \tag{10}$$

In particular, C_0 is closed under the antisymmetric product.

Proof. By (5), (8) and (9) we have

$$[ab, a'b'] = a[b, a'b'] + [a, a'b']b = a(a'[b, b']) + ([a, a']b')b = [a, a'](b'b) + (aa')[b, b'] \,.$$

□

In the next step we analyze the consequences of the Jacobi identity within C_0. We compute the second antisymmetric product, with $a_i \in A, b_i \in B, i = 1, 2, 3$,

$$[[a_1b_1, a_2b_2], a_3b_3] = [[a_1, a_2]b_1b_2 + a_1a_2[b_1, b_2], a_3b_3]$$

$$= [[a_1, a_2], a_3](b_1b_2)b_3 + (a_1a_2)a_3[[b_1, b_2], b_3] + [a_1, a_2]a_3[b_1b_2, b_3] + [a_1a_2, a_3][b_1, b_2]b_3 \tag{11}$$

In the last 2 terms we apply the derivation property (5) and obtain 4 terms,

$$[a_1, a_2]a_3[b_1b_2, b_3] + [a_1a_2, a_3][b_1, b_2]b_3$$

$$= [a_1, a_2]a_3b_1[b_2, b_3] + [a_1, a_2]a_3[b_1, b_2]b_3 + a_1[a_2, a_3][b_1, b_2]b_3 + [a_1, a_3]a_2[b_1, b_2]b_3 \,.$$

If we perform a cyclic sum over the indices we see that the 1st and the 4th term cancel, and also the 2nd and 3rd term.

Thus for the Jacobi identity only the first 2 terms in (11) contribute. We use the Jacobi identities in A and B,

$$[[a_1, a_2], a_3] = -[[a_2, a_3], a_1] - [[a_3, a_1], a_2] \,,\ [[b_1, b_2], b_3] = -[[b_2, b_3], b_1] - [[b_3, b_1], b_2]$$

The Jacobi identity in C then amounts to the relation

$$0 = [[a_2, a_3], a_1]((b_2b_3)b_1 - (b_1b_2)b_3) + [[a_3, a_1], a_2](b_3b_1)b_2 - (b_1b_2)b_3) + (a \leftrightarrow b))$$

$$\equiv [[a_2, a_1, a_3]][b_3, b_2, b_1] + [[a_3, a_2, a_1]][b_3, b_1, b_2] + (a \leftrightarrow b)).$$

To simplify this expression we choose $a_3 = a_1$. Then both associators $[[a_3, a_2, a_1]]$ and $[a_3, a_2, a_1]$ vanish, and we obtain the relation

$$[[a_2, a_1, a_1]][b_3, b_2, b_1] + [a_2, a_1, a_1][[b_3, b_2, b_1]] = 0 \; . \tag{12}$$

We want to exclude the possibility that $[[a_2, a_1, a_1]] = 0 \forall a_1, a_2 \in A$. If all these quantities would vanish, the associator for the antisymmetric product would be totally antisymmetric and hence had to vanish because of the Jacobi identity. We therefore require that the associator of the antisymmetric product in A is nonvanishing. Since C_0 is as a vector space isomorphic to $A \otimes B$, we find the relation

$$[b_3, b_2, b_1] = \lambda[[b_3, b_2, b_1]] \tag{13}$$

for some $\lambda \in \mathbb{R}$.

Finally, we determine the symmetric product in C_0, under the assumption that the associator relation (13) holds within C. By the independence of the embeddings we have

$$(ab)b' = a(bb') \text{ and } a(a'b) = (aa')b.$$

We now compute $(ab)(a'b')$. We have by the definition of the associator

$$(ab)(a'b') = ((ab)a')b' - [ab, a', b] \; .$$

We apply (9) twice and obtain

$$((ab)a')b' = (a'(ab))b' = ((aa')b)b' = (aa')(bb') \; .$$

Thus, due to the relation (13) between the associators,

$$[ab, a', b'] = \lambda[[ab, b'], a'] = \lambda[a[b, b'], a'] = \lambda[a, a'][b, b'] \; .$$

We therefore arrive at the formula for the symmetric product

$$(ab)(a'b') = (aa')(bb') - \lambda[a,a'][b,b'] \,. \tag{14}$$

We conclude that C_0 is also closed under the symmetric product.

An embedding of A and B can be constructed if both satisfy (13) with the same λ. Let $A\otimes B$ be the tensor product of the vector spaces A and B. We introduce a symmetric product

$$(a\otimes b)\circ(a'\otimes b') = aa'\otimes bb' - \lambda[a,a']\otimes[b,b']$$

and an antisymmetric product

$$[a\otimes b, a'\otimes b'] = [a,a']\otimes bb' + aa'\otimes[b,b']$$

and obtain a q-algebra $A\otimes_\lambda B$ together with maps $\alpha : A\to A\otimes B,\ a\mapsto a\otimes 1, \beta : B\to A\otimes B,\ b\mapsto 1\otimes b$ which satisfy the condition of an independent embedding. Moreover, also the associators in $A\otimes_\lambda B$ satisfy the associator relation (13).

We arrive at the following theorem:

Theorem 3.1. Let A, B be q-algebras with nontrivial associators for the antisymmetric products. Then an embedding as independent subsystems exists if and only if the associators in A and in B are related by (13) with the same λ. Moreover, given any such embedding $(\alpha,\beta) : A\times B\to C$ where C also satisfies (13), there is a unique injective homomorphism $\gamma : A\otimes_\lambda B\to C$ with $\gamma(a\otimes b) = \alpha(a)\circ\beta(b),\ a\in A,\ b\in B$.

Proof. Assume that an independent embedding exists. Then, from (12), we conclude the relation (13) for B. The argument for A follows analogously. If, on the other side, (13) holds for both A and B, we can construct $A\otimes_\lambda B$ as an example for an independent embedding. Now let $(\alpha,\beta) : A\times B\to C$ be any independent embedding. The map γ given in the Theorem is by definion a linear monomorphism and preserves the unit. From (14) and (10) we then conclude that also both products are preserved, hence γ is a monomorphism of q-algebras. $\square$

4. The operator product

Let A be a q-algebra which satisfies the associator equality for some $\lambda \in \mathbb{R}$. We distinguish three cases:

$\lambda = 0:$ In this case the $\circ$-product of A is associative, and we are in the situation of classical physics.

$\lambda < 0:$ For $\lambda < 0$ we can introduce in A an associative noncommutative product by

$$a \bullet b = a \circ b + \sqrt{|\lambda|}[a, b] \ .$$

The $\circ$ product is then $\frac{1}{2}$ times the anticommutator, and it is easy to see that also the condition (1) for Jordan algebras is fulfilled. If A is finite dimensional, the positivity condition (2) cannot be fulfilled for associative algebras (Braun and Koecher, 1966). It is likely that this remains true in the infinite dimensional case, but existent results use additional input, in particular the existence of a norm. See (McCrimmon, 2004) for an overview.

$\lambda > 0:$ For $\lambda > 0$ we define a product in the complexification $A \otimes \mathbb{C}$ of A as in (7) with $\hbar = 2\sqrt{\lambda}$ and an antilinear involution

$$(a \otimes z)^* = a \otimes \overline{z} \ .$$

A is then the self-adjoint subspace of the complex associative involutive algebra $A \otimes \mathbb{C}$, hence we obtain the well known structure of quantum theory.

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Ehrenfest's Theorem Revisited

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Abstract

Historically, Ehrenfest's theorem (1927) is the first one which shows that classical physics can emerge from quantum physics as a kind of approximation. We recall the theorem in its original form, and we highlight its generalizations to the relativistic Dirac particle and to a particle with spin and izospin. We argue that aparent classicality of the macroscopic world can probably be explained within the framework of standard quantum mechanics.

Keywords

Ehrenfest's theorem, wave packets, quantum vs. classical world.

1. Introduction

The principal aim of both classical and quantum mechanics is to describe motions of certain physical objects. Both theories can be very successfully applied to various physical objects, but the sets of these objects do not coincide. As is well known, classical mechanics gives wrong predictions when applied to microscopic objects such as atoms. On the other hand, it seems that quantum mechanics is capable to correctly describe motions of the elementary particles as well as motions of macroscopic bodies, hence it has a wider range of ap-

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plicability. Nevertheless, there are phenomena description of which requires a theory still more general than quantum mechanics. For example, scattering of elementary particles can lead to creation or annihilation of particles—here quantum field theory is needed. Such a generalization of quantum mechanics to quantum field theory is well-known since the middle of 20^{th} century. There are still some problems with it, but the prevailing opinion is that they concern more its mathematical side than foundations. Another departure from standard quantum mechanics seems to be necessary when an elementary particle interacts with a very complex, perhaps even randomly fluctuating or unstable, environment. Understanding of this case is rather poor. To a certain degree the situation is analogous to the well known partition of classical electrodynamics into the theory of the electromagnetic field in vacuum and the electrodynamics of continua with constitutive relations and other additional ingredients. An effective quantum mechanics in continua is still under construction.

It turns out that classical mechanics can be derived from quantum mechanics as a kind of approximate theory. Such derivations are usually called classical limit of quantum mechanics. There exist several of them, including the discussed below Ehrenfest type classical limit, which dates back to 1927, and is likely the oldest one. Its main feature is that it links solutions of the pertinent fundamental evolution equations, which are the Schroedinger wave equation in quantum mechanics and the Newton equation of motion in classical mechanics. Other kinds of classical limits are carried out on more advanced levels of theory. For instance, one may derive from quantum mechanics the classical Hamilton-Jacobi equation (see, e.g. Schiff, 1968, chap.8, sec.34), the Lagrange formalism (see, e.g. Rosen, 1969, chap.1, sec.2), or distributions on phase space (see, e.g. Siegel, 1976; Curtright, Fairlie and Zachos, 2014).

Our main goal here is to recall the seminal paper (Ehrenfest, 1927), and to show, using modern examples, how fruitful is the invented by Paul Ehrenfest method for deriving classical mechanics from quantum mechanics. It leads to very interesting extended versions of classical mechanics featuring, e.g., a non relativistic particle with spin and izospin, or a relativistic particle with spin, which all emerge from quantum mechanics. Furthermore, Ehrenfest's theorem provides a tantalizing suggestion that perhaps whole classical physics can be recovered as certain approximations to quantum theories. Considering wave packets, we find some arguments that corroborate this idea.

The present article is addressed to audience wider than just theoretical physicists. Nevertheless, certain familiarity with basic equations of classical and quantum mechanics is assumed.

The plan of the article is as follows. First, we briefly discuss description of states of a particle in quantum mechanics with emphasis on the so called wave packets. Section 3 is devoted to the original form of Ehrenfest's theorem. In Section 4 we sketch a solution of the main problem with the Ehrenfest method: the lack of relativistic covariance. Next, in the Section 5 we discuss certain extension of that theorem, which leads to a less known example of classical mechanics of a point-like particle with spin and izospin. Section 6 contains remarks on applicability of quantum mechanics to macroscopic bodies, including a new argument for practical nonexistence of so called Schroedinger's cats.

2. Quantum states of a particle

Classical mechanics and quantum mechanics address the same issue: description of motions of a set of particles. Such set could consists of just one particle, or a finite number of them. The restriction to finite number of particles is important, because otherwise one would have to use a field theory which is regarded as different from mechanics for several important reasons. Classical and quantum mechanics are structurally similar to each other in the sense that in both theories we introduce a space of states of the particle and we postulate an equation of motion. They differ in the form of equation of motion: in classical mechanics this can be, for example, the Newton equation, while in quantum mechanics the Schrödinger equation. Also the spaces of states are very different. For instance, for the simplest single, point-like particle it is six dimensional phase space in classical mechanics, and infinite dimensional Hilbert space in quantum mechanics. The different equations of motion, and different sets of measurable properties (called observables) for the same set of particles are possible because the spaces of states in classical and quantum mechanics are not identical. Therefore, we regard this latter difference as the most important one.

In this article we consider the simplest particles, which we describe as elementary. Particles which possess constituents, for example, hadrons, nuclei, or atoms, are excluded. Physical incarnations of the elementary particles are, e.g. electrons, photons, quarks, or the Higgs particle.

The term 'point-like particle' used above is well justified only in classical mechanics. It refers to the fact that in the simplest case the state of a single particle at fixed time t is given by the position and velocity of the particle. The position is represented by a point in the R^3

space. In quantum mechanics the complete description of the state of the particle at a given time t is provided by a smooth wave function $\psi(\vec{x}, t)$ defined on the R^3 space.[1] There is no reason to relate such a quantum particle with a material point moving in the space. Rather, it should better be pictured as a cloud of matter of a very special kind, which is present at all points where the wave function does not vanish. In particular, it does not have any constant shape or size. The most peculiar feature of the elementary quantum particle is that it can not be destroyed or created in parts in spite of its spatial extension, while, for example, a drop of water can be divided into parts, and one part evaporated without disturbing the remaining parts. Physical processes always involve whole elementary quantum particles, which are single indivisible entities, albeit spatially extended.[2] With such picture of the quantum particle, the often discussed and experimentally verified nonlocality of quantum mechanics is natural and rather obvious feature. We shall return to the question what is the best intuitive picture of the quantum particle in the last section.

Certain special clouds of quantum matter are called wave packets. Roughly speaking, a wave packet is compact and it consists of a single bit, as opposed to more general quantum states of the particle which, for example, can consist of several non-overlapping compact bits. Change in time of any state is described by the Schrödinger equation. It turns out that in the case of particle in empty space typical wave packet expands. For example, the width $l(t)$ of a three dimensional (spherical Gaussian) wave packet for a parti-

[1] For simplicity, we consider here only so called pure states, omitting more general mixed states.

[2] In literature this feature is often referred to as the unitarity.

cle at rest is given by the formula [p.66](Białynicki-Birula, Cieplak and Kamiński, 1991)

$$l(t) = \sqrt{l_0^2 + \frac{\hbar^2 t^2}{m^2 l_0^2}},$$

where l_0 is the initial width at $t = 0$, m is the mass of the particle, $\hbar$ is the Planck constant. This formula implies that the velocity of the expansion monotonically increases to the asymptotic value

$$v_0 = \frac{\hbar}{m l_0}.$$

The value of Planck's constant is $\hbar = 1.0545 \cdot 10^{-34} J \cdot s$, and the masses of electron and proton are, respectively, $m_e = 9.1 \cdot 10^{-28} g, \quad m_p = 1.67 \cdot 10^{-24} g$. We would like to draw attention of the reader to the exceedingly small values of these masses. The hydrogen atom H—one proton plus one electron, and the hydrogen molecule H_2—two hydrogen atoms, also are very very light. If we would like to have hand-picked one milligram of hydrogen gas,[3] adding one molecule H_2 per second, it would take about 10^{13} years, while the estimated age of our Universe is about $1.4 \cdot 10^{10}$ years. One should be very cautious when extrapolating our picture of macroscopic particles to such tiny objects.

It is instructive to compute the asymptotic velocity v_0 for various masses and initial widths. Let us first take as the initial width $l_0 = 10^{-8} cm$, which is the typical atomic size. Then, for the electron we find $v_0 \approx 1160 \ \frac{km}{s}$. For a nucleus with the mass $m = 100 m_p$, $v_0 \approx 6.4 \ \frac{m}{s}$. However, already for a 'speck of dust' of size $l_0 = 10^{-6} cm$ and mass $m = 10^{-4} g$ the velocity is $v_0 \approx 10^{-13} \ \frac{cm}{s} \approx 3.2 \cdot 10^{-10} \frac{cm}{year}$. This means that the wave packet will increase by

[3] About 11 ccm at 0^oC and the normal pressure.

1% during 30 years. For a drop of water in a fog, $l_0 = 10^{-1} cm$, $m = 10^{-2} g$, and $v_0 \approx 3 \cdot 10^{-17} \frac{cm}{year}$. Thus we see that the electron in empty space expands very rapidly. On the other hand, the size of the wave packet for the 'speck of dust', and also for larger and heavier particles at rest, remains practically constant—the wave packet of appears as a 'frozen' blob of quantum matter.

What happens to the wave packet when we switch on interactions of our quantum particle with other particles? P. Ehrenfest considered relatively simple case when the interaction is described by a smooth potential $V(\mathbf{x})$ of a fixed form (thus he neglected backreaction of the particle on the other particles). He proved the theorem which quite often is summarized by saying that in such circumstances the wave packet moves in the space along a trajectory $\mathbf{x}(\mathbf{t})$ which obeys Newton's equation of motion

$$\ddot{\mathbf{x}}(t) = -\nabla\, V(\mathbf{x}(\mathbf{t})). \tag{1}$$

Strictly speaking, the actual content of the theorem is a bit weaker. Nevertheless, classical equations of the form (1) can be obtained from the theorem after some further steps.

Our notation is as follows. The dot denotes the derivative with respect to the time t. The boldface denotes three-dimensional vectors, for example $\ddot{\mathbf{x}} = (\ddot{x}^1, \ddot{x}^2, \ddot{x}^3)$, where x^1, x^2, x^3 are Cartesian coordinates in the space, and $\mathbf{x} = (x^1, x^2, x^3)$. ∇ is the vector composed of derivatives with respect to the coordinates, i.e., $\nabla = (\partial_1, \partial_2, \partial_3)$, where $\partial_1 = \partial/\partial x^1$, etc., and $\nabla\, V = (\partial_1 V, \partial_2 V, \partial_3 V)$. Summation over repeated indices is understood irrespectively of the level of indices. $\mathbf{ab} = a^i b^i$ denotes the scalar product of the three-dimensional vectors $\mathbf{a} = (a^1, a^2, a^3)$ and $\mathbf{b} = (b^1, b^2, b^3)$.

3. The original form of Ehrenfest's theorem

The seminal paper (Ehrenfest, 1927) is entitled "Bemerkung über die angenäherte Gültigkeit der klassischen Mechanik innerhalb der Quantenmechanik". It counts merely two and half pages including the title, abstract and references. In its first half the Schrödinger equation for the wave function Ψ is quoted [4],

$$-\frac{\hbar^2}{2m}\frac{\partial^2\Psi}{\partial x^2} + V(x)\Psi = i\hbar\frac{\partial\Psi}{\partial t},$$

as well as its complex conjugation. Next it is stated that these equations imply the following relations

$$\frac{dQ}{dt} = \frac{1}{m}P, \quad m\frac{d^2Q}{dt^2} = \frac{dP}{dt} = \int dx\ \Psi\Psi^*(-\frac{\partial V}{\partial x}), \tag{2}$$

where

$$Q(t) \equiv \int dx\ x\Psi\Psi^*, \quad \text{and} \quad P(t) \equiv i\hbar\int dx\ \Psi\frac{\partial\Psi^*}{\partial x}.$$

Details of the derivation are omitted, except for the remark that the second relation in formulas (2) is obtained with the help of integration by parts. P. Ehrenfest assumes that the spatial extension of the wave packet is small compared with macroscopic distances (nota bene, he uses the name 'wave packet' for the product $\Psi\Psi^*$).

Commenting on his results, P. Ehrenfest underlines similarity of the second relation in (2) to Newton's equation of classical mechanics. He is satisfied with such qualitative correspondence, and does

[4] In the present paragraph I copy the original notation from (Ehrenfest, 1927) in which no special symbol is used for the three dimensional vectors. The Abstract in (Ehrenfest, 1927) clearly indicates that the three dimensional case is considered. In particular, $\partial/\partial x$ above should be identified with ∇, and $\partial^2/\partial x^2$ with the Laplacian Δ.

not attempt to make it more precise. In particular, he does not even mention the approximation

$$\int dx\ \Psi\Psi^*(-\frac{\partial V}{\partial x}) \approx -\frac{\partial V(Q)}{\partial Q},$$

probably because he knew that it would be a hard task to formulate it in a rigorous manner. In fact, this approximation is the subject of numerous nontrivial investigations till nowadays. Only with this approximation the second relation (2) turns into Newton's equation (1) if we identify $Q(t)$ with $\mathbf{x}(t)$.

The second part of the paper has the subtitle 'Bemerkungen'. It is devoted to the one dimensional Gaussian wave packet for a free particle ($V = 0$). Its explicit form is presented, and the spreading out discussed. The paper ends with the observation that in the case of a very heavy particle the Gaussian wave packet expands very slowly, while for proton very rapidly.

Paper (Ehrenfest, 1927) is very important, indeed, for at least two reasons. First, P. Ehrenfest has shown that quantum mechanics does not contradict classical mechanics, but rather generalizes it—the latter can be regarded as a very good approximation to the former for a large class of physical phenomena. Second, he pioneered derivations of various kinds of classical equations of motion from underlying quantum mechanical models. Two important examples of this kind are outlined below.

4. Lorentz covariant formulation of the Ehrenfest's method

There is a problem with Lorentz covariance in the Ehrenfest approach to classical limit: because the standard expectation values do not have clear relativistic transformation law, the classical mechanics derived from Lorentz covariant quantum mechanics based on, e.g., the Dirac equation, is not covariant. Hence, it can hardly be accepted as the correct classical limit. This problem is explicitely pointed out in (Hilgevoord and Wouthuysen, 1963).

It turns out that there exists a modification of the Ehrenfest method which yields Lorentz covariant result (Aródź and Ruijgrok, 1988). Below we give a description of the results. Our main point here is that there is no single classical mechanics that follows from the underlying quantum theory. Instead, we obtain an infinite sequence of classical theories, which approximate the quantum theory with better and better accuracy and, unfortunately, with a complexity rapidly increasing to the level that renders such classical theories impractical.

This paragraph contains certain technical details given here for the readers interested in the theoretical physics aspects of the work (Aródź and Ruijgrok, 1988)—it can be omitted by not interested ones. In the improved approach, we start from a new definition of expectation values, which respects the Lorentz covariance. In this definition, the integral over the three Cartesian coordinates x^1, x^2, x^3 is replaced by an integral over three new spatial coordinates in a special coordinate system in the Minkowski space-time. In this system, the time axis is replaced by a time-like line $X^\mu(s)$ in the Minkowski space-time. This line will ultimately coincide with the classical trajectory associated with the wave packet. The three new spatial coordinates parameterize the directions perpendicular to this line (in the Minkowski sense). The Cartesian time coordinate t is replaced by the proper time coordinate s on that line. Next, the Dirac equation is transformed to these new coordinates. The evolution parameter is not the laboratory time t, but the proper time s. There are certain consistency conditions for the new expectation values which result from the requirement that the line $X^\mu(s)$ stays close to the wave packet, which evolves according to the Dirac equation. The explicit form of the wave packet is not needed. The consistency conditions imply the classical equations of motion for $X^\mu(s)$, and for other quantities like spin. Their form is approximate one in the sense that all terms proportional to $1/m^2$ or to higher powers of $1/m$ have been neglected. This is justified because m is assumed to have a large value. We use the Foldy-Wouthuysen transformation.

The starting point—the Dirac equation for a single electron—has the form:

$$\gamma^\mu \left(\frac{\partial}{\partial x^\mu} + iA_\mu \right) \psi + im\,\psi = 0.$$

It replaces the Schrödinger equation considered by P. Ehrenfest. $A_\mu(x)$ in the Dirac equation denotes the so called four-potential of electromagnetic field. It encodes information about electric and magnetic fields in which the electron moves. By assumption, it does not include the field generated by the considered electron. Furthermore, we assume that the mass m is large, in accordance with the discussion of spreading out of wave packets in Section 2. For convenience, we use the notation in which the Planck constant $\hbar$ and the velocity of light in vacuum c are not visible—as if $c = \hbar = 1$ (the notation commonly referred to as 'the natural units'). We also assume that the particle has unit electric charge. Summation over repeated indices is understood. We use the standard relativistic four dimensional notation as explained in, e.g., (Landau and Lifshitz, 1971, chap.1–2).

The modified Ehrenfest method yields the classical equations of motion which read:

$$m\ddot{X}_\mu = F_{\mu\nu}\dot{X}^\nu + \frac{1}{2m}\epsilon_{\nu\lambda\sigma\alpha}\dot{X}^\lambda(\delta^\beta_\mu - \dot{X}^\beta\dot{X}_\mu)\,W^\sigma\,\partial_\beta F^{\alpha\nu}$$
$$+\frac{1}{2m}(\delta^\sigma_\mu - \dot{X}^\sigma\dot{X}_\mu)\,C^{\rho\nu}\,\partial_\rho F_{\nu\sigma} + \frac{1}{2m}\dot{X}^\rho\dot{X}_\nu\,C_{\mu\sigma}\,\partial_\rho F^{\nu\sigma}, \quad (3)$$

$$
\begin{aligned}
\frac{dW^\lambda}{ds} &= -\dot{X}^\lambda \ddot{X}_\mu W^\mu + \frac{1}{m}(\delta^\lambda_\mu - \dot{X}^\lambda \dot{X}_\mu)\, W_\nu \, F^{\mu\nu} \\
&\quad + \frac{1}{m}(\delta^\lambda_\mu - \dot{X}^\lambda \dot{X}_\mu)\, Z_{\sigma\rho}\, F^{\mu\sigma,\rho} + \frac{1}{m}(\ddot{X}^\lambda P^\nu_\nu + \ddot{X}_\nu P^{\nu\lambda}). \quad (4)
\end{aligned}
$$

Technical details again: the dot denotes the derivative d/ds, where s is the proper time along the classical trajectory $X^\mu(s)$. The proper time replaces the time t present in Eqs. (2). Furthermore, ∂_μ stands for the partial derivative $\partial/\partial x^\mu$, and $F_{\mu\nu} = \partial_\mu A_\nu - \partial_\nu A_\mu$ is the electromagnetic field strength tensor. It is composed of the electric and magnetic fields. $\epsilon_{\nu\lambda\sigma\alpha}$ (the so called totally antisymmetric symbol) is equal to $0, 1, -1$ depending on the values of the Greek indices, for instance, $\epsilon_{0123} = 1$. The spin four-vector W^σ is related to the expectation value of the quantum spin operator. In the particular case of constant electric and magnetic fields, equations (3) and (4) reduce to the well-known Bargmann-Michel-Telegdi equations for a relativistic particle with spin.

The relativistic classical equation of motion for a point-like particle with the unit electric charge ($e = 1$) in the external electromagnetic field that is usually given in textbooks has the form:

$$
m\ddot{X}_\mu = F_{\mu\nu}\dot{X}^\nu. \quad (5)
$$

It precedes the quantum mechanics and also the concept of spin. We see that it is a small part of equation (3) above. Moreover, equation (5) does not take into account the spin of the particle, which in equations (3), (4) is represented by W^μ. In many important tasks, for example, in calculations of trajectories of electrons or protons in accelerators, one has to use equations which take into account the spin in order to achieve the desired accuracy—equation (5) is not good enough. In practice, certain simplified version of equations obtained with the Ehrenfest method is used. Such nontrivial and successful applications corroborate the correctness of the attitude that classical equations of motion should be derived from underlying quantum theory. On the other hand, there are many problems in which the spin is not important. In such cases the old equation (5) gives very good predictions for trajectories of the particle.

The classical variables $C^{\rho\nu}(s),\ \ Z_{\rho\sigma}(s),\ \ P^{\nu\lambda}(s)$ are related to entanglement of quantum observables: position with momentum, position with spin, and momentum with spin, respectively (Aródź and Ruijgrok, 1988). In principle, also equations of motion for $C^{\rho\nu}(s),\ Z_{\rho\sigma}(s)$ and $P^{\nu\lambda}(s)$ are needed for the mathematical completeness of the system of equations. They can be obtained with the help of the (modified) Ehrenfest method, but in practice one usually eliminates these variables by making certain simplifying assumptions. For example, in most situations all terms in the second line of equation (3), and also in the second line of (4), can be omitted. Then we do not need equations of motion for $C^{\rho\nu}(s),\ Z_{\rho\sigma}(s),\ P^{\nu\lambda}(s)$. If the equations of motion for these classical variables were included, one would get even more accurate classical approximation to the quantum mechanics, but at the price of having to deal with a much larger set of equations.

5. Classical mechanics of a point-like particle with spin and color

This example of derivation of classical mechanics is interesting because prior to the pertinent quantum theory such a classical theory had not been known at all. Once derived, it has turned out to be a useful tool for theoretical investigations of quark matter. Quarks have special charges, called color and weak izospin, which make them sensitive to the so called non-Abelian gauge fields. Both the non-Abelian gauge fields and the quarks as constituents of the material world were discovered in 1960's and 1970's. Certain particular version of the non-Abelian gauge field is called the Yang-Mills field. Below we outline the basic features of the classical limit for a quantum particle that interacts with the Yang-Mills field. The resulting classical theory describes motion of a point particle, known as the particle with color or izospin, in certain fixed Yang-Mills field.

Historically, the first attempt to derive classical equations of motion for a point particle interacting with the Yang-Mills field was made by S.K. Wong (1970). Classical state of this particle at given time t is represented jointly by: the so called classical izospin vector $I^a(t)$, where the index a takes values 1, 2 and 3; the position $\mathbf{x}(t)$; and the velocity $\dot{\mathbf{x}}(t)$. The derivation given by Wong does not use the Ehrenfest method. For that matter, it should rather be described as an educated guess based on symmetry principles and algebraic structure of the Dirac equation. In consequence, his equations respect the Lorentz invariance as well as the so called gauge invariance, but they miss the spin of the particle and certain less obvious classical variable, as explained below. We will not present here these equations in order to avoid overloading this article with technical details. Interested reader may consult the original paper by Wong or (Arodź, 1982).

More systematic derivation is based on the Ehrenfest method (Arodź, 1982). We consider expectation values of the following quantum observables: the position $\hat{\mathbf{x}}$, the so called kinetic momentum $\hat{\mathbf{p}} - \mathbf{A}^a\hat{T}^a$, the spin $\hat{\mathbf{S}} = (\hat{S}^i)$, the izospin $\hat{\vec{T}} = (\hat{T}^a)$, and the product of the spin and izospin operators $\hat{J}^{ai} = \hat{T}^a\hat{S}^i$. The hat ˆ means that these objects are operators in pertinent Hilbert space. The indices a, i

take values 1,2, and 3. The three vectors $\mathbf{A}^a$ represent the Yang-Mills field. They are counterparts of the electromagnetic vector potential $\mathbf{A}$, which is a part of the four-potential A_μ introduced in the previous section, $(A_\mu) = (A_0,\ \mathbf{A})$.

Expectation values of these observables become the classical dynamical variables. Furthermore, the pertinent quantum evolution equation yields the counterpart of the Newton equation and a few other equations. In this manner we obtain the classical mechanics with the following classical variables that characterize the point particle with izospin: the position $\mathbf{x}(t)$, the velocity $\dot{\mathbf{x}}(t)$, the classical izospin $I^a(t)$, the classical spin vector $\mathbf{S}(t)$, and a novel classical variable $J^{ai}(t)$.

The novel dynamical variable $J^{ai}(t)$ is the expectation value of the operator $\hat{J}^{ai}$. It can be regarded as the three vectors $\mathbf{J}^a(t)$, $a = 1, 2, 3$, with their components enumerated by the index i. In spite of the fact that the operator $\hat{J}^{ai}$ is the product of operators $\hat{T}^a$ and $\hat{S}^i$, its expectation value does not have to be equal to the product $I^a(t)S^i(t)$, because in general expectation value of product of operators is not equal to product of expectation values of the operators.

The Ehrenfest method not only reveals the new classical variable—it also shows that there are relations, traditionally called constraints, between the classical variables, which reflect the fact that the classical variables are defined as the expectation values in the same quantum state $\psi(\mathbf{x}, t)$. These constraints have the following form

$$4J^{ia}S^i = I^a, \quad 4J^{ia}J^{ib} = (\frac{1}{4} - \mathbf{S}^2)\,\delta_{ab} + I^aI^b.$$

To summarize, applying the Ehrenfest method we have discovered that Wong's equations of motion for the classical point particle with izospin are rather oversimplified version of the more adequate equations. In particular, we have found the new classical variable $J^{ai}(t)$, which appears because the particle possesses both spin and izospin.

6. Conclusion and remarks

1. Ehrenfest's theorem and its generalizations show that classical mechanics of particles can be reinterpreted in terms of expectation values, with pertinent quantum states being the wave packets. In this way, the relation between classical and quantum mechanics, viewed as the relation between old and new theories, acquires the perfect form: the new theory is more general and more accurate, and it rather encompasses the old one instead of contradicting it in all respects. Furthermore, the method used by Ehrenfest—the emphasis on properties and evolution of expectation values—has turned out to be very fruitful as the tool for improving existing classical theories. In Section 4 we have seen such improvement in the case of classical particle in electromagnetic field. The method can also provide completely new classical mechanics, unknown prior to quantum theory, as discussed in section 5 on the example of particle with spin and izospin.

2. The enormous success of the Ehrenfest method suggests that perhaps no part of the material world is purely classical, that quantum mechanics embraces all physical phenomena,[5] and that the classical world is fictitious in the sense that it exists only as certain theoretical approximation to the real world.[6] Such assumption of absolute quan-

[5] With possible exception for gravitational phenomena. So far there is no experimental evidence for quantum nature of gravitation.

[6] Here we touch the philosophical problem to what extent it really does not exist. Interesting philosophical analysis of a related problem can be found in (Heller, 2018).

tumness of the seemingly classical macroscopic world leads to the following question: why we do not see in nature isolated macroscopic bodies in typical quantum states such as, e.g., wave packets spatially extended over sizable distances (in literature dubbed 'Schrödinger's cats'). To explain their absence, one can propose a new theory which deviates from quantum mechanics in the macroscopic world, and essentially coincides with it in the micro-world. The recently popular Continuous Spontaneous Localization theory (Ghirardi, Pearle and Rimini, 1990) is of this kind. One should also mention the decoherence phenomenon (Zeh, 1970; Zurek, 2003), in which states of a quantum system are very quickly transformed into the so called mixed states, due to strong interactions with environment. Here the absence of widely extended wave packets of macroscopic particles is explained by the presence of interactions with an environment. Which mixed state ('pointer state') appears at the end of the process of decoherence of a concrete wave packet still is the matter of many investigations. It is a difficult problem, and there are many related hypotheses, some with picturesque names, e.g., 'quantum Darwinism' (Zurek, 2009). The decoherence phenomenon belongs to the realm of effective quantum mechanics in continua, mentioned in the Introduction.

The author prefers another viewpoint: we think that one can provide an explanation for the apparent absence of quantum phenomena in the macroscopic world using the standard quantum mechanics. An interesting possibility is that such extended quantum states of heavy isolated particles are possible in principle, but that they are hardly achievable in reality. The main difficulty is that a spatially extended state has to be produced as such, because wave packets of very heavy particles practically do not expand. This can be rather difficult task. For illustration, let us consider the following thought experiment. Suppose that we can produce a kind of hydrogen-like 'atom' in which the electron is replaced by a heavy (in comparison with electron) particle of the mass $M = 10^{-6}g$, and the proton with an even more massive particle. Next, let us excite it in order to increase its spatial size. Highly excited states close to ionization threshold have a macroscopic size—there is no theoretical upper bound on the size of excited atoms. Finally, we ionize that 'atom'—this would provide the heavy particle ('electron') in an extended quantum state of the size of the 'atom'. The trouble is that the energy needed for the ionization is of the order 10^{13} GeV, as a simple calculation shows, while the highest achievable at present energies of particles are of the order 10^4 GeV only.

Another thought experiment involves quantum harmonic oscillator. This system is ubiquitous in physics—it arises as a very good approximation to many complex systems. Classical harmonic oscillator consists of a particle of mass M subject to a force which increases proportionally to the distance from a fixed point, called the center, to the particle. The strength of the force is characterized by a constant k. Quantum theory of such object predicts that the least energy state has the form of a wave packet of the size $l = \sqrt{2\hbar/\sqrt{Mk}}$. Now, let us take the particle roughly of the size of a droplet of water from a fog. Its radius is $r = 10^{-1}cm$ and the mass $M = 10^{-2}g$. We are interested in situations such that l is much larger than r—then the wave packet will be much larger than the classical radius of the particle. Simple calculation shows that the constant k has to be exceedingly small, namely $k \ll 4 \cdot 10^{-48}g/s^2$. Sizable force appears only when the distance from the center is of the order $10^{40}cm$. Let us recall that the light year counts about $10^{18}cm$. Construction of such a feeble harmonic oscillator is far beyond the present day engineering. On the other hand, if we take a more realistic value $k = 1\ g/s^2$, the condition $l \gg r$ is satisfied only if $M \ll 4 \cdot 10^{-50} g$—the mass incomparably smaller than the mass of electron. Such particle certainly is not macroscopic.

3. Let us return to the question from section 2: what is the best intuitive picture of elementary quantum particle. Such a picture can be very helpful if it is adequate, or very misleading when wrong. In our opinion, many mysteries, controversies, and so called paradoxes that are discussed in literature on quantum mechanics arise in a large part from inadequate images of the quantum particle. As we have written in section 2, we prefer to regard the quantum particle as a cloud of quantum matter. Its main feature is that it can be created or annihilated as a whole—it is impossible to have one half of electron. Notwithstanding our views, we admit that there exist other pictures as well. It seems that the most popular one is that actually there exists exactly point-like material particle which has a concrete position in space at each time, but we do not know that position. What is known is merely the probability of finding this point-like particle in a chosen volume of the space. It is calculated as the integral of the modulus squared of the wave function over that volume. We think that by adopting such image of the quantum particle one simply carries over to quantum mechanics the picture from classical mechanics.[7] This can not be justified, especially if we regard classical mechanics of point-like particles as a secondary theory which is derived from quantum mechanics. Therefore we should base our intuitions solely on the Schrödinger equation, and on the actual mathematical representation of the states of the particle as wave functions, forgetting completely about the classical mechanics.

[7] This is precisely what was done in the prequantum theory of atoms with the Bohr-Sommerfeld rules. This theory is in fact classical one. The Bohr-Sommefeld rules serve only as a tool for selecting particular classical trajectories.

The picture of point-like quantum particle with concrete yet unknown location in the space may be motivated also by unjustified enhancement of the probabilistic interpretation of quantum mechanics. It is known for sure from numerous experiments that outcomes of measurements are distributed with certain probability, which can be calculated with the help of quantum mechanics if we assume the so called Born rule. The point is that there is no experimental evidence for the probabilistic character of quantum mechanics without invoking an experiment. Thus, we may suppose that it is a specific coupling between the two systems: the quantum particle and a very special physical macroscopic apparatus—the measuring apparatus—that is responsible for the probabilistic nature of outcomes of experiments. We adhere precisely to this view.

To summarize, we prefer the picture of elementary particle as a cloud of quantum matter. The probabilistic outcomes of measurements are due to interaction of the particle with a macroscopic measuring apparatus. For us, such views are quite natural corollaries to Ehrenfest's theorem.

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Topology and models of ZFC at early Universe

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Abstract

Recently the cosmological evolution of the universe has been considered where 3-dimensional spatial topology undergone drastic changes. The process can explain, among others, the observed smallness of the neutrino masses and the speed of inflation. However, the entire evolution is perfectly smooth from 4-dimensional point of view. Thus the raison d'être for such topology changes is the existence of certain non-standard 4-smoothness on $\mathbb{R}^4$ already at very early stages of the universe. We show that the existence of such smoothness can be understood as a byproduct of the quantumness of the origins of the universe. Our analysis is based on certain formal aspects of the quantum mechanical lattice of projections of infinite dimensional Hilbert spaces where formalization reaches the level of models of axiomatic set theory.

Keywords

Cosmological model, Exotic R^4 and $S^3 \times \mathbb{R}$ in cosmology, 4-exotic smoothness, Models of ZFC, Topological model for inflation, Topological model for neutrino masses, Forcing, QM lattice of projections, tions.

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1. Introduction

There exist many free parameters of physics which can be determined experimentally though fundamental theoretic derivation of them is still missing. Moreover, knowing such derivation presumably will lead to the fundamental revolution in physics which would rely both on the extension of the standard model of particles (e.g. Weinberg, 2018) and understanding gravity at quantum regime through cosmological data (e.g. Woodard, 2014). Among the parameters in question there are masses of elementary particles, mixing angles, coupling constants and in cosmology the value of the cosmological constant, the speed of inflation or the α parameter in the Starobinsky model. For example we have experimental bounds on the neutrinos masses from PLANCK, Baryon Acoustic Oscillations (BAO) and KamLAND-Zen (Majorana neutrino) experiments (Gando et al., 2016; Collaboration et al., 2016; Harnois-Déraps et al., 2015). The smallest experimental bound for the sum of the three neutrino masses reads

$$\sum_i m_{\nu_i} \leq 0,12eV\,.$$

A way how these bounds were obtained indicate strongly that successful predicting true values for the masses should deal with cosmology. That is why the following question is in order.

> Q1: Do we know any candidate for the model of cosmological evolution which would help determining theoretically the realistic (bound for) neutrino masses?

We do know the seesaw mechanism of generating small neutrino masses, however, the derivation deals with two energy scales as free parameters which are not, however, fundamentally fixed. The Q1 has been answered in affirmative in the series of papers (Asselmeyer-Maluga and Król, 2018; 2014; 2019) where suitable cosmological model has been constructed. Hence the immediate additional problem emerges:

> Q2: Does the model of Q1 predicts realistic values of some inflationary parameters, like a speed of inflation?

The affirmative answer for Q2 has been indeed recently given. The model is based on smooth differential structure on $\mathbb{R}^4$ which is not standard, i.e. is not any smooth product $\mathbb{R} \times \mathbb{R}^3$. We call such structure exotic smooth structure and $\mathbb{R}^4$ with it an exotic R^4. There exist infinitely continuum many such different, pairwise nondiffeomorphic, exotic R^4's. Thus mathematics favours dimension 4 in this respect, i.e. any other $\mathbb{R}^n$ for any $n \neq 4$ carries *unique* standard smooth structure. The physics of the proposed cosmological model distinguishes one of the structure, namely this which embeds in, also exotic, K3 surface, i.e.

$$R^4 \hookrightarrow K3 \oplus \overline{\mathrm{CP}}^2 \,.$$

> Q3: K3 is compact, does it have any physical, probably cosmological, meaning which would extend the embedded non-compact R^4 representing spacetime?

After presenting some details of the model we will present certain speculative ideas regarding this issue.

In the second part of the paper we will deal with quantum origins of the smoothness required by the model. Provided, the initial state of the Universe is quantum mechanical, i.e. formulated as usual by a Hilbert space of states, we will analyze the following problem:

> Q4: Does the QM formalism know that the universe at large scales is smooth, 4-dimensional and exotic?

Similar question has been recently addressed in (Król, Asselmeyer-Maluga et al., 2017; Król, Klimasara et al., 2017). More thorough analysis will be given elsewhere. We show that the smoothness on $\mathbb{R}^4$ which agrees with QM formalism, has to be exotic. This result strongly supports the proposed model. Questions Q1 and Q2 along with the details of the model will be presented in the next section. The results and arguments concerning Q4 will appear in the subsequent section. We close the paper with the discussion which covers also Q3.

2. The smooth cosmological model for inflation and neutrino masses

It is quite successful scenario to apply the Friedman-Robertson-Walker (FRW) cosmology for describing large scale universe with its homogeneous and isotropic structure. The time-like slices define 3-geometries which, in the case of the closed universe, are 3-spheres S^3 ($k = +1$) giving rise to the model based on $S^3 \times \mathbb{R}$. Since 1979 seminal work by M. H. Freedman (1979) mathematicians have become aware of the existence and basic constructions of smooth manifolds which all are topologically $S^3 \times \mathbb{R}$, however, smoothly they are nondiffeomorphic each to the other and to $S^3 \times \mathbb{R}$. Soon after in 1980s mathematicians again found that similar open 4-manifolds exist also for $\mathbb{R}^4$—exotic R^4's. They all are homeomorphic to $\mathbb{R}^4$ being pairwise nondiffeomorphic. Moreover there are usually continuum infinite many nondiffeomorphic classes of exotic open 4-manifolds each being homeomorphic with a given open 4-manifold (see e.g. Gompf and Stipsicz, 1999). This and the existence of exotic R^4 make the dimension 4 completely distinguished in mathematics unlike the other dimensions where the usual tools of differential geometry and topology work well. Many new techniques have been found and developed within the recent years to understand and explore the phenomenon of exotic 4-smoothness. Among which Casson handles, Akbuluth corks, tools of hyperbolic geometry, handle calculus and many others have become an everyday toolkit of researchers in the field. We do not know any way how to avoid the constructions and replace them by known techniques from other dimensions. That is why it is not that surprise that the variety of methods are being to be applied in order to recognize physical applications of 4-exotic smooth structures. Two aspects are particularly promising—the dimension 4 is also distinguished by physics and several parameters in cosmology and particle physics call for their fundamental derivation and explanation.

In a way of searching for such explanation we have recently proposed the cosmological model based on exotic $S^3 \times \mathbb{R}$, i.e. $S^3 \times_\Theta \mathbb{R}$ (Asselmeyer-Maluga and Król, 2018; 2014; 2019). Such smooth open 4-manifolds (infinite continuum many of them) can be seen as submanifolds (exotic ends) of exotic R^4's

$$S^3 \times_\Theta \mathbb{R} \subset R^4 \,.$$

Here Θ refers to certain homology 3-sphere smoothly embedded in exotic $S^3 \times_\Theta \mathbb{R}$ and allows for distinguishing between these exotic smooth manifolds. When $\Theta = S^3$ the product is globally smooth and the 4-manifold becomes the unique standard smooth $S^3 \times \mathbb{R}$. So the base for the cosmological model is to refer to exotic smooth $S^3 \times_\Theta \mathbb{R}$ rather than to the standard smooth $S^3 \times \mathbb{R}$.

Exotic $S^3 \times_\Theta \mathbb{R}$ is a smooth 4-manifold, so the cosmic evolution seen from dimension 4 can be considered perfectly smooth. However, the 3-dimensional slices undergoes drastic topology changes

$$S^3 \xrightarrow{1.} \Sigma(2,5,7) \xrightarrow{2.} P\#P\,. \qquad (1)$$

which will determine the values of certain physical parameters like neutrino masses.

The 3-dimensional evolution within the standard $S^3 \times \mathbb{R}$ is trivially $S^3 \to S^3 \to S^3$.

Here $\Sigma(2,5,7)$ is the Brieskorn homology 3-sphere and $P\#P$ is the connected sum of two copies of the Poincaré 3-sphere (Asselmeyer-Maluga and Król, 2018; 2019). To pinpoint physics into the model we are taking the radius of S^3 in (1) to be of order of the Planck length and natural energy of that epoch to be Planck energy. Such choice is natural since the evolution of the universe should start with the quantum Planck era. The topology change 1. is the 4-dimensional cobordism $W(S^3, \Sigma(2,5,7))$ between 3-sphere and the Brieskorn sphere. To make it smooth we need to glue a Casson handle. A Casson handle (Ch) is the infinite geometric construction which becomes main player in investigating of 4-exotic smoothness. As shown by Freedman every Ch is topologically the ordinary 2-handle, $D^2 \times \mathbb{R}^2$, with the attaching region $S^1 \times \mathbb{R}^2$ while smoothly there are infinitely many of different exotic Ch's (e.g. Gompf and Stipsicz, 1999). The infinite geometric construction present within any Ch is naturally grouped into the layers indexed by $n \in \mathbb{N}$ and each layer corresponds to the level of a labeled tree defining Ch. Each level n contributes topologically to the change of the length scale by the expression $\sim \frac{\theta^n}{n!}$. θ is the function of purely topological invariant of the 3-manifold $\Sigma(2,5,7)$, i.e. $\theta = \frac{3}{2 \cdot CS(\Sigma(2,5,7))}$ where in the denominator stands Chern-Simons invariant of the Brieskorn homology sphere.

The contribution of the entire infinite Casson handle is thus given by (Asselmeyer-Maluga and Król, 2018; 2014; 2019)

$$a = a_0 \sum_{n=0}^{\infty} \frac{\theta^n}{n!} \,. \tag{2}$$

To determine energy scale of the first topology change 1. in (1) relies on finding the minimal segment of Ch which has to be develop in order to make the cobordism $W(S^3, \Sigma(2,5,7))$ smooth in $S^3 \times_\Theta \mathbb{R} \subset R^4$. As argued in (Asselmeyer-Maluga and Król, 2014; 2019) on the base of Freedman result there are needed 3-stages of Ch: Every Ch is embeddable in its first 3-stages. Thus the shortest possible time change, coordinatized by the levels n, reads (Asselmeyer-Maluga and Król, 2014; 2019)

$$\Delta t = \left(1 + \theta + \frac{\theta^2}{2} + \frac{\theta^3}{6}\right) \cdot t_{\text{Planck}}$$

which is the shortest change lowering the Planck energy. This gives rise to the first, below Planck, energy scale

$$E_{\text{GUT}} = \frac{E_{\text{Planck}}}{1 + \theta + \frac{\theta^2}{2} + \frac{\theta^3}{6}} \,.$$

Calculating θ as a function of Chern-Simons invariant, $\theta = \frac{140}{3}$, gives rise to the GUT scale, i.e. $E_{\text{GUT}} \simeq 0,67 \cdot 10^{15}$ GeV. But this time it is purely topologically determine energy scale (up to the initial Planck energy).

Let us turn to the second topology transition in (1), i.e. $\Sigma(2,5,7) \to P\#P$. To make it smooth we need to glue in three Ch's. This is due to the topological decomposition of the boundary of $E8 \oplus E8$ as K3 surface $E(2)$ (Asselmeyer-Maluga and Król, 2018; 2019). Thus the second topology change follows from the embedding of exotic R^4 into $E(2)\#\overline{CP^2}$. Taking into account the entire infinite stages of these Ch's, and relating the result to the initial Planck energy, gives rise to the result

$$E_2 = \frac{E_{\text{Planck}} \cdot \exp\left(-\frac{1}{2 \cdot CS(P\#P)}\right)}{1 + \theta + \frac{\theta^2}{2} + \frac{\theta^3}{6}}.$$

This energy $E_2 \simeq 63\,\mathrm{GeV}$ is of the order of the elektoweak energy scale (or the half of the Higgs mass). Again, the result is supported topologically and the exotic smoothness of R^4 is the main reason for this support.

Given the two topological energy scales we are using them to evaluate the neutrino masses. Applying the simplest seesaw mechanism we are taking the mass matrix

$$\begin{pmatrix} 0 & M \\ M & B \end{pmatrix}$$

with energy scales $M \sim E\mathrm{GUT} \simeq 0,67 \cdot 10^{15}\,\mathrm{GeV}$ and $B \sim E_2 \simeq 63\,\mathrm{GeV}$ introduced. Then the mechanism relies on calculating the eigenvalues which read

$$\lambda_1 \approx B \qquad \lambda_2 \approx -\frac{M^2}{B},$$

thus the neutrino mass is predicted as

$$m = \lambda_2 \simeq 0,006\,\mathrm{eV}.$$

The value agrees with the current experimental upper-bounds (e.g. Collaboration et al., 2016; Harnois-Déraps et al., 2015). It is, however, the value protected by the topology of the unverse which underlies certain nonstandard (exotic) smooth differentiable structure of R^4.

It is quite interesting that the formula (2) allows for determining the e-folds number N for the inflation in such topological model (Asselmeyer-Maluga and Król, 2014; 2019). Namely

$$N = \frac{3}{2 \cdot CS(\Sigma(2,5,7))} \simeq 47$$

which is experimentally acceptable and, as the model explains, it is again topologically supported value.

3. Very early universe and the evolution of the models of ZFC

We have seen that exotic smoothness on $S^3 \times \mathbb{R}$ replacing the standard one has tremendous impact on understanding of the cosmological evolution of the universe and leads to determining neutrino masses along with GUT and electroweak energy scales. All this works under initial conditions settled as quantum Planck energy scale and the Planck length radius of S^3 so that the question Q4 from the Introduction can be addressed. Namely, does QM formalism determine the large scale smoothness of the evolving universe? Is this smoothness in dimension 4 indeed exotic or rather standard? If the answers support indeed exotic geometry that would be a strong indication in favour of the entire model. The key tool to attack this problem is *formalization*: one goes back to a formal level where set theoretic constructions of QM and differential geometry become important. Especially instead of working in undetermined formally universal space one starts working in specific models of Zermelo-Fränkel set theory with the axiom of choice (ZFC). The models can vary along the evolution of the universe and their relations serve as additional physical degrees of freedom.

Let $\mathcal{H}$ be an infinite dimensional separable complex Hilbert space representing states of the quantum world at the Planck era. Infinite dimensionality is enforced by the need to refer to spacetime with momentum and position operators. Then let $\{\mathbf{L}, \wedge, \vee, \neg, \mathbf{0}, \mathbf{1}\}$ be the lattice of projections of $\mathcal{H}$. Local Boolean frames of the lattice are given by maximal complete Boolean algebras of projections B's. If $\dim \mathcal{H} = \infty$ then each such B is, in general, decomposed into the atomic, B_a, and atomless, B_c, parts (Kappos, 1969)

$$B = B_a \otimes B_c \,. \tag{3}$$

B_c is the atomless measure Boolean algebra, i.e. $B_c \simeq \mathrm{Bor}([0,1])/\mathcal{N}$ where $\mathrm{Bor}([0,1])$ is the Borel algebra of subsets of $[0,1] \subset \mathbb{R}$ and $\mathcal{N}$ ideal of Lebesgue measure zero subsets of $[0,1]$. B_c is homogenous, i.e. $B_c \overset{\text{iso}}{\simeq} B_c(p)$ for every $p > 0$ where $B_c(p)$ is the algebra of all $q \leq p$ with the unit p. It follows that B_c is the universal algebra for all B's which means that

$$\forall_{B \subset \mathbf{L}}\ B \text{ is completely embeddable in } B_c \,.$$

Thus in what follows we will use a single symbol B for this atomless, complete, universal measure algebra, replacing the variety of B's in (3).

Let V be a transitive standard universe of set theory and V^B the Boolean-valued class of ZFC (e.g. Jech, 2003). Such transitive standard model V exists provided ZFC is consistent due to the Mostowski collapsing theorem (Jech, 2003). The construction of V^B is also well recognized and described in a variety of textbooks (see e.g. Jech, 2003; Bell, 2005). For us V^B is a Boolean-valued model which gathers together Boolean frames B's derived from $\mathbf{L}$. Maximal complete Boolean algebras B's determine maximal sets of commuting observables of QM based on $\mathcal{H}$ by virtue of the spectral theorem. Let

$\{A_i, i \in I\}$ be a set of commuting observables on $\mathcal{H}$. Maximality of such family is equivalent to the existence of a single self-adjoint operator A with the spectral measure μ_σ on $S(A)$—the Stone spectrum of the projection algebra B. To every set $\{A_i, i \in I\}$ as above there exists a Boolean algebra of projections B with the spectrum $S(A)$. The algebra B generates all observables in the family in the sense that the projections being the values of the spectral measures live in B. In the case B is maximal the family of observables is also maximal (complete) set of observables. Let $L^2(S(A), \mu_\sigma)$ be the Hilbert space of square μ_σ-integrable complex-valued functions on $S(A)$.

Lemma 1. (Boos, 1996) The following statements are equivalent:

i. There exists a unitary isomorphism $U : \mathcal{H} \to L^2(S(A), \mu_\sigma)$ such that
$$UAU^{-1}(\psi(x)) = x\psi(x)$$
is the self-adjoint position operator Q on $L^2(S(A), \mu_\sigma)$.
ii. A is maximal.
iii. B is complete and maximal.
iv. Every self-adjoint operator C on $\mathcal{H}$ commuting with B, fulfills $C = f(A)$ for some Borel function $f : S(A) \to \mathbb{R}$.

We see that there is the strict $1 : 1$ correspondence between frames of complete sets of self-adjoint operators and maximal Boolean algebras B's. Now let us assign the maximal operator A to every self-adjoint C on $\mathcal{H}$. Subsequently there corresponds to A the maximal complete Boolean algebra B. The important, though obvious, consequence is the following

Corollary 1. In QM one cannot reduce the resulting family of B's as above to the single-element family $\{B\}$.

The reason is that there exist noncommuting observables determining different maximal families which correspond to different maximal complete Boolean measure algebras B's. Otherwise the correspondence would not be $1:1$.

Now let us assign the family of copies of V^B's to B's according to the lemma above. Again the assignment is irreducible to a single model V^B even though the models V^B's are isomorphic. Each V^B is a Boolean-valued model of ZFC. To reduce it to a 2-valued model $V^{\{0,1\}}$ one should make use of certain homomorphisms

$$h_B : B \to \{0,1\}\,.$$

We want to preserve as much of the structure of B as possible since these algebras are local frames of QM. Each B is atomless complete maximal measure algebra. In particular given a subset $S \subset B$ there always exist maximum $\bigvee S \in B$. We say that h_B preserves completeness of B if for every family $S \subset B$ with $\bigvee S \in B$ (B is complete)

$$h_B(\bigvee S) = \bigvee\{h_B(a) : a \in S\}\,.$$

Moreover we want to preserve dense families in B. A subset $X \subset B$ is dense in B when

$$q \in X \wedge p \in B \wedge q \leq p \Rightarrow p \in X \tag{4}$$

$$\forall p \in B \exists q \in X (p \leq q)\,. \tag{5}$$

Particularly important families in B are generic ultrafilters. A subset $X \subset B$ is a filter on B when

$$q_1, q_2 \in X \Rightarrow \exists z \in X \text{ such that } q_1 \leq z \wedge q_2 \leq z \tag{6}$$

$$q \in X \wedge p \in B \wedge p \leq q \Rightarrow p \in X\,. \tag{7}$$

Definition 1. A generic ultrafilter on B is a filter $\mathcal{U} \subset B$ such that for any family $X \subset B$ dense in B

$$X \cap \mathcal{U} \neq \emptyset .$$

Then it holds

Lemma 2. (Solovay, 1970, p.35) Let $h_B : B \to \{0, 1\}$ be a complete homomorphism. Then

$$h_B^{-1}(1) = \mathcal{U}$$

is a generic ultrafilter on B.

Let V be the universe of sets as before and $\mathcal{U} \subset B$ a generic ultrafilter on B i.e. $\mathcal{U} \cap X \neq \emptyset$ for every dense subfamily X on B in V.

Lemma 3. (Jech, 1986, p.7) B is atomless iff $\mathcal{U} \notin V$.

From Lemmas 2 and 3 it follows

Lemma 4. In V: There does not exist any complete $h_B : B \to \{0, 1\}$ for the measure algebra B.

Proof. B is atomless so there does not exist any generic $\mathcal{U}$ in B in V. □

One way to overpass this no-go property is to relativize ZFC into models of ZFC or allow for changing the universe of sets. Let V be a standard transitive model of ZFC as above. We need 2 important conditions imposed on B defined in V. If B is a complete atomless Boolean algebra in V and P a dense partial order $P \subset B$ in V. Then we, following (Solovay, 1970), assume that

1. There exist only countably many subsets of P in V.

2. $h_B : B \to \{0,1\}$ is said to be V-complete if for every family $S \subseteq B$ living in V, i.e. $S \in V$, and $\bigvee S \in V$ then

$$h_B(\bigvee S) = \bigvee\{h_B(s) : s \in S\}.$$

3. A filter $\mathcal{U}$ on B is V-generic when $\mathcal{U}$ has nonempty intersection with every dense family in V.

Then one proves

Lemma 5. (Solovay, 1970, p.35) For every V-complete homomorphism $h_B : B \to \{0,1\}$ the set

$$\mathcal{U} = \{x : h_B(x) = 1\} \tag{8}$$

is an V-generic filter on B.

Conversely, for any V-generic filter $\mathcal{U}$ there exists unique h_B fulfilling (8).

Note that for B atomless still Lemma 3 forbids the existence of $\mathcal{U}$ in V so that

$$h_B \notin V \text{ and } \mathcal{U} \notin V\ .$$

However, due to the relativization of models of ZFC in models of ZFC we can now indicate the model V' extending the V where there live both h_B and $\mathcal{U}$. This is the random forcing extension of V.

Theorem 1. (Solovay, 1970, p.36) There is a canonical 1:1 correspondence between the reals random over V and V-complete homomorphisms of B, $h_B \to \{0,1\}$.

As we noted before the Boolean-valued models $\{V^B : B \in \mathcal{B}\}$ are isomorphic. On the other hand these models cannot be reduced to a single-element family V^B. Given the procedure above reducing B to $\{0,1\}$ we are faced with a family of trivially isomorphic algebras $\{0,1\}$ so that they are distinguished by different V-generic filters $\mathcal{U}$'s.

As the result we have a family of pairs $\{(V^{\{0,1\}}, \mathcal{U}_\alpha)_{\alpha\in I}\}$. There exist, however, corresponding reductions of V^B to 2-valued models as in the above family of pairs. It follows from Theorem 1.

Lemma 6. The family $\{(V^{\{0,1\}}, \mathcal{U}_\alpha)_{\alpha\in I}\}$ is given by the random forcing extensions $\{V[\mathcal{U}_\alpha], \alpha \in I\}$.

In this way we avoid just to duplicate isomorphic copies of $V^{\{0,1\}}$. Rather there are 2-valued forcing extensions $V[\mathcal{U}_\alpha]^{\{0,1\}} = V[\mathcal{U}_\alpha], \alpha \in I$ respecting 2-valued algebras and ultrafilters $\mathcal{U}_\alpha$. Now we can give the construction of a spacetime manifold M^4 via local coordinate frames supported by the models of ZFC. Let M^4 be a smooth 4-manifold with a smooth atlas $\{U_\alpha \simeq \mathbb{R}^4 : \alpha \in J\}$.

Definition 2. We call an atlas $\{U_\alpha \simeq \mathbb{R}^4 : \alpha \in J\}$ of M^4 **L**-supported if for every U_α there exists V-generic ultrafilter $\mathcal{U}_\alpha$ and the model $V[\mathcal{U}_\alpha]$ such that the formalisations of U_α read

$$U_\alpha \simeq R^4_{V[\mathcal{U}_\alpha]} . \tag{9}$$

If for every local QM frame $B = B_\alpha \in \mathcal{B}$ and its corresponding 2-valued forcing reduction $V[\mathcal{U}_\alpha]$ every smooth atlas of M^4 contains all formalisations as in (9), then we say that M^4 covers smoothly **L**.

Here $R_{V[\mathcal{U}_\alpha]}$ is the unique model of complete algebraically closed field of real numbers in the model $V[\mathcal{U}_\alpha]$.

Theorem 2. If every atlas of a smooth $\mathbb{R}^4$ covers smoothly **L** then such $\mathbb{R}^4$ cannot be the standard smooth $\mathbb{R}^4$.

Proof. The family $\{B_\alpha\}$ of QM frames is not any single-element family (see Corollary 1) so thus the families of $\{V^{B_\alpha}\}$ and its 2-valued reductions $\{V[\mathcal{U}_\alpha]\}$. From the smooth covering of **L** property as in Definition 2 every smooth atlas on $\mathbb{R}^4$ contains a family of $\{U_\alpha\}$

with the corresponding formalisations $\{U_\alpha \simeq R^4_{V[\mathcal{U}_\alpha]}\}$. Thus every smooth atlas cannot be a single-chart one. So we have smooth $\mathbb{R}^4$ whose none smooth atlas is single-chart. Now it is enough to note that any smooth $\mathbb{R}^4$ which is diffeomorphic to the standard $\mathbb{R}^4$ assumes 1-chart smooth atlas. Otherwise it would not be diffeomorphic to the standard $\mathbb{R}^4$. □

So to make agreement between smooth structure on $\mathbb{R}^4$ and QM lattice $\mathbf{L}$ such that $\mathbf{L}$ supports this structure requires referring to exotic R^4. The standard $\mathbb{R}^4$ cannot cover $\mathbf{L}$. If such agreement took place in the real evolution of the universe the phenomenon of changing models of set theory from V to the forcing extension $V[\mathcal{U}_\alpha]$ should also be a physical process. This more that as we saw in Sec. 2 certain exotic R^4 considered as input of the cosmological model allows for predicting the values of important physical parameters like GUT and electroweak energy scales and the neutrino masses.

4. Discussion

One disturbing feature of the presented model is that the exotic R^4 generating reliable values of physical parameters is determined by the embedding

$$R^4 \subset K3\#\overline{CP^2}\,.$$

What is a physical role ascribed to $K3\#\overline{CP^2}$? One possible answer is to see R^4 as a small part of the entire universe which remains outside of our observational capabilities. It is not excluded by current experiments (cf. Asselmeyer-Maluga and Król, 2018). However, accepting this point of view there remains the question about the origins of shape and compactness of the 4-dimensional large universe like $K3\#\overline{CP^2}$. One indication is the uniqueness of the $K3$ surface as Ricci flat Callabi-Yau manifold, without closed time-like loops. Possibly certain minimality conditions imposed from general relativity would enforce such structure of the universe. Moreover this is the peculiar and important prediction of our model that the universe at largest scales is compact and based on smooth (exotic) $K3$ surface.

Another possibility is the fundamental role ascribed to exotic 4-smoothness on open 4-manifolds in the evolution of the universe, especially exotic R^4 and $S^3 \times_\Theta \mathbb{R}$. This indicates rather technical and purely mathematical appearance of $K3\#\overline{CP^2}$ which, however, determines both spatial topological transitions supporting physical results. Anyway the model shows that exotic smoothness on open 4-manifolds appears as new and fundamental tool for physics. Many unanswered so far questions of physics gain new formulations resulting in their resolutions.

It seems quite important to derive these exotic smooth manifolds directly from QM formalism. If succeeded the model would show very strong indication that the differential structure of 4-dimensional spacetime regions *must* be exotic. We showed that the standard structure on large scales of the universe does not agree with QM. Similar approach, though using somewhat different techniques, have been proposed and developed already in (Król, Asselmeyer-Maluga et al., 2017; Król, Klimasara et al., 2017). The proposal here is an important step into this direction. There remains, however, to determine precisely this unique exotic $R^4 \subset K3\#\overline{CP^2}$ from QM.

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The theory of ideas and Plato's philosophy of mathematics

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Abstract

In this article I analyze the issue of many levels of reality that are studied by natural sciences. A particularly interesting level is the level of mathematics and the question of the relationship between mathematics and the structure of the real world. The mathematical nature of the world has been considered since ancient times and is the ongoing subject of research for philosophers of science to this day. One of the viewpoints in this field is mathematical Platonism.

In contemporary philosophy it is widely accepted that according to Plato mathematics is the domain of ideal beings (ideas) that are eternal and unalterable and exist independently from the subject's beliefs and decisions. Two issues seem to be important here. The first issue concerns the question: was Plato really a proponent of present-day mathematical Platonism? The second one is of greater importance: how mathematics influences our understanding of the nature of the world on its many ontological levels?

In the article I consider three issues: the Platonic theory of "two worlds", the method of building a mathematical structure, and the ontology of mathematics.

Keywords

mathematical Platonism, ontology, Platonic Academy.

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Evolution has provided us with the ability to describe the world appropriately, at least onour scale. However, from the point of view of evolution, there is no reason for us to be able to describe it on other scales. Science and philosophy convince us that these different scales really exist. The problem is, however, that we are forced to perceive the world on many scales from the perspective of one particular scale. The questions thus arise: what does the world look like on our scale, and what does it look like on other scales? If we focus our attention on natural sciences, I think that one can agree with the statement that the most accurate description of reality at various scales is provided by the language of mathematics. If so, then the image of the world depends essentially on the description that is made in this language. This means, however, that mathematics determines what the world looks like, especially at those levels that are not directly accessible for us. But is mathematics the same as the structure of the world, or is it only its subjective image? Already in Antiquity attempts were made to answer these questions. Particular attention was paid to them by Plato and members of his Academy who initiated the program of mathematical natural sciences—a program that has become the basis for the development of the technical civilization. In this way, the Academy has become not only the first university in the history of the world, but also the place where the most important projectcrucial for the development of science and civilization was created. Its creators, apart from Plato, were such eminent thinkers as Eudoxos of Knidos, Theaetetus, Menaichmos, Theudius,Leon, Speusippus, Xenocrates, Heraklides of Pontus,

It is widely accepted in the contemporary philosophy of mathematics, that according to Plato, mathematics is the domain of ideal beings (ideas) that are eternal, unchanging and exist independently of the subject's decisions (Brown, 2008, pp.12–16). Two issues seem important. The first one concerns the question: Was Plato really thinking that way? The second one is more important: How does mathematics influence our understanding of the nature of the world on its many levels of being?

Three things need consideration. The first one is related to the Platonic theory of "two worlds." The second one—with the method of building a mathematical structure. **The t**hird one concerns the ontology of mathematics.

The popular belief is that in Plato's philosophy we have two worlds separate from each other, one of which is the world of phenomena, and the other is the world of ideas (Dembiński, 2007). The conclusion derived from this assumption is that mathematics belongs to the Platonic world of ideas, which is eternal and unchanging. In this world, **mathematical** objects obtain the status of ideal beings and their existence is independent from the subject and phenomena. One can only acquire knowledge about this self-existing world of mathematical objects through the intellect. Unfortunately, this popular belief is in fact inconsistent with Plato's philosophy and his understanding of mathematics. Nevertheless, it has persevered in the history of the interpretation of Platonic philosophy and **has been** transferred to the philosophy of mathematics. The reason lies in the adaptation of a simplified vision of Platonism, which assumes the existence of only two levels of reality, the level of ideas and the level of phenomena, with the mathematical objects situated among the former. Meanwhile, in his analysis of mathematical objects' mode of existence, Plato concluded that such objects are related neither to the world of ideas nor to the world of phenomena. He claimed that they occupy an intermediate position and do not belong to any of them.

[1] Instead, they are merely the product of our mind. This position requires a further explanation, because it is of crucial importance for Plato's understanding of mathematics, as well as on the contemporary discussions about mathematical Platonism.

Plato tries to explain his position, first and foremost, in books VI and VII of the *Republic* and in *Letter VII*. The original starting point is the conviction that the first stage of cognition (including mathematical cognition) emerges from the observation of nature in its phenomenal form. Then the sensory imaginations arise, which provide only the images of the phenomena (*eikasia*). We see, says Plato, the reflections of reality that are born in our senses. They are imperfect and often illusive.[2] At the next stage, we try to make these images credible (*pistis*). We try to confirm the data of sensual experience by examining and observing certain states of things, from as many perspectives as possible (Plato, 1955, Republic, 509d-511e). Today, such behavior would correspond to empirical tests. The most important element of this study has given Plato the ability to discern patterns present in phenomena. These patterns indicate the order according to which the phenomena are organized, as well as the existence of regularities that this order defines. Plato discusses movement patterns, harmony patterns, or (in relation to human activities) ethical and aesthetic patterns. In the context of the Platonic philosophy of mathematics, the most important role is played by the movement patterns of the celestial bodies, based on the man's ability to recognize them. Plato

[1] "Further, he states that besides sensible things and the Forms there exists an intermediate class, the objects of mathematics, which differ from sensible things in being eternal and immutable, and from the Forms in that there are many similar objects of mathematics, whereas each Form is itself unique." (Aristotle, 1924, p.987b).

[2] "first, shadows, and then reflections in water and on surfaces of dense, smooth and bright texture, and everything of that kind, if you apprehend." (Plato, 1955, Republic, 510a).

considers this ability to be the supreme gift of the gods. Observation of patterns in nature: rhythms, motifs, harmony, symmetry or proportion, directs the subject's attention towards their source (Plato, 1955, Timaeus, 47a-e). But the source itself is no longer available for sensual cognition. It is available only for intellectual cognition. On the border between sensual and intellectual cognition, there is a kind of intuition that Plato describes as "the suspicion of truth." The point is that from the sensory data, basing on the perceived regularities, patterns and proportions, one is able to formulate hypotheses regarding their source. Plato calls these hypotheses "true opinion (*alēthēs doxa*)" (Plato, 1955, Meno, 85c.98a.97c). To confirm their value, one is asked to verify them. This is the task of yet another higher cognitive power, which is the reason (*dianoia*). To put it simply, the task of reason is to conduct logical chains of inference and to analyze causal relationships, what Plato calls "causal splicing (*symploke*)" (Plato, 1955, Meno, 97e-98a). Reason is the authority of the subject, who plays the essential role in the Platonic philosophy of mathematics. His role boils down to creating intellectual models of sensually given states of affairs and patterns perceived in nature. These models are the representation of phenomena and patterns at the level of intellect, based on the abstraction skills (*aphairesis*) and they constitute the product of the activities confirming the permanent occurrence of a certain set of features in a certain class of objects.[3] Abstraction concerns the regularities observed in nature, which in turn were derived at the levels of *eikasia* and *pistis* (*doxa*). Such models are also math-

[3] „For I think you are aware that students of geometry and reckoning and such subjects first postulate the odd and the even and the various figures and the three kinds of angles and other things akin to these in each branch of science, regard them as known, and, treating them as absolute assumptions, do not deign to render any further account of them to themselves or others, taking it for granted that they are obvious to everybody." (Plato, 1955, Republic, 510c).

ematical objects, according to Plato. The examples are numbers. Euclid, who was educated at the Plato Academy, complies with Plato's intuition by defining the number as "a multitude made up of monads" (*Arithmos de, to ek monadōn sygkeimenon plēthos*). A number is defined by a monad (multiple monads). But what is a monad? It is a model created at the level of intellect, allowing to describe the infinite multitudes appearing at the level of the senses (things and their reflections). In contrast to sensual images, which are always different, a monad is "always equal to every other, and no different from any other, and has no part in it" (Plato, 1955, Republic, 526a). It is a model created by the intellect, presenting structural features of every sensual multitude. The model understood in this way has a completely different status than counted items and, most importantly, it is the creation of the object. In this sense too, the numbers are the objects of the object. The same applies to geometrical objects. A line is a length without a width, a surface possesses only length and width, and a circle is a plane figure contained by one line comprised of points equally distant from the circle's center. None of the sensory objects has such properties. One can consider the internal structure of the model, one can also analyze the relationship of a given model to other models, one can finally examine which models are possible, which are necessary and which are completely excluded. The analysis of these models is the subject of the work of mathematicians. However, a mathematician, let alone a philosopher, cannot avoid asking questions concerning the legitimacy of creating such models. The question arises: What makes mathematical objects, which are human creations, not arbitrary?

Searching for the answer, Plato appealed to the concept of ideas-measures. He claimed that mathematics can neither derive its own justification from the phenomena, which are variable and temporary, nor from the arbitrary decisions of the subject. However, there must be something that guarantees the functioning of the cosmic order, and also ensures the correctness of the mathematical models constructed. In this context, Plato proposes the adoption of the eternal model of the organization of the world, which is created according to unchanging regularities that define the order of the Cosmos. It was these norms, these measures establishing the model of the cosmic order, which he called ideas. Today, their equivalent would be the laws of nature and the laws of physics. It is not difficult to notice that these laws exist differently than phenomena do. A physical law and its implementation have different ontological statuses. As far as we know, the former is immutable, has the feature of unity (there are no two identical laws), and does not depend on the decision of the subject. We can only say about such laws that they are, and that they are always as they are. Plato attributed to this way of existence the name of being, and their mode of being he called "really real" (*ontōs on*). He claimed that beings, ideas, inhabit a separate reality which constitutes the (eternal) organizational model of the Cosmos, and is the essence of existence of all phenomenal structures and processes. This model manifests itself in the form of symmetry, proportions, various types of harmony, which can be understood as defining the essence and behavior of phenomenal structures. The goal of philosophy and science, says Plato, is to reach the idea-measures, that condition a particular kind of order, regardless of whether it is cosmic, ethical or aesthetic. Of course, this also applies to the mathematical order. That is why Plato also postulates the existence of mathematical ideas, that form the basis and cause of mathematical order. However, the most important is, and what must be always remembered, that these ideas are not mathematical objects themselves. Mathematical ideas constitute a completely autonomous world, existing outside the world of our mathematics, just as the laws of physics exist outside the world of physical theories we create. As Aristotle comments, no mathematical operations can be performed on ideas (Aristotle, 1924, Metaphysica, 1081a). One can only examine the relationships that exist between them. However, for such a study the mathematical method with its axiomatic approach is inapplicable. It is rather the dialectical method, whose purpose is exactly to study the relationship between the ideas, which is appropriate. Confusion among of mathematical Platonism stems from the unawareness of the difference between mathematical ideas and mathematical objects. Plato tries to explain precisely that issue in *Letter VII*.[4]

Plato considers a simple mathematical object—a circle. We can assign a name to it. It could be changed, because—as he argues—"none of the objects, we affirm, has any fixed name, [...] nor is there anything to prevent forms which are now called 'round' from being called 'straight,' and the 'straight' 'round'; and men will find the names no less firmly fixed when they have shifted them and apply them in an opposite sense." (Plato, 1955, Letters , 343b). Next, we try to formulate a fairly precise definition of a circle. It should cover everything that is round and circular. Most often, according to Plato, an imperfect definition is formulated, based on specific wording, which "inasmuch as it is compounded of names and verbs, it is in no case fixed with sufficient firmness." (Plato, 1955, Letters , 343b). In the further course of the procedure, an attempt may be made to build a model or a schema, corresponding to what has been defined. We can do this by creating thoughtful constructions, presenting drawings or spatial visualizations. Later, the analysis of thus obtained model and its relationship to other models (mathematical objects) is developed into a special theory, which includes all previous stages. Theory is

[4] For an in-depth discussion of this issue, see: (Dembiński, 2003, pp.55–110).

the highest degree of cognition. That level a cognizing subject can attain thanks to his abilities, i.e. sensual perception, abstraction, and logical analysis. Plato, however, strongly believes that regardless of the degree of precision available at the above-mentioned levels of cognition, one should be aware that "their inaccuracy is an endless topic" (Plato, 1955, Letters , 343b), how much arbitrariness and uncertainty is associated with them, and how much they depend on the cognizing subject and its limitations. Meanwhile, mathematical cognition is required to be certain; to be characterized by necessity, universality and truthfulness. Therefore, there must be some basis, upon which we could justify and validate the four existing procedures of cognition (name, definition, model and theory). We find it, according to Plato, inthe idea of the circle, "the circle as such" (*autos o kyklos*). Such an idea must be called a real being (*alethés òn*), the essence of a thing (*tode ti*). Plato describes it as "the Fifth (*to pempton*)" (Plato, 1955, Letters , 342a–343d). The circle "in itself" exists differently than the one which is the intellectual model or the circle we draw, which the wheelwright creates, or which we observe in phenomena. The circle "in itself" is the highest, unchanging and only measure of all circularity, a condition for the possibility of creating theories, models, definitions and names involving the circular. The circle "in itself", as a regularity, as the measure of the specificity of everything that is circular, is unique, unchangeable and independent of the subject's beliefs.The same applies to numbers. If we take a numerical idea, for example the ideal number two, then with its help we are able to determine the essence of each mathematical two. Using the descriptive language, one can say that the ideal number two defines the structural features of each mathematical two. And the mathematical two is only an intellectual model created by the subject. If we add two plus two, we are not in the world of ideas, but at the level of our mathematics. A good description of this situation was presented by Michael Heller. He accepts the distinction between mathematics with a lowercase “m”, and Mathematics with a capital “M”. The former is the mathematics created by man. The latter is the mathematics which is inherent to nature, and to which we have no direct access. Indirect access is provided only by means of representation, which is our mathematics, the mathematics with a lowercase “m”. The Mathematics with a capital “M” corresponds to the level of Platonic, mathematical ideas. This situation is explained by Plato in the “Metaphor of the Cave”. We humans are only able to see the world of shadows (Plato, 1955, Republic, 514–518d). We know, however, that these shadows are shadows of something we do not directly see (real figures, fire). Therefore, we are forced to create images, models of what we do not see. These models come with all the disadvantages and limitations that arise in the subjective process of cognition. However, according to Plato, there are moments (being a gift of the gods), when we are temporarily given a somewhat vague, intuitive “seeing” of the outlines of regularity, organizing the order of nature. This are the moments when someone in the cave suddenly “senses” that there is “something” which is the cause and condition of the existence of shadows. Plato describes this moment as the “conversion of the soul” (*periagoge tes psyches*). In such a “foresight”, however, we are not able to stay for long, instead we return quickly to our shadows, and again continue the arduous inference from the representation. We return to our mathematical world of shadows, to our mathematics named with the lowercase “m”.

The proponents of mathematical Platonism claim that—according to Plato–mathematical objects exist independently of the subject. We still **need** to answer the question: Where did this conviction originate?The answer is simple. Such a belief was born not in the thoughts of Plato but in the thoughts of his successors, Speusippus and Xenocrates (see Dembiński, 2010; Dillon, 2003). Speusippus decided that the Platonic world of ideas should be inhabited by mathematical objects.[5] He assigned to them all the attributes of an idea: separate existence, eternity, immutability and independence from the subject. He decided that it is unnecessary to double the worlds and postulated the existence of something beyond just mathematics. In this way, he put mathematics at the top of the world of Beings. Mathematics took the place of the Platonic world of ideas. In this way, he expected to eliminate the difficulties associated with explaining the relationship between ideas (ideal numbers, ideal figures) and the objects of mathematics. He thought that it is sufficient to recognize the objects of mathematics themselves as ideals and that there is no need to introduce difficult notions of ideal numbers and figures. Aristotle did not consider such a solution as a good one. Probably because he thought that Speusippus wanted to replace in this way philosophical cognition of the world—and even its very existence—with mathematics. Aristotle attributed to Speusippus the claim that the whole philosophy of his time can be reduced to mathematics (Aristotle, 1924, Methaphysics, 992a30). Speusippus stand was strengthened by Xenocrates, another Academy scholar, who decided to replace ontology with mathematics. He claimed that mathematics is the only acceptable ontology, because the world is in fact created and constituted according to mathematical patterns and structures.

Ideas are, according to Xenocrates, identical with mathematical numbers. Their geometrical form constitutes geometric ideas. Aristo-

[5] "Those who recognize only the objects of mathematics as existing besides sensible things, abandoned Ideal number and posited mathematical number because they perceived the difficulty and artificiality of the Ideal theory." (Aristotle, 1924, Methaphysics, 1086a).

tle considered this solution to be the worst one. Perhaps he thought so because he accepted Plato's conviction that ideas are independently existing entities on which no mathematical operations can be carried out. As he believed, treating objects of mathematics as ideas would exclude the possibility of the existence of mathematics.However, the proposal of Xenocrates could have a different meaning. Recognizing mathematical objects as ideas, Xenocrates wanted to draw attention to the deep relationship of ontology with mathematics, where mathematics is understood as the only acceptable ontology. If the world were ultimately created according to mathematical structures and patterns, mathematics would be its proper ontology. In similar manner, for example, R. Penrose and many mathematicians admitting mathematical Platonism. Thus, modern advocates of mathematical Platonism must remember that by adopting Platonism, they essentially adopt the position of Speusippus and Xenocrates, not of Plato himself. This, of course, is still Platonism in a broad sense. Nevertheless, it is not a Platonism understood as Plato's position.

Above considerations lead to the following conclusions: first of all, Plato's concept of mathematics is not the one that is usually referred to by the adherents of mathematical Platonism. Secondly, the mathematics we use is always our human mathematics created by us for the sake of representing nature, whereas nature itself uses different mathematics, the outline of which we can see only fragmentarily through intellectual intuition. We will never fully see the Mathematics of nature, because it is directly inaccessible for us. We know, however, that this "capital-M" Mathematics is there, and that it justifies the existence of our mathematics, the "lowercase-M" mathematics of the shadow world. This also applies to logic that organizes the world on its many levels. We, the people who see only shadows, want to describe and understand other levels of reality with this view. But these levels exist differently, have a completely different structure, and different logic. Getting to know them requires different methods. Plato suggested that this situation should be taken into account, without confusing the modes of existence at various levels. The most common type of error we make is the attempt to describe other levels using the methods valid at our own level. Yet ideas, mathematical objects, and other time-space phenomena exist differently. Today, we begin to understand that the logic valid at the microscale is different from the one at our scale, and different from the logic at the macroscale. Perhaps it is worth to resort to the intuition of the ancient thinkers who, as the poets say: are closer to the gods, and see better than us.

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Artykuły

Articles

Does the quantum mechanical wave function exist?

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Abstract

I address the question whether the wave function in quantum theory exists as a real (ontic) quantity or not. For this purpose, I discuss the essentials of the quantum formalism and emphasize the central role of the superposition principle. I then explain the measurement problem and discuss the process of decoherence. Finally, I address the special features that the quantization of gravity brings into the game. From all of this I conclude that the wave function really exists, that is, it is a real (ontic) feature of Nature.

Keywords

wave function, quantum mechanics, ontology, quantum formalism, superposition principle, measurement problem, decoherence, quantum gravity.

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1. Quantum theory

The title of my contribution may sound somewhat surprising, at least at first glance. After all, the quantum mechanical wave function and its generalizations in quantum field theory (generically here called Ψ) are standard tools in quantum theory and its many applications in physics, chemistry, and even biology. This is true, and one can definitely say that Ψ exists in a mathematical sense. The question addressed here instead refers to whether Ψ can be attributed an ontic or merely an epistemic role, that is, whether Ψ can be attributed reality in the same way as, for example, an electric field possesses, or whether it merely describes something like an information catalogue (as Schrödinger once put it). This is a question that has occupied physicists since the advent of quantum theory in the 1920s and that still occupies them today; see, for example, d'Espagnat (1995), Kiefer (2015a), or Boge (2018) and the many references quoted therein. Here, I will argue that that the answer to the question posed in the title is definitely in the affirmative, and I will try to put together the main arguments of why this is so and why the wave function has an ontic (real) status. Some of these arguments have been presented in an earlier article (Kiefer, 2012a), to which I will occasionally refer.

At the heart of all of quantum theory is the *superposition principle*. It can be separated into a kinematical and a dynamical version (Joos, Zeh et al., 2003). The kinematical version expresses the fact that if Ψ_1 and Ψ_2 are physical states, then $\alpha\Psi_1 + \beta\Psi_2$ is again a physical state, where α and β are complex numbers. For more than one degree of freedom, this leads to the important concept of *entanglement* (*Verschränkung*) between systems (Kiefer, 2015a), which plays a particular important role in modern developments such as quantum information. The very concept of a quantum computer relies on entanglement.

It is clear that this kinematical version only makes sense if it is consistent with the dynamics of the theory. But this is the case. The fundamental equation is the Schrödinger equation (by which I include its field theoretic generalization, the functional Schrödinger equation), and this equation is *linear*: the sum of two solutions is again a solution. An importance consequence of the superposition principle is obvious: the space of what we may call "classical states" form only a tiny subset in the space of all possible states. A simple example is the superposition of two localized states, each of which can describe a classical state, to a nonlocal (and thus nonclassical) state. It must be emphasized that the quantum mechanical wave functions are not defined on spacetime, but on *configuration space* (cf. e.g. Zeh, 2016 for a lucid conceptual discussion). Except for the case of one particle, this is a high-dimensional space: the dimension is $3N$ for a system of N particles, and infinite in field theory. Otherwise, there would be no entanglement between systems.

Entanglement is the central distinguishing feature of quantum theory. As already Erwin Schrödinger put it (Schrödinger, 1935, p. 555):

> I would not call that *one* but rather *the* characteristic trait of quantum mechanics, the one that enforces its entire departure from classical lines of thought. By the interaction the two representatives (or ψ-functions) have become entangled. . . . Another way of expressing the peculiar situation is: the best possible knowledge of a *whole* does not necessarily include the best possible knowledge of all its *parts*, even though they may be entirely separated . . .

The superposition principle has been experimentally tested in uncountably many experiments (Schlosshauer, 2007; Kiefer, 2015a). Even before the term entanglement was coined, it was clear that the electrons in a helium atom must be entangled in order to lead to the correct observed binding energies (Hylleraas, 1929). Modern experiments include the interference of biomolecules, the entanglement of photon pairs over distances of hundreds of kilometres, and the observation of neutrino oscillations, to name only a few; see, for example, Deng *et al.* (2019) for an experiment involving interference between light sources separated by 150 million kilometres. There can thus be no doubt that the superposition principle holds. The generation of “macroscopic” superpositions is being seriously considered; see, for example, Clarke and Vanner (2018).

In the mathematical language of quantum theory, the validity of the superposition principle is encapsuled in the use of vector spaces for the quantum states (wave functions). The stronger concept of a Hilbert space (using a scalar product between states) is motivated by the probability interpretation of quantum theory, which by itself is connected to the “measurement problem” discussed since the early days of the theory. This measurement problem refers, in fact, to the only class of situations in which the superposition principle seems to break down.

What is the measurement problem? Let us consider the simple situation of an apparatus, A, coupled to a system, S:[1] I emphasize

$$\boxed{\quad \mathrm{S} \quad} \longrightarrow \boxed{\quad \mathrm{A} \quad}$$

that both system and apparatus are described by quantum theory. This analysis goes back to John von Neumann (Neumann, 1932). The simplest situation of an interaction is the “ideal measurement”: the system is not disturbed by the apparatus, but the state of the apparatus becomes correlated with the state of the system. If S is in a state $|n\rangle$ and A in an initially uncorrelated state $|\Phi_0\rangle$, the total state of S and A evolves as

$$|n\rangle|\Phi_0\rangle \xrightarrow{t} |n\rangle|\Phi_n(t)\rangle. \tag{1}$$

[1] This and the following diagramme are taken from our monograph (Joos, Zeh et al., 2003).

The measurement problem appears when we consider a superposition of possible states $|n\rangle$.[2] This leads to the evolution:

$$\left(\sum_n c_n|n\rangle\right)|\Phi_0\rangle \xrightarrow{t} \sum_n c_n|n\rangle|\Phi_n(t)\rangle, \tag{2}$$

where $|\Phi_n(t)\rangle$ is the resulting state ('pointer state') of A. But (2) is a *macroscopic* superposition! Since such superpositions[3] are not observed, John von Neumann postulated the occurrence of a "collapse of the wave function" in measurement-like interactions; he did not, however, present a dynamical equation for such a collapse, which must be unitarity violating and is thus in conflict with the Schrödinger equation.

In more recent years, various models of wave function collapse have been presented in the literature and possible experimental tests have been discussed.[4] It must be emphasized that most of these models only make sense if the wave function acquires a real (ontic) status. This is different from its role in the Copenhagen interpretation of quantum theory, where the 'collapse' has the mere formal meaning of an information increase. We shall see in the next section how we can proceed without assuming a dynamical collapse, that is, without violating the unitarity of quantum theory.

Before doing so, I want to conclude this section with some remarks on relativistic quantum theory, in particular the Dirac equation. The Dirac spinor appearing there should not be confused with the wave function discussed above. The spinor is *not* defined in configuration space; it is defined on a classical (in general four-dimensional)

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[2] In the simple situation of spin-1/2, one would have two states $|n\rangle$, one corresponding to (say) spin up and the other to spin down.

[3] An especially impressive example is Schrödinger's cat.

[4] See, for example, Bassi *et al*. (2013) for a comprehensive review.

spacetime. It thus cannot describe entanglement and can only serve to address one-particle situations; it can describe correctly the situation in the hydrogen atom, but cannot even be formulated for the helium atom.[5] Relativistic quantum theory is only consistent in the form of quantum *field* theory; the Dirac equation follows from quantum electrodynamics (QED) for the special case of one-particle excitations. When we talk here about the ontological status of Ψ, this refers in the general case of quantum field theory to wave *functionals*. These functionals are defined on the configuration space of all fields; in the case of QED, for example, this is the space of all vector potentials and charged Grassmann (anti-commuting) fields.

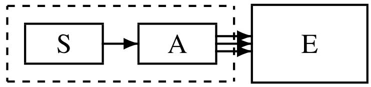
2. Decoherence

How can one understand the nonobservation of superpositions such as (2) without advocating an explicit collapse? The key role in answering this question is played by the presence of the ubiquitous environment for the apparatus. This was clearly recognized in the pioneering work by H.-Dieter Zeh in 1970.[6] 'Environment' is here a technical terms that stands for additional degrees of freedom whose interaction with the 'apparatus' (or other systems under consideration) cannot be avoided. In concrete examples, these may be air molecules or photons that scatter off the 'apparatus'. One thus has instead of the above diagramme the following situation:

Here, E stands for the environmental degrees of freedom, and the three arrows between A and E indicate the many degrees of freedom.

[5] Cf. in this context Zeh (2016).

[6] The original reference is Zeh (1970). See Joos *et al.* (2003) for details and references.



If the environment is in an initial state $|E_0\rangle$, the superposition principle for the whole system of S, A, and E leads to

$$\left(\sum_n c_n|n\rangle|\Phi_n\rangle\right)|E_0\rangle \quad \stackrel{t}{\longrightarrow} \quad \sum_n c_n|n\rangle|\Phi_n\rangle|E_n\rangle. \tag{3}$$

But this is an even more macroscopic superposition than (2) because it not only includes system and apparatus, but also the many degrees of freedom of the environment. Has the situation not become worse now? The answer is no. The reason is because the degrees of freedom of the environment are in general not accessible; when dealing with S and A only, one has to trace them out and to consider instead the reduced density matrix of S and A alone, from which all *local* observations follow. Since different environmental states are in general orthogonal (because they can discriminate between different states of A), $\langle E_n|E_m\rangle \approx \delta_{nm}$, the reduced density matrix assumes the form

$$\rho_{\mathrm{SA}} \approx \sum_n |c_n|^2|n\rangle\langle n| \otimes |\Phi_n\rangle\langle\Phi_n|, \tag{4}$$

which is approximately (but not identically) equal to a classical stochastic mixture. The information about the original superposition of (2) has now been transferred to a nonlocal correlation between S and A on the one side, and E on the other side. They are no longer observable at S and A itself: "The interferences exist, but they are not *there*." The various system states $|n\rangle$ are distributed with probabilities $|c_n|^2$ according to the Born rule of quantum theory. (It should be noted, though, that the very notion of density matrix is based on the validity of the Born rule.)

This irreversible emergence of classical properties (nonobservability of interference terms) through the unavoidable interaction with the environment is called *decoherence*. It has been explored in the last decades, both experimentally and theoretically.[7] According to decoherence, macroscopic objects *appear* classically, although they are fundamentally described by quantum theory. Decoherence is a process that can be treated entirely within standard quantum mechanics and which can be based on realistic processes discusses in a quantitative manner.[8]

What are the consequences of this for the interpretation of quantum theory in general and for the wave function in particular? If the superposition principle and the Schrödinger equation are universally valid, one arrives at what is called the Everett or many-worlds interpretation (see e.g. d'Espagnat, 1995; Zeh, 2016). Unitary quantum theory is then exact and never violated. The dead and the alive Schrödinger cat, for example, then indeed exist simultaneously in different "Everett branches", and also the observer seeing the cat exists in two versions. In this point of view, the wave function definitely has an ontic status and exists in the way discussed above. The Everett interpretation together with decoherence makes the measurement problem obsolete.

A question often asked is about the derivation of the probability interpretation (the Born rule) in the Everett picture. This has been discussed at length in the literature; see, for example, Zurek (2018) and the references therein. The probability interpretation only makes sense for situations in which decoherence is effective, because only

[7] Major reviews are Joos et al (2003), Zurek (2003), Schlosshauer (2007). Crucial experiments are also discussed in Haroche (2014).

[8] Such quantitative calculations were presented for the first time in Joos and Zeh (1985).

then the various alternatives can be treated independently and can be assigned probabilities. Whether the Born rule can then really be *derived* or only be made plausible, is a contentious issue. But what is clear that the Everett interpretation together with decoherence and the Born rule gives a consistent picture that is not in need of completion.

The Everett interpretation is the simplest one at the level of the mathematical formalism. The fundamental equations are all linear. It is not a simple interpretation if one sticks to a classical picture of the world. This is what the main alternative – explicit collapse models – wants to achieve (see e.g. Bassi et al., 2013). But this requires a modification of the usual formalism by bringing in nonlinearities or stochastic terms. Also here, the wave function assumes an ontic status. The main task is to work out concrete models and to explore their experimental status.

A rather mild modification is the de Broglie–Bohm theory. The Schrödinger for Ψ is left untouched, but in addition particle (or classical field) configurations are introduced. The wave function, which is defined in configuration space, acts as a kind of 'guiding field' for the particles in ordinary space. There, too, it has an ontic status and can thus be assumed to exist. At least in nonrelativistic quantum mechanics, the predictions of the de Broglie–Bohm theory agree with the predictions of standard quantum theory.

The prototype of an epistemic point of view is the Copenhagen interpretation. There, Ψ merely provides an increase of information during a measurement and has no physical existence on its own – only the classical concepts such as particle posititions have. But is such a point of view really consistent and satisfactory? This is hard to believe. New light on these interpretational questions is shed by entering the realm of quantum gravity and quantum cosmology. This is the topic of my final section.

3. Quantum gravity

In 1957, a group of distinguished physicists met at the University of North Carolina to explore the prospects of gravitational physics. This also included the possible quantization of the gravitational field. In the discussion, Richard Feynman came up with the following gedanken experiment. In a Stern–Gerlach type of setting, a particle is brought into a superposition of, say, spin up and spin down. Introducing some interconnections to a macroscopic object, say a ball of 1 cm diameter, one can bring the ball into a superposition of being translated upwards and downwards. But this corresponds to a superposition of two measurable gravitational fields (measurable e.g. with a Cavendish balance). Feynman then states (De Witt, 1957):

> …if you believe in quantum mechanics up to any level then you have to believe in gravitational quantization in order to describe this experiment. …It may turn out, since we've never done an experiment at this level, that it's not possible …that there is something the matter with our quantum mechanics when we have too much *action* in the system, or too much mass—or something. But that is the only way I can see which would keep you from the necessity of quantizing the gravitational field. It's a way that I don't want to propose. …

In order words, unless one assumes that the superposition principle and the standard formalism of quantum theory is violated when gravitational fields play a role (as, for example, Lajos Diósi and

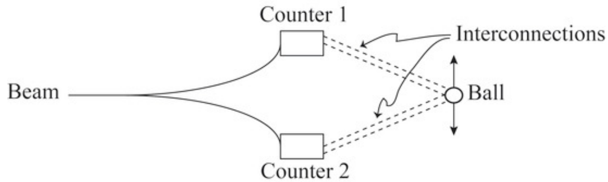


Figure 1: Feynman's gedanken experiment in which a microscopic superposition is transferred to a macroscopic one of a ball being simultaneously at two different places. Figure adapted from De Witt, 1957.

Roger Penrose envisage), the quantization of gravity seems unavoidable. The majority of researchers thus accepts the assumption of extrapolating the standard linear formalism of quantum theory to quantum gravity. This holds for almost all of the existing approaches, from canonical and covariant quantum gravity up to string theory (Kiefer, 2012b).

At present, there is a discussion about the possibility to observe gravitational superpositions in the laboratory. There are proposals to probe a nonclassical gravitational field generated by two masses each of which is superposed at two locations (see e.g. Marletto and Vedral, 2017) or to probe such a field generated by the superposition of one mass in the spirit of Feynman's proposal cited above (see e.g. the remarks in Schmöle et al., 2016). The observability of such superpositions also meets with criticism (Anastopoulos and Hu, 2018).

What are the consequences of quantum gravity for our question about the reality of the wave function? In order to answer this question, it is sufficient to use the simplest and most conservative approaches to quantum gravity, which is quantum geometrodynamics.[9]

[9] Details and relevant references can be found, for example, in my monograph (Kiefer, 2012b).

One arrives at this theory when asking the following question: what is the quantum formalism that gives back Einstein's equations in the semiclassical (WKB) limit? This is analogous to the heuristic procedure that Erwin Schrödinger led to his equation in 1926.

The canonical formalism of general relativity discloses the real dynamical quantity of the theory: it is the *three-dimensional* geometry. The configuration space is thus the infinite-dimensional space of all three-dimensional metrics, with an additional constraint which guarantees that metrics related by coordinate transformations are counted only once. The theory possesses four local constraints, which after Dirac quantization are heuristically transformed into quantum constraints on physically allowed wave functionals. In a shorthand notation, they read

$$\hat{H}\Psi = 0, \tag{5}$$

where $\hat{H}$ denotes the Hamilton operator of all gravitational and nongravitational degrees of freedom. The functional Ψ is defined on the space of three-metrics and nongravitational fields. Equation (5) is also called Wheeler–DeWitt equation.[10]

One recognizes from (5) the absence of any external time parameter (see in this context Kiefer, 2015b). This is obvious for conceptual reasons. In classical relativity, spacetime (four-geometry) plays the same role that a particle trajectory plays in mechanics. After quantization, spacetime has disappeared in the same way as the particle trajectory has disappeared in quantum mechanics. But whereas in quantum mechanics Newton's absolute time t has survived, no such absolute time is present in Einstein's theory. As a result, the fundamental quantum gravity equations are timeless.

[10] More precisely, if written out, (5) includes the Wheeler–DeWitt equation and the diffeomorphism constraints.

Of special concern here is quantum cosmology – the application of quantum theory to the Universe as a whole. In the simple case of a Friedmann universe with scale factor a and a conformally coupled scalar field χ, the Wheeler–DeWitt equation assumes the form (after some rescaling and with a suitable choice of units):

$$\hat{H}_0\psi(a,\chi) \equiv (-H_a + H_\chi)\psi \equiv \left(\frac{\partial^2}{\partial a^2} - \frac{\partial^2}{\partial \chi^2} - a^2 + \chi^2\right)\psi = 0. \tag{6}$$

How can one interpret such equations? At the most fundamental level, there is no time and there are no classical observers who could perform measurements. Therefore, the Copenhagen interpretation which requires the need of classical measurement agencies from the outset, is inapplicable. The question thus arises in which limit approximate notions of time and observers (more generally, of classical properties) emerge and what relevance this emergence has for the interpretation of the wave function.

Such a limit exists and is well understood (Kiefer, 2012b; 2015b). It is similar to the Born–Oppenheimer approximation in molecular physics. If one adds inhomogeneous degrees of freedom to the Hamiltonian in (6), the Wheeler–DeWitt equation is of the form

$$\left(H_0 + \sum_n H_n(a,\phi,x_n)\right)\Psi(\alpha,\phi,\{x_n\}) = 0, \tag{7}$$

where the x_n stand for the inhomogeneities (gravitational waves, density perturbations). Writing $\Psi = \psi_0 \prod_n \psi_n$ and assuming that ψ_0 is of WKB form, that is, $\psi_0 \approx C\exp(\mathrm{i}S_0/\hbar)$ (with a slowly varying prefactor C), one gets

$$\mathrm{i}\hbar\frac{\partial \psi_n}{\partial t} \approx H_n\psi_n \tag{8}$$

with

$$\frac{\partial}{\partial t} := \nabla S_0 \cdot \nabla.$$

This is nothing but a set of time-dependent Schrödinger equations for the inhomogeneities with respect to a time variable t that is defined from the homogeneous cosmological background; t is also called 'WKB time' and controls the dynamics in this approximation. Only in this limit can one talk about the probability interpretation of quantum theory and the existence of observers. It is thus not at all obvious whether the standard notion of Hilbert space need, or even can, be extrapolated to the level of full quantum gravity (beyond this level of the Born–Oppenheimer approximation).

In quantum cosmology, arbitrary superpositions of the gravitational field and matter states can occur. How can we understand the emergence of an (approximate) classical Universe? This is achieved by the process of decoherence introduced above (Kiefer, 2012a). Decoherence is a process in configuration space, and the irrelevant degrees of freedom can be taken to be part of the inhomogeneities x_n. In this way, the scale factor a and the field χ can be shown to assume classical properties. The same then holds for WKB time t, which is constructed from these background variables. After this classicality is understood, one can investigate decoherence for some relevant inhomogeneous degrees of freedom; these include the inhomogeneities of an inflaton scalar field and of the metric, giving rise, after decoherence, to the observed CMB anisotropies and the (not yet discovered) primordial gravitational waves. In all these considerations, the wave function is assumed to be real (ontic); this is also the case if one applies collapse models to quantum cosmology. I should also mention that even the problem of the arrow of time can, at least in principle, be understood in the framework of timeless quantum cosmology (Zeh, 2007).

It is clear that the debate about the correct interpretation of quantum theory will continue, at least until a clear experimental decision is reached (which could take quite a while). In this contribution, I have collected arguments which strongly support the point of view that the wave function is real (ontic), in the same way as, say, an electric field, is real. Thus, the wave function *exists*. The perhaps most important open question is: what is the configuration state for the wave functional at the most fundamental level? In canonical quantum gravity, it is the space of three-geometries plus nongravitational fields; what it is at the level of a fundamental quantum theory of all interactions, is unknown.

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Where are Sunspots? The Practical Method of Galileo as an example of Mental Model

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Abstract

After the publication of *Sidereus Nuncius* Galileo, in the controversy with Ch. Scheiner, developed several arguments on behalf of the hypothesis that sunspots are contiguous to the surface of the Sun, and presented them in his *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti* (Rome 1613). One of them, named by Galileo a Practical Method, advocates very clearly the correctness of the hypothesis. In the paper the method in question is briefly described. It is argued that the Practical Method is not a thought experiment, but rather a mental model proposed precisely in order to solve the problem of sunspots' location.

Keywords

sunspots, Galileo's practical method, mental models, early modern science.

1. Introduction

After Galileo discovered many celestial novelties through his telescope, as described in the widely read *Sidereus Nuncius*,

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his *vis polemica* found a space in the sunspot dispute with the Jesuit priest Christopher Scheiner (Camerota, 2004, pp.238–259; Fantoli, 2003, chap.2.6; Heilbron, 2010; Galilei and Scheiner, 2010; Shea, 1970; Sierotowicz, 2013)[1]. In this article, I pay special attention to a few pages from *Istoria e dimostrazioni intorno alle macchie solari e loro accidenti* (Galilei, 1613), which has been largely neglected by scholars. In the work, the Pisano[2] presented what he himself called the most powerful reason in favour of his thesis about the location of sunspots. This is an original method, called the Practical Method (see OG V, 121.5; Galilei, 2010, p.112), through which Galileo tried to prove his hypothesis by combining in a paradigmatic way sensible experiences (observations) and necessary demonstrations (a geometrical model).

Galileo Galilei, in opposition to Christopher Scheiner, considered sunspots a phenomenon that exist on the surface of the Sun and, as a consequence, participate in the rotation of the solar globe. However, according to Scheiner, sunspots are just shadows of the passage of planets distant from the Sun through the solar disk. All these planets have to move with an angular velocity that is equal to the angular velocity of the rotation of the Sun.

To decide which of these two theories is right, Galileo proposed several arguments. One of them, a sort of cinematic and geometric

[1] Bibliographical note: the *opera* of Galileo Galilei discussed in the paper can be easily found on the Web: *Istoria e dimostrazioni...*, (Galilei, 1613) see, e.g., copy of the National Library of Florence: BNCF - II 461, the Nencini collection: brunelleschi.imss.fi.it/bibliotecagalileo; [Accessed 23 Dec. 2010]. In the national edition of the works of Galilei, *Istoria e dimostrazioni...* can be found in the fifth volume: (Favaro, 1895, pp.25–239). Favaro's edition is cited here with the abbreviation OG, followed by the number of the volume, the page number, and the verse number. For the English translation, see (Galilei, 2010). Figures: For all details, see (Sierotowicz, 2013).

[2] Galileo is often referred to as the Pisano, after the city of his birth.

reasoning, was based on the variation of the relative distance between the two spots moving along the same parallel from the edge of the solar disk towards its centre. The Pisano noted that the observed distance between such two sunspots changes in a particular way because of the projection on the observation plane of the segment between the spots according to the angle that the segment forms with the plane of observation. Galileo developed a geometrical model of this situation in his work on sunspots mentioned above (Fig. 2).

The apparent distance between two sunspots is normally smaller than the actual distance between them. However, on one occasion, namely, when the segment between two spots is parallel to the observation plane, the length of the segment and the length of its orthogonal projection onto the observation plane will be the same. This happens when the straight line joining the observer's eye with the centre of the Sun is aligned with the centre of the segment that joins the spots (Fig. 6). Of course, this situation is rather exceptional, but Galileo was lucky enough to observe it for two sunspots on 01 July 1613 and 05 July 1613, respectively (Fig. 3 and 4). Extraordinary, masterly executed drawings representing these observations were printed in *Istoria e dimostrazioni…*. In fact, on July 5, 1613, spots A and B appeared to be symmetrically located with respect to the centre of the solar disk. This particular observation allowed Galileo to develop and apply the Practical Method.

2. The Practical Method

First, Galileo noted that “because the distance between the Sun and us is very great in proportion to the diameter of its body” (see OG V, 121.5; Galilei, 2010, p.112) some preliminary premises, also of a physical nature, for his model can be made:

1. The rays that reach the eye of the terrestrial observer can be considered as parallel lines (see the rays ZDG, OLI, and QP in Fig. 2);
2. Sunspots are located on the same latitude and pass through the centre of the solar disk or very close to it; and
3. This situation occurs in the case of two spots, A and B (see Fig. 3 and 4).

Starting with these suppositions, Galileo constructed the geometric model of sunspot observations. The plane of the drawing (Fig. 2) corresponds to the plane passing through the observer, the centre of the Sun, and the parallel (practically the equator) on which the sunspots A and B are located. The GDZ line, coming out from the centre of the Sun G, goes towards the terrestrial observer to whom the spots appear projected on the plane perpendicular to the plane just mentioned and passing through the centre of the Sun (the observation plane). Let CDE be the semicircle representing the surface of the Sun, or the parallel on which the A and B spots move, and let the points on the CGE segment correspond to the observed positions of sunspots in an orthogonal projection on the observation plane. The points L and H are the sunspots A and B, respectively, observed on 01 July 1613 (Fig. 3). The actual distance between these spots is equal to the HL chord, but because of the projection effects, these spots are observed as points F and I, and their observed distance is equal to the length of the FI segment. It is easy to see that in a situation where the HL segment is symmetrical with respect to the GDZ line, the observed distance should be equal to the length of the HL segment because the HL, FI, and CGE segments are parallel. This situation actually occurred on 5 July 1613 (see Fig. 4 and 6).

Naturally, the construction described above corresponds to the hypothesis that sunspots are supposed to be located on the surface of the Sun, or very near to it. What if the spots are but the shadows of a distant planet (Scheiner's hypothesis)? Let us now imagine that the phenomenon of the spots is caused by a transition through the solar disk of objects distant from the surface of the Sun that are about 1/20th of the diameter of the solar disk. The objects in question therefore move on the MNO semicircle (Fig. 2). In this case, sunspots should be collocated in points N and O, and their real distance should correspond to the segment NO. It is easy to find out using a compass that the segment NO is shorter than the HL one. Nevertheless, even in this case, the observed distance corresponds to the FI segment because the geometric construction represents the way in which the mind and the eye of the terrestrial observer builds images of the spots. How to decide which of the two situations, clearly different, reflects the real situation? To put it briefly: where are sunspots – on the MNO or on the CDE semicircle?

To answer this question, Galileo used a simple and ingenious method, inspired by the observation record of 05 July 1613. Let the observer execute the drawing that represents this observation and its geometrical reconstruction on the same scale; that is, let the observer trace the circle that represents the solar disk with the same opening of the compass for both the geometric reconstruction (Fig. 2) and the recording of the observations (Fig. 3 and 4). In these circumstances, the distance between spots A and B on 05 July directly gives the length of the chord that corresponds to the real distance between the sunspots in the scale adopted for the drawing.

Therefore, should the spots be on the surface of the Sun, it would correspond to the HL segment; otherwise, it would correspond to the NO segment. A graphical manipulation shown on Fig 2 and 5 clearly indicates that the AB distance observed on July 1 corresponds to the length of the FI segment. At the same time, reiterating the same operation for Fig 2 and 4, and thus arriving at Fig. 6, it becomes clear that the distance between spots A and B on 5 July 1613 are equal to the length of the HL segment, which is significantly greater than the length of the NO segment. This confirms Galileo's hypothesis.

Galileo must have had this procedure in mind. In fact, in the *Istoria e dimostrazioni…* he wrote, "now if one looks at the illustration of the fifth day [Fig. 4], […] one will find that [the] distance [of the spots] A and B would be exactly equal to the chord HL. This can in no way happen if their revolution takes place along a circle at any distance whatsoever from the surface of the Sun" (see OG V, 122.34-36; Galilei, 2010, p.115). Then, he proposed the reasoning summarised above, calculating subsequently the numerical values of the difference between chords in question (see OG V, 123).

3. The Practical Method and its Application

The Practical Method is a brilliant argument that makes it possible to distinguish between two hypotheses on the location of sunspots based on observations using a simple and immediate measurement based on a properly scaled geometric model. The procedure seems to have innovative features. On the one hand, Galileo built a geometric model of the phenomenon, but on the other hand, the model itself acts as a sort of measuring instrument that allows one to resolve the dispute between two hypotheses with reference to the observations. It can therefore be said that the graphical reasoning based on the geometrical model of the event (Fig. 6) permits one to assign a numerical value to a physical quantity (a distance between sunspots) and solve the problem.

A *sine qua non* condition for such a reasoning to work is to assign the same value to the diameter of the solar disk both in the construction of the geometric model and for the drawings representing the observations. Galilei followed precisely this method. In fact, I have analysed a copy of Galileo's treatise on sunspots from the Early Printed Books Collection of the Jagiellonian Library in Cracow (593892 II). Here are the results of my measurements performed on the drawing that corresponds to Fig. 2 (accuracy of ± 1 mm.):

HL = 60mm / ON= 52mm / GM = 69mm / FI = 42mm /
CE = 123mm / CG = 62mm

In the same volume, the distance between the sunspots AB is, respectively, 42 mm (observation on 1 July: the diameter of the solar disk is 123 mm) and 61 mm (observation on 5 July: the diameter of the solar disk is 122 mm).

Therefore, my measurements within the margin of errors seem to indicate that the length of the HL segment is equal to the distance between the AB spots on 5 July, which in principle solves the ongoing dispute between Galileo and Scheiner. Of course, things are not that simple because, first, the spots are not exactly point like, and second, they change shape continuously, moving with their own motion.

In the *Istoria e dimostrazioni…* Galileo applied the Practical Method to other situations, using it as a starting point for the prediction of the probable results of some possible sunspots observations (see OG V, 124.13-21; Galilei, 2010, p.116). Suppose, for example, that spot A is collocated at the edge of the solar disk in point C (Fig. 2), and spot B in point H; then, the arc CGH would be equal to 4°. Assuming that the GC radius is of 10000 units and the GM radius is of 10100, then the lengths of both the CH, and the RN segments can be easily calculated; the CH segment would be of 419 units, and the RN of 94 units. The effect would then be extremely strong (CH/RN = 4.46 versus HL/ON = 1.12 in Fig. 6). Unfortunately, Galileo did not find any observations corresponding to the situation in question.

4. The Practical Method – Methodological Aspects

How to interpret Galileo's reasoning? The Practical Method has some characteristics of what is defined in the literature as an epistemic image. Such visual structures usually serve as a starting point for the formulation and development of a given theory. The Feynman diagrams are often indicated as an example of this.

Galileo began with some hypotheses that simplified the description of the observational situation (e.g. the parallel rays that reach the observer) and then constructed a geometric model that, through some mathematical calculations, permits one to depict the evolution of the phenomenon. The model permits the prediction of the results of the observations. Galileo's two-dimensional model becomes, in a sense, a sort of measuring instrument that allows one to compare directly the two hypotheses by referring them to the observations, solving in this manner a theoretical problem.

Thus, it seems reasonable to state that the Practical Method is a reasoning on which is based the argument in favour of Galileo's thesis about the location of sunspots. Briefly, the Practical Method is a graphic solution of an analysed problem. At the beginning of his scientific career, Galileo, in the context of his studies on the centre of gravity of bodies, emphasised in his discussion with Clavius the importance of drawing in investigation procedures. Lodovico Cigoli, in a letter to Galileo dated 11 August 1611, wrote that a "mathematician, even the greatest, without the help of detailed sketch is only half a mathematician - more, a man without eyes" (OG XI, 168).

Galileo's Practical Method thus offers a sort of geometrical reasoning on behalf of his thesis about the location of the spots. From the mathematical point of view, he constructed a univocal (isomorphic) model of some aspects of the phenomena that occur on the surface of a sphere. The accuracy of the thesis can be confirmed after having assigned a numerical value to the crucial physical quantities that assume different values in different scenarios (Galileo's *vs* Scheiner's hypothesis). Here, with great clarity, one can see the connection between geometry and observations inside a specific model. The Galileo's Practical Method is therefore an example of model-based reasoning, which is not the reasoning referred to the figures of syllogism in Aristotelian terms, but – in this case – to the geometric considerations developed inside the model of a specific phenomenon.

5. Conclusion – Galileo's Practical Method as a Mental Model

The Practical Method of Galileo assuredly is not an example, or perfect description of what later was called the Galilean method; the method in which insight into the universe is "gained through a self-reinforcing loop between experimental data and theoretical analysis, based on the use of mathematics and modelling" (Succi and Coveney, 2019, p.2). Nevertheless, it contains very valuable hints on how some natural problems can be solved.

Galileo's protocol is based on a geometric model, which, starting from simplifying hypotheses, develops an isomorphic geometric structure of the evolution of a given phenomenon. It allows, as a consequence of some geometrical reasoning, a direct comparison between the model and the observations, thus providing graphic reasoning on behalf of a specific thesis as far as a particular aspect of the phenomenon in question (the location of sunspots) is considered. From that point of view Galileo's procedure seems to be very similar to what is now called "mental models", considered together with "mental/thought experiments"[3], a fundamental evolutionary achievement of the human race (see Nersessian, 1992; 2008; Stuart, Fehige and Brown, 2018).

The notion of mental models dates back to the ideas formulated by Charles Peirce and Ludwig Wittgenstein. Kenneth Craik developed the concept of mental models in a remarkably stimulating way. His theory identified the ability to predict events as a fundamental property of human thought and as a particularly advantageous adaptive conquest. Regarding mental models, Craik emphasised their ability to reflect the processes of the real world, both in terms of structure and in the processes that occur there:

> By a model we thus mean any physical or chemical system which has a similar relation-structure to that of the process it imitates. By 'relation-structure' I do not mean some obscure non-physical entity which attends the model, but the fact that

[3] The bibliography of thought experiments in Galileo is very extensive, e.g. (Camilleri, 2015; Palmerino, 2018; Palmieri, 2003).

> it is a physical working model which works in the same way as the process it parallels, in the aspects under consideration at any moment (Craik, 1943, p.51).

In short, mental models have a structure similar to the structure of a "fragment" of the reality they represent.

According to Craik, the construction of mental models belongs to the natural capacities of the human mind. The mental process that constitutes a model is based on three fundamental processes: translation of the processes of the external world into words, numbers, or symbols; reasoning, that is, the transition to other symbols through deduction, induction, etc.; and finally, re-translation of these symbols into external processes or the recognition of correspondence between symbols and external processes.

Since the 1980s, the concept of mental models has been an object of intense research in the fields of philosophy, literary criticism, computer science, and cognitive science. According to interpretations that follow Craik's approach, especially those of Nancy Nersessian, mental models are the constructs of the human mind that represent situations, events, and processes designed to solve certain problems. These models can be manipulated, transformed, and dynamically developed through modes of reasoning that are quite different with respect to the forms of reasoning applied to systems of propositions, such as deduction or induction. In short, there is no strict equivalence between reasoning and logic, because in the case of mental models, 'inferences are made through constructing and manipulating models that are structural, behavioral or functional analog models of target phenomena' (Nersessian, 2008, p.184). Mental experiments are often referred to as an example of this type of "manipulation" understood here as a form of reasoning. Consequently "thought experimenting is a form of 'simulative model-based reasoning'" (Nersessian, 1992, p.291).

Galileo's Practical Method (OG V, 121-122), developed as a measurement tool to be used in the specific dispute on sunspots, as illustrated above using an example of the drawings from the Jagiellonian Edition, seems to be quite near to the aforementioned description of mental models. In that sense, his solution to the problem of the location of sunspots can be considered an example of the construction and the use of mental models. Such models are present in other discoveries of Galileo, and in the history of physics in the post-Galilean era. However, detailed analysis and investigation of this topic must be left for future research.

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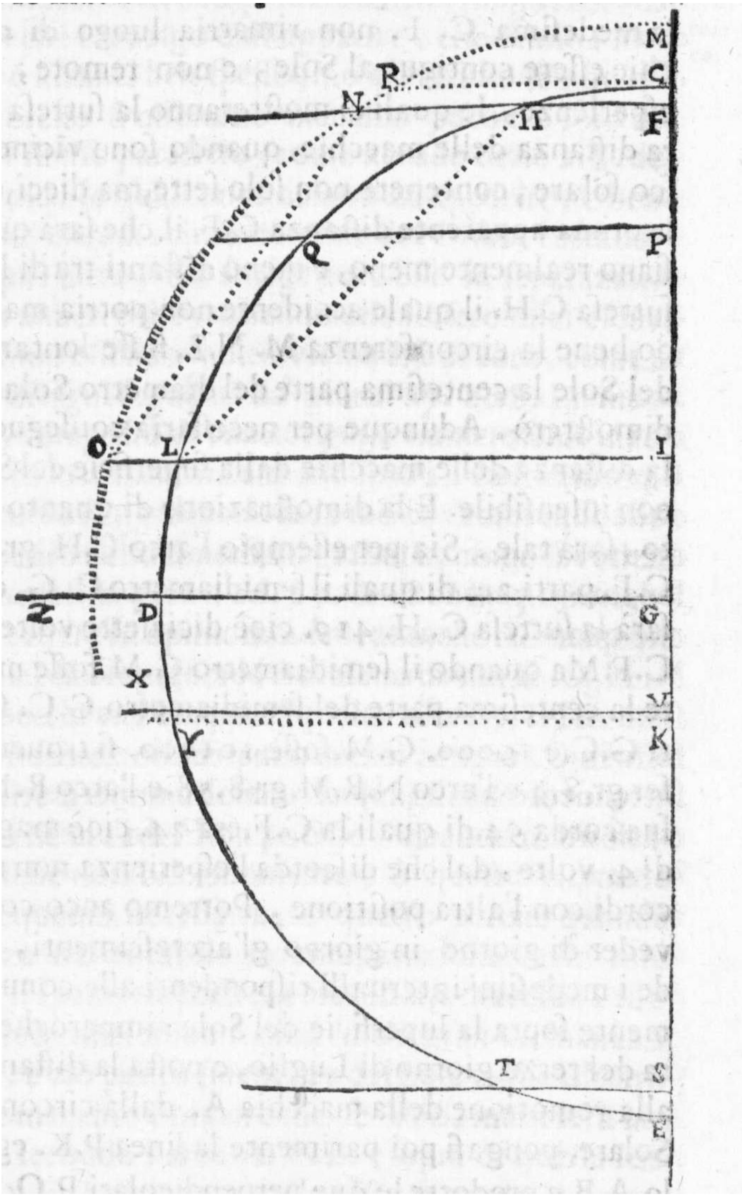

Figure 2: Galileo's Practical Method – the projection on the observational plane of the segment between two sunspots.

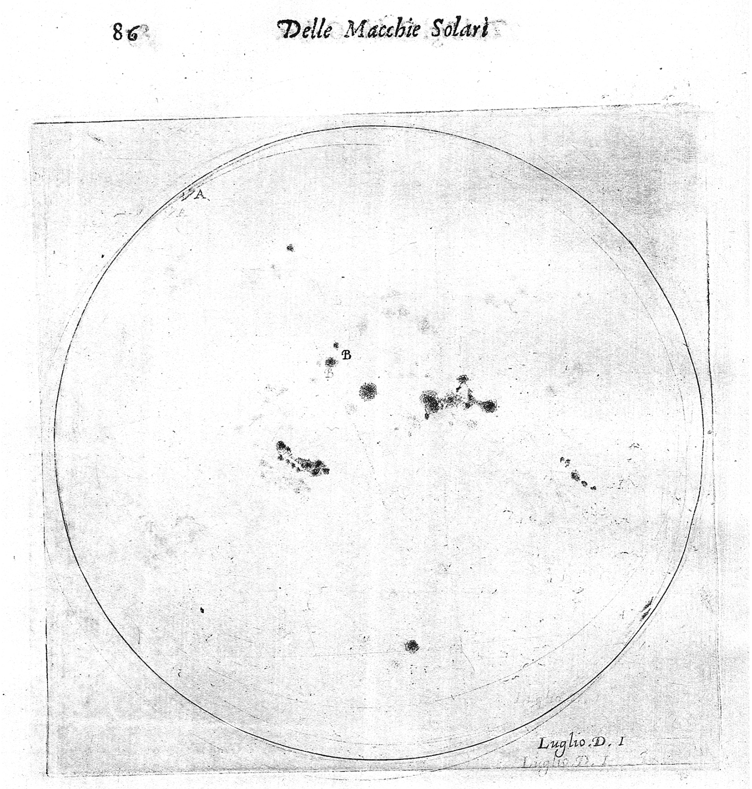

Figure 3: Galileo's observation on the 1st of July 1613.

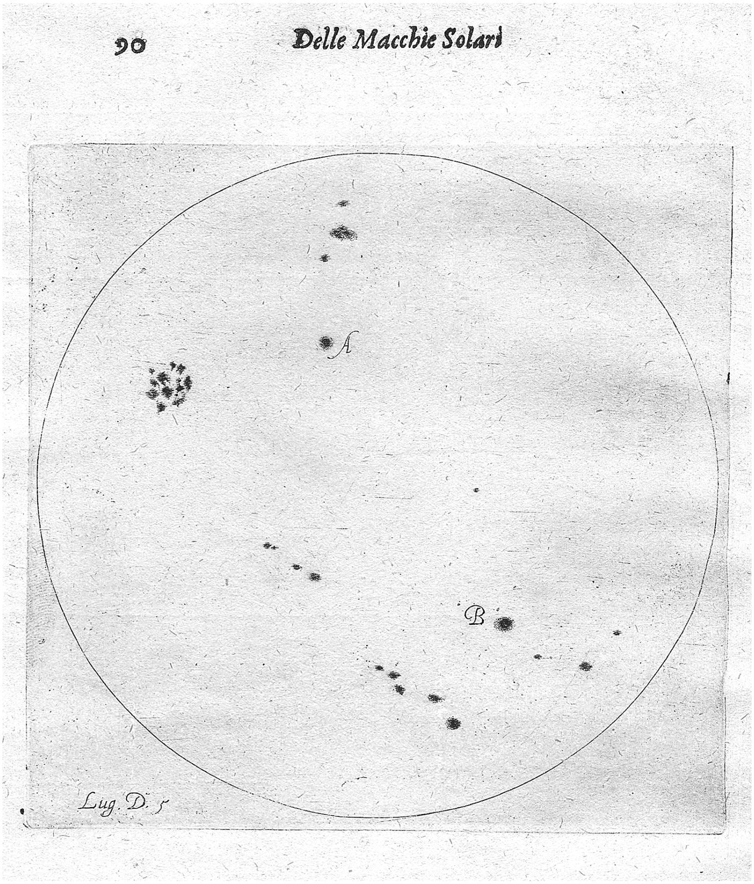

Figure 4: Galileo's observation on the 5th of July 1613.

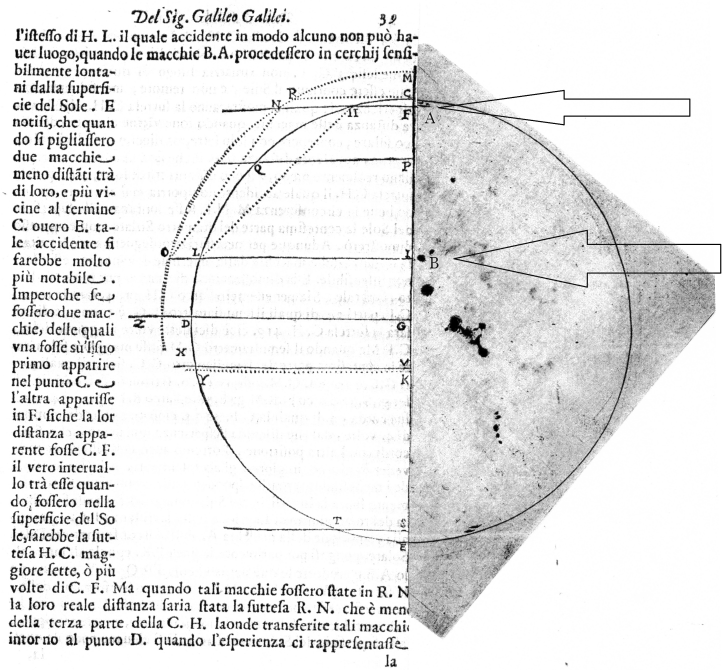

Figure 5: Sunspots observation, and its geometrical model for the 1st of July 1613. The reconstruction by the Author.

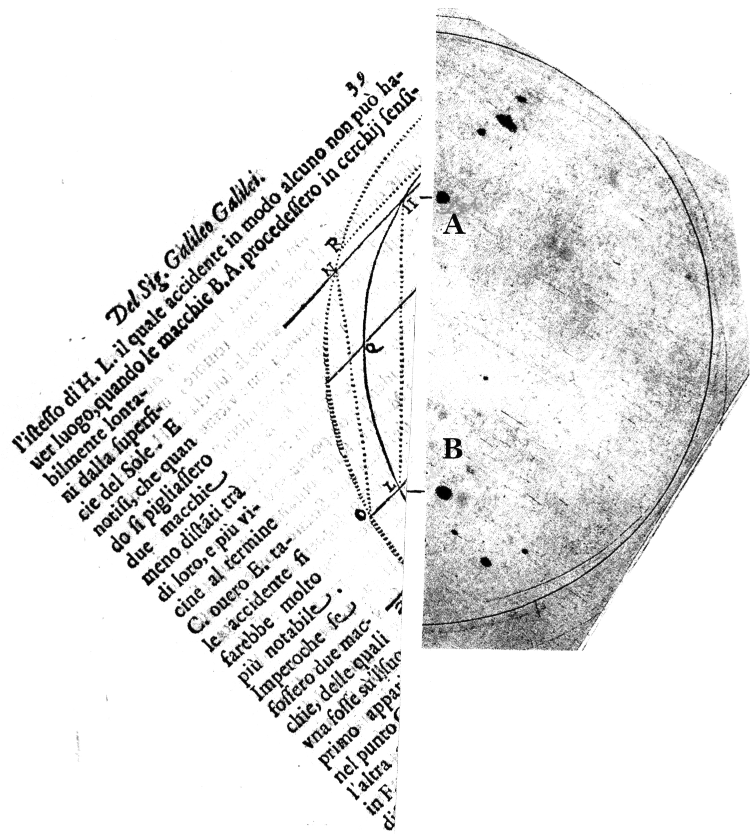

Figure 6: Sunspots observation, and its geometrical model for the 5th July. The reconstruction by the Author.

Nauka jako racjonalna *doxa*. Józefa Życińskiego koncepcja nauki i filozofii nauki – poza internalizmem i eksternalizmem

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Science as rational *doxa*. J. Życiński's understanding of science and philosophy of science – Beyond internalism and externalism

Abstract

Philosophical interests of Joseph Życiński (1948-2011) in the domain of the philosophy of science were focused on the debate concerning the nature of science and philosophy of science that followed the Einstein-Planck revolution in science. The unexpected discovery of the philosophical, extra-scientific presuppositions in science, as well as of the extra-rational factors determining the way these presuppositions are accepted in science were to be explained within the meta-scientific framework. It is the aim of this paper to present Życiński's diagnosis of this post-revolutionary situation in the philosophy of science as well as his critique of the metascientific answers to this challenge. The reasons will be given why all those answers are put under two dichotomous rubrics of *internalism* and *externalism*. It will be also explained how Życiński intends to supersede this false in his opinion opposition with a new concept of doxatic rationality. However the details of the metascientific proposal of Życiński will

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be given only in the subsequent paper. In order to perform the aim of the paper the metatheoretic tools set out by Popper (1979) will be used.

Keywords

rationalism, skepticism, internalism, externalism, scientific revolution, metascientific revolution, philosophical presumptions in science, commitment to the research tradition.

Wstęp

W przekonaniu Życińskiego dwudziestowieczny spór filozoficzny o charakter nauki należy interpretować jako kolejny, historyczny przejaw bardziej fundamentalnego sporu między racjonalizmem a sceptycyzmem o racjonalność poznania.

W dwudziestowiecznej tradycji filozoficznej spór o rozumienie tego, czym jest *nauka*, wiąże się ściśle ze sporem *o racjonalność* rozwoju wiedzy naukowej. W swej najbardziej radykalnej wersji – wersji „zewnętrznej" – jest to spór racjonalizmu ze sceptycyzmem o *istnienie* względnie *nieistnienie* specyficznego, „racjonalnego elementu" poznania, różnego od zmysłowego postrzeżenia, czyli tak czy inaczej rozumianego *rozumu*. W swej wersji „wewnętrznej" – to znaczy wewnątrz tradycji racjonalistycznej, uznającej istnienie *elementu racjonalnego* – jest to spór o charakter tego elementu oraz o jego miejsce w procesie zmiany teoretycznej[1].

[1] Na zasadzie *pars pro toto* Życiński (1983, s. 184) utożsamia naukę z „rozumem naukowym" a filozofię nauki określa na sposób Kanta mianem *Krytyki rozumu naukowego*. Ponieważ w celu metanaukowej analizy rzeczywistej nauki tego typu em-

W diagnozie Życińskiego dwudziestowieczny spór o *naukę* jest sporem fundamentalnym, sporem o rozumienie racjonalności jako takiej, i to zarówno w jego wersji „zewnętrznej", jak i „wewnętrznej". W przypadku sporu „zewnętrznego" – i tutaj tkwi *proprium* rozwiązania Życińskiego – obie strony sporu, czyli rozwiązania zarówno racjonalistyczne, jak i sceptyczne, mają wspólny rodowód. Łączy je błędna, tradycyjna koncepcja racjonalności, a tym samym błędna koncepcja nauki. W tezie tej ujawnia się ogólniejsza presupozycja metafilozoficzna postulująca relatywny charakter sceptycyzmu filozoficznego. Jest to sceptycyzm *względem* określonej koncepcji racjonalności lub naukowości, a nie sceptycyzm *w ogóle*. Jest on specyficzną teorią metanaukową odrzucającą *określoną historycznie* koncepcję naukowości i racjonalności. W przypadku sceptycyzmu dwudziestowiecznego, przedmiotu analiz Życińskiego, negacji podlega koncepcja racjonalności, jaką żywił się dwudziestowieczny racjonalizm metanaukowy, racjonalizm normatywno-demarkacjonistyczny Koła Wiedeńskiego, Poppera i Lakatosa[2].

Motywem, który skłonił Życińskiego do postawienia takiej tezy metafilozoficznej, była konstatacja pewnego istotnego faktu historycznego: to, że oba te przeciwstawne rozwiązania metanaukowe powstały w reakcji na dwudziestowieczną rewolucję naukową, która zanegowała dotychczasowy wzorzec naukowości i racjonalności wywołując efekt „intelektualnego szoku" i prowadząc do „rewolucji metanaukowej". Ale konstatacja faktu z historii nauki jest jednocześnie interpretacją sytuacji filozoficznej w perspektywie metanaukowego

blematyczne określenie jest mało przydatne ze względu na swą nadmierną ogólność, Życiński wprowadza bardziej realistyczną kategorię ‘elementu racjonalnego' (Życiński, 1983, s. 142n).

[2] Zob. (Życiński, 1996, s. 228; 1983, s. 186). W tym drugim tekście jest mowa o porzuceniu ostrej dychotomii kontekstu odkrycia i uzasadnienia typowej dla demarkacjonistycznych metodologii.

sporu o racjonalność naukową. Jest to zatem „konstrukcja" specyficznego faktu metanaukowego, w którym już zawiera się *implicite* możliwe rozwiązanie spornego problemu filozoficznego[3]. Rozwiązanie proponowane przez Życińskiego jest bezpośrednią konsekwencją przyjętej diagnozy metafilozoficznej: przezwyciężenie problemów związanych z tradycyjną koncepcją racjonalności powinno skutkować przezwyciężeniem współczesnego sporu racjonalizmu ze sceptycyzmem. Konieczna jest zatem istotna modyfikacja tradycyjnego rozumienia racjonalności i naukowości. Taka, która rozwiąże problemy nierozwiązywalne na gruncie koncepcji tradycyjnej a będące bezpośrednim powodem pojawienia się nowej wersji sceptycyzmu. W ujęciu Życińskiego problemy te sprowadzają się do wspólnego mianownika, jakim jest problem pogodzenia racjonalności naukowej z występowaniem w nauce i w jej rozwoju *elementów pozanaukowych* i *czynników pozaracjonalnych*.

Celem niniejszego artykułu jest analiza wewnętrznej logiki filozofii nauki Życińskiego, jaka prowadzi go od konstatacji faktów metanaukowych do nowego metateoretycznego rozwiązania kwestii racjonalności naukowej. Panuje dość powszechna opinia wśród czytelników prac Józefa Życińskiego, że są one trudne w percepcji ze względu na swą zawiłość, wielowątkowość, a przede wszystkim ze względu na specyficzny, retoryczny styl. W jego tekstach, zwłaszcza pozycjach książkowych, roi się od przykładów, polemik i dygresji. Analizy o wysokim stopniu abstrakcji i metateoretycznej precyzji przeplatają się z argumentami retorycznymi a subtelny język analiz miesza się z literackim językiem ciętej polemiki. Lekturze jego prac towarzyszy jednak trudna do wyartykułowania intuicja zasadniczej

[3] Życiński nie wierzył oczywiście w istnienie czystych faktów. Zob. niżej przypis 12. W tym wypadku ma swoje zastosowanie teza Lakatosa o zaślubinach filozofii nauki z historią nauki (zob. Życiński, 1983, s. 121n; 1988b, s. 26; 2013, s. 47).

poprawności jego argumentacji. To właśnie ta intuicja stanowiła dla mnie motywację poszukiwania teoretycznej spójności w filozofii nauki Życińskiego.

Metoda poszukiwania wewnętrznej logiki i ukrytych przedzałożeń jest w dużej mierze metodą racjonalnej rekonstrukcji. Wszelkie próby nazbyt deskryptywnego podejścia do filozofii nauki Życińskiego musiałyby ponieść fiasko ze względu na wspomniany charakter jego prac. Element deskryptywny musi zostać zracjonalizowany za pomocą choćby tymczasowej metateoretycznej perspektywy interpretacyjnej. Najbardziej zasadne wydaje się wybranie perspektywy wyznaczanej przez tradycję epistemologiczną, z jaką Życiński wiąże swoje filozofowanie o nauce. Pomimo wszystkich jego krytycznych uwag i zastrzeżeń, jest to szeroko rozumiana racjonalistyczna tradycja *episteme* (por. *Elementy*, s. 126) w jej dwudziestowiecznej wersji zapoczątkowanej przez logiczne badania nad językiem, matematyką i samą logiką oraz przez logiczno-metodologiczne badania nad nauką i jej rozwojem w ujęciu Poppera i Lakatosa. Tradycja ta dostarczy odpowiednich kategorii, przy pomocy których będę mógł rozpocząć analizę tekstów Życińskiego.

Ogólny, racjonalno-rekonstrukcyjny cel mojego tekstu usprawiedliwia ograniczenie zakresu podejmowanych w niniejszym tekście analiz i wykorzystywanych tekstów źródłowych. Ponieważ nie jest moim celem szczegółowa prezentacja wszystkich wątków i wszystkich możliwych niuansów proponowanych przez Życińskiego rozwiązań metanaukowych, dlatego w artykule wykorzystane zostaną jedynie jego książki z zakresu filozofii nauki z pominięciem licznych, szczegółowych artykułów[4]. Ze względu na obszerność ko-

[4] Są to następujące prace: *Język i metoda* (1983), *Teizm i filozofia analityczna*, tom 1 (1985), *Structure of the Metascientific Revolution* (1988b), *Granice racjonalności* (1993) oraz *Elementy filozofii nauki* (1996). Wiele wątków powtarza się przez wszyst-

niecznych analiz zostały one podzielone na dwie odrębne części[5]. Część pierwsza – zawarta w niniejszym artykule – obejmuje sobą najogólniejsze, metateoretyczne analizy koncepcji Życińskiego filozofii nauki. Część druga zawierać będzie szczegółowe rozwiązania metanaukowe zaproponowane przez Życińskiego w celu wypracowania nowego rozumienia nauki i racjonalności. Są to rozwiązania najczęściej kojarzone z jego nazwiskiem, takie jak zasada aracjonalności, zasada epistemologicznej niepewności, naturalności interdyscyplinarnej, czy też kwestia różnych typów racjonalności i ewolucji pojęcia racjonalności.

1. Perspektywa metateoretyczna

Nowego rozumienia racjonalności Życiński nie poszukuje na drodze apriorycznych analiz językowo-logicznych, lecz przez pełniej-

kie prace. Różni je bardziej sposób prezentacji niż charakter proponowanych rozwiązań. Zauważalna jest zasadnicza ciągłość w myśli metanaukowej Życińskiego, począwszy od pierwszej pracy. Tylko w wybranych przypadkach wskażę pewną ewolucję myśli Życińskiego, inne pominę, jako nieistotne dla celu mojej pracy. Najbardziej rozwinięte analizy metafilozoficzne Życińskiego można odnaleźć w jego *Strukturze rewolucji metanaukowej* (1988b; 2013). Tutaj pojawiają się istotne z punktu widzenia rozumienia racjonalności nauki koncepcje *ideatów* oraz *ideologicznych programów badawczych*. Pisząc ten tekst korzystałem z oryginału angielskiego (Życiński, 1988b). Tekst ten został przetłumaczony na język polski już po śmierci J. Życińskiego w 2013 r. Cytując fragmenty tej publikacji korzystam z przekładu M. Furmana dokonując wszakże pewnych modyfikacji w celu dostosowania terminologii do terminologii niniejszego artykułu. Odnośniki do stron podaję zarówno dla tekstu angielskiego, jak i dla polskiego przekładu. Istnieje również drugie, poszerzone wydanie *Elementów* filozofii nauki (Życiński, 2015).

Czytelnikowi, który chciałby poznać najważniejsze myśli całego dorobku Józefa Życińskiego, a nie tylko z zakresu filozofii nauki, polecam przeglądowy artykuł Michała Hellera (2011). Metanaukowe poglądy Życińskiego omawiam również w (Liana, 2016).

[5] Podział ten dokonany został na prośbę Redakcji.

sze uwzględnienie faktów z historii nauki. Jego podstawowy postulat metafilozoficzny głosi, że filozofia nauki powinna zrezygnować z uproszczonych i wyidealizowanych koncepcji nauki i wypracowywać koncepcje bardziej realistyczne uwzględniające w większym stopniu analizę rzeczywistych zachowań naukowców. Tego typu deklaracja metaepistemologiczna stanowi wyraz określonych przekonań ontologicznych i metametodologicznych Życińskiego. Element racjonalny ma charakter *obiektywny*, jest obecny w nauce realnie, aczkolwiek nierzadko *implicite*. Racjonalność obiektywna ujawnia swój charakter stopniowo w meandrach historii nauki. Filozofowi nie pozostaje nic innego, jak uważnie obserwować tę historię i próbować *odkrywać* racjonalność obiektywną za pomocą hipotez metanaukowych i testować te ostatnie w oparciu o faktyczne i faktualne przejawy racjonalności w dziejach nauki. Widać z tego, że pod względem metodologicznym filozofia nauki Życińskiego bliższa jest nauce empirycznej niż analitycznej. Pod tym względem Życiński staje w jednej, *transcendentalnej* tradycji metafilozoficznej obok Poppera i Lakatosa[6].

Swoje koncepcje metafilozoficzne, w tym koncepcję metody transcendentalnej, Popper przedstawił najpełniej w *Die beiden Grundprobleme der Erkenntnistheorie* (1979). To właśnie tam broni tezy, że metodologia jest specyficzną metanauką posługującą się specyficzną metodą transcendentalną[7]. Wprawdzie nazwa metody zaczerpnięta jest od Kanta, to jednak jej rozumienie różni się znacząco od znaczenia, jakie temu wyrażeniu nadawał Kant. Nie jest to metoda dedukcji transcendentalnej, lecz, jak podkreśla Popper, metoda

[6] Życiński poddaje jednak tę metodę reinterpretacji, gdyż inaczej rozumie on *rzeczywistość* nauki niż Popper czy Lakatos. Kwestia ta zostanie przedstawiona pod koniec artykułu.

[7] Koncepcję Poppera filozofii jako metanauki omawiam szczegółowo w (Liana, 2006).

analogiczna do metody empirycznej. Także w przypadku metodologii mamy do czynienia z określonymi faktami filozoficznymi, które wymagają filozoficznego wyjaśnienia. Wyjaśnienia filozoficzne są wartościowe o tyle, o ile wytrzymują konfrontację z faktami filozoficznymi. W pracy tej Popper nie nazywa wyjaśnień metodologicznych hipotezami, a ich obalenia przez fakty metodologiczne falsyfikacją. Oba te wyrażenia rezerwuje wyłącznie dla nauk empirycznych. W metodologii i w metodzie transcedentalnej mamy do czynienia z tak zwaną *transcedentalną sprzecznością*, analogiczną do empirycznej falsyfikacji. Metoda ta została zmodyfikowana przez Lakatosa i poszerzona o wykorzystanie faktów z historii nauki w celu quasi-empirycznej konfrontacji rozwiązań metanaukowych. Życiński przyjmuje w filozofii identyczną transcendentalną metodę, z tym że terminy 'falsyfikacja' i 'hipoteza' odnosi on bez żadnego rozróżnienia zarówno do metody nauk empirycznych, jak i do metody metanauki lub szerzej filozofii nauki[8].

Należy też zauważyć, że Życiński rozróżnia wąsko rozumianą metanaukę od szerszej filozofii nauki (Życiński, 1996, s. 14–16). Metanauka bada kwestie logiczne, metodologiczne i epistemologiczne w nauce. Filozofia nauki zajmuje się z kolei wypracowaniem całościowej wizji nauki w jej ujęciu diachronicznym, czyli strukturą rewolucji naukowych, związkami między czynnikami racjonalnymi i socjologicznymi, ewolucją pojęcia racjonalności. Nie należy ich wszakże sobie przeciwstawiać, gdyż obie zajmują się „tą samą rze-

[8] W jego tekstach można znaleźć wiele wypowiedzi o *falsyfikacji* koncepcji filozoficznych (Życiński, 1985, s. 177; 1988b, s. 9, 12, 97, 135; 2013, s. 17, 22, 174, 239). W *Elementach* (1996, s. 166) mówi o wymogu *falsyfikowalności* rozwiązań metanaukowych. Życiński odróżnia falsyfikowalne hipotezy metanaukowe od niefalsyfikowalnych idei metafizycznych. Za Popperem uznaje metafizykę za *zasadniczo niefalsyfikowalną* (Życiński, 1996, s. 130).

czywistością nauki” a granice między nimi są do pewnego stopnia *rozmyte* (Życiński, 1996, s. 16). Metoda transcedentalnej falsyfikacji stosuje się do nich obu.

Z perspektywy analizy koncepcji Życińskiego szczególnie przydatne wydaje się wprowadzone przez Poppera pojęcie *faktu metodologicznego* lub *epistemologicznego*, czyli *faktu metanaukowego*[9]. W kontekście filozofii Życińskiego można pojęcie to rozszerzyć do pojęcia *faktu filozoficznego*. Pojęcia te, jakkolwiek budzą w niektórych kręgach filozoficznych sprzeciw i opór, to jednak w perspektywie współczesnej filozofii nauki odrzucającej „zgodnie” – podobnie jak Popper – istnienie *czystych faktów*, są one jak najbardziej na miejscu. Skoro fakt rozumiany jako pewne zdanie bazowe nauki lub metanauki jest w sposób nieunikniony teoretyczną interpretacją obserwacji[10], to staje się on pojęciem analogicznym. W zależności od typu interpretacji mamy do czynienia z faktem empirycznym, filozoficznym, naukowym lub metanaukowym, etc. W „socjologicznej” koncepcji faktu wprowadzonej przez Poppera[11] stopień obiektywności faktu mierzy się stopniem jego intersubiektywności. Z tego względu

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[9] Dla ułatwienia lektury w całym tekście treść idei, podobnie jak treść pojęć i koncepcji, piszę zazwyczaj kursywą. Kursywa służy również do podkreślenia istotnych elementów znaczeniowych.

[10] O ile w czasach Poppera i Koła Wiedeńskiego teza ta była przedmiotem sporu, o tyle w czasach Życińskiego była ona już podstawowym i „niekontrowersyjnym” założeniem filozofii nauki. Życiński uznaje ten fakt to za skutek rewolucji metanaukowej (zob. Życiński, 1996, s. 127).

[11] Termin ‘socjologiczna’ oznacza według Poppera konieczność intersubiektywnej zgody. Warto zauważyć, że według niego chodzi o zgodę „negatywną”, a nie pozytywną. Ta ostatnia implikowałaby swoisty indukcjonizm, który, jak wiadomo, jest dla Poppera niemożliwy do zaakceptowania z racji logicznych. Zgodę faktualną uznaje się za obowiązującą wtedy, gdy nikt *kompetentny* (*sic!*) nie wyraża sprzeciwu, i to tylko tymczasowo, do chwili, gdy pojawi się ktoś *kompetentny* zgłaszający sprzeciw. Taki sprzeciw zmusza do rewizji zdań faktualnych. Najpełniejsze przedstawienie „negatywnej” koncepcji *faktu* można odnaleźć w pracy Poppera (1979, s. 122–135, zwł. 131n.)

fakty filozoficzne cechują się znacznie mniejszym stopniem obiektywności niż fakty empiryczne i w konsekwencji możliwość obalenia koncepcji metanaukowych przez fakty filozoficzne jest dużo mniejsza niż w możliwość falsyfikacji hipotez empirycznych. O ile Popper już w *Die beiden Grundprobleme* zaczął wątpić w możliwość rozstrzygającego obalenia na gruncie metodologii, o tyle u Życińskiego niełatwo znaleźć artykulacje podobnych wątpliwości[12].

2. Rewolucja naukowa a rewolucja metanaukowa – narzędzia metateoretycznej analizy

Punktem wyjścia dla Życińskiego do poszukiwania nowych rozwiązań metanaukowych i z zakresu filozofii nauki jest swoista metahistoryczna i metanaukowa teza. W filozofii nauki doszło do *metanaukowej rewolucji*, która została wywołana przez wcześniejszą rewolucję naukową[13]. Jedna i druga były równie gwałtowne, powiązane z doświadczeniem rzeczonego intelektualnego szoku. W celu rekonstrukcji logiki ukrytej w rozwiązaniu Życińskiego konieczne wydaje

[12] Mówi on o falsyfikacji' *tout court* metanaukowej koncepcji teorii nauki obiektywnej (Życiński, 1988b, s. 9; 2013, s. 17), albo o *praktycznej* falsyfikacji intuicjonizmu w matematyce (Życiński, 1988b, s. 97; 2013, s. 174). Jednocześnie jednak Życiński (1988b, s. 140; 2013, s. 248) krytykuje Poppera i jego tezę o możliwości obiektywnej oceny wartości schematów pojęciowo-metodologicznych. W świetle *Die beiden Grundprobleme* nie wydaje się, by teza ta faktycznie była głoszona przez Poppera, a po drugie, jej krytyka wydaje się niezbyt spójna z niekrytycznym użyciem przez Życińskiego terminu 'falsyfikacja'.

[13] Termin 'rewolucja metanaukowa' pojawia się już w (Życiński, 1983, s. 99) w tytule drugiej części dzieła: „Struktura rewolucji metanaukowych". Ten sam tytuł nosi angielska książka Życińskiego (1988b) będąca rozwinięciem idei zawartych w *Języku i metodzie*. Życiński mówi o odstępie „półwiecza" między rewolucją naukową a rewolucją metanaukową. Wyrażenie pojawia się w (Życiński, 1983, s. 101) i zostaje powtórzone w (Życiński, 1996, s. 126).

się poddanie analizie specyficznej relacji, jaka zachodzi w jego koncepcji między dwoma faktami: między faktem rewolucji naukowej a faktem rewolucji metanaukowej.

Język, jakim Życiński operuje w kontekście omawiania relacji między rewolucją metanaukową a rewolucją naukową, jest zarówno językiem logiki, jak i językiem psychologii. Jest językiem logiki, gdyż mówi *implikowaniu* rewolucji metanaukowej przez rewolucję naukową oraz o metanaukowych *konsekwencjach* rewolucji naukowej (Życiński, 1988b, s. 8.13; 2013, s. 15.24)[14]. Język ten sugeruje, iż Życiński postuluje istnienie silnych związków merytoryczno-logicznych pomiędzy tymi rewolucjami. Teoretyczne rozwiązania zaproponowane w ramach rewolucji metanaukowej nie miały zatem genezy czysto apriorycznej, lecz były w dużej mierze zdeterminowane rzeczywistą nauką. Ale z drugiej strony Życiński mówi o *szoku* wywołanym przez rewolucję naukową wśród filozofów nauki i o ich reakcji na ten szok. Także sam termin 'rewolucja' użyty przez Życińskiego niesie ze sobą silne konotacje pozalogiczne. Został wprowadzony do filozofii nauki przez Kuhna w celu podkreślenia istotnej roli czynników socjologicznych, psychologicznych i kulturowych w rozwoju nauki. Odrzucając dychotomię skrajnego internalizmu i skrajnego eksternalizmu Życiński odrzuca zarówno koncepcję relacji czysto logicznej jak i koncepcję relację czysto „zewnętrznej", przyczynowej. W konsekwencji przedstawia tę relację dwoiście, zarówno jako związek logiczny, racjonalny, jak i jako związek przyczynowo-skutkowy[15].

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[14] Polski przekład 'rezultat' nie oddaje wiernie angielskiego 'implied'.

[15] Nie jest możliwe przedstawienie koncepcji Życińskiego w sposób liniowy, niejako „cegiełka po cegiełce". Ponieważ to, w jaki sposób dokonuje on metanaukowej interpretacji faktów historycznych i relacji między nimi, jest już warunkowane *implicite* przez jego metanaukowe koncepcje, dlatego nieunikniona jest pewna kolistość prezen-

Życiński podejmuje studium i analizę historii nauki i historii filozofii nauki w celu zidentyfikowania różnego typu faktów metanaukowych, zarówno racjonalnych jak pozaracjonalnych, które pozwolą mu na właściwą interpretację zależności rewolucji metanaukowej od rewolucji naukowej. Z jednej strony poszukuje faktów pozaempirycznych metanaukowych z historii nauki, które pozwolą mu na uchwycenie racjonalnych (logiczno-merytorycznych) relacji zachodzących pomiędzy zmianami na poziomie nauki teoretycznej ze zmianami na poziomie teorii metanaukowych (zmian metateoretycznych). Z drugiej strony poszukuje faktów „psychologicznych" usprawiedliwiających (potwierdzających) użycie psychologicznych kategorii 'rewolucji' i 'intelektualnego szoku'[16] dla opisu stanu umysłu filozofów nauki.

Poniżej przedstawiam w formie metateoretycznych uwag wyjaśnienie różnych typów faktu metanaukowego. Uwagi te są przydatne do lepszego zrozumienia dalszych analiz, ale nie są konieczne. Jako bardziej abstrakcyjne i niekonieczne do dalszej lektury zostały wydzielone z całości tekstu.[17]

Uwaga 1. Fakty metanaukowe to quasi-empiryczne fakty z historii nauki będące przedmiotem wyjaśnień metanaukowych. Idea wyróżnienia faktów

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tacji i przyjęcie na początku pojęć, które będą wyjaśniane dopiero z czasem. Pojęcia *internalizmu* i *eksternalizmu* zostaną przedstawione w punkcie 5, a psychologiczne pojęcie *szoku* w punkcie 4b.

[16] Ponieważ pojęcie *kategorii* traktuję jako pojęcie metateoretyczne i metajęzykowe zarazem, dlatego treść kategorii będę zapisywał w cudzysłowie metajęzykowym.

[17] W całym artykule bardziej abstrakcyjne analizy metateoretyczne lub metafilozoficzne przedstawiam w formie oddzielnych uwag. Mam nadzieję, że ułatwi to lekturę tekstu. W obecnym artykule nie podejmuję szczegółowej analizy metody, jaką Życiński posługuje się w filozofii nauki. Byłaby to niewątpliwie niezwykle interesująca strategia poszukiwania ukrytych presupozycji Życińskiego na temat racjonalności metodologicznej, ale zaciemniłoby to dodatkowo i tak stosunkowo skomplikowany tekst artykułu. Dlatego ograniczam się w tym względzie do rzeczonych uwag.

empirycznych od quasi-empirycznych faktów metanaukowych pochodzi od Poppera. Wprawdzie Życiński nie stosuje tego typu terminologii, niemniej mówi o faktach z historii nauki będących przedmiotem wyjaśnień metanaukowych. Odróżnienie Poppera ma charakter demarkacjonistyczny. Na gruncie metodologicznym odróżnia on wyraźnie fakty (zdania) empiryczne od faktów epistemologicznych, czy metodologicznych. Jego teoria *metodologicznych typów* faktów bazuje na specyficznej *zasadzie analogii*: fakty epistemologiczne są analogiczne do faktów empirycznych, podobnie jak metoda filozofii nauki (metoda transcedentalna) jest analogiczna do metody empirycznej. W przypadku Życińskiego, jego teksty sugerują, że operuje on raczej jednoznacznym niż analogicznym pojęciem faktu we wszystkich kontekstach metodologicznych. Brak wystarczających danych, by rozstrzygnąć, czy jest to świadoma i krytyczna postawa metodologiczna, czy jedynie spontaniczna. Z tego względu zastosowaniu terminologii Poppera do interpretacji tekstu Życińskiego musi towarzyszyć daleko posunięta ostrożność hermeneutyczna. Tego typu ostrożna interpretacja terminu ‘fakt metanaukowy’ abstrahuje od meta-metodologicznych idei Poppera i ogranicza znaczenie tego terminu do własności *bycia faktem wyjaśnianym przez metanaukę*.

Uwaga 2. Fakt metanaukowy, analogicznie do faktu empirycznego, stanowi językowe przedstawienie i zarazem teoretyczną interpretację konkretnego, postrzeganego zmysłami „zdarzenia”. W nie-demarkacjonistycznym ujęciu Życińskiego zdarzeniem ujmowanym przez fakt metanaukowy może być zarówno zjawisko fizyczne (np. psychologiczne), jak i zjawisko „językowe”. Takim zjawiskiem językowym jest pojawienie się określonego twierdzenia lub teorii o określonych cechach ontologicznych lub logicznych. Może nim być także zachowanie meta-językowe i zarazem metanaukowe (epistemologiczne, metodologiczne) naukowca: to, co robi on ze swoimi wypowiedziami, by je uzasadnić, obalić, sprawdzić, a także to, do jakich celów poznawczych ich używa.

Ze względu na dwa typy faktów metanaukowych: empiryczne fakty metanaukowe i pozaempiryczne „językowe” fakty metanaukowe, te ostatnie będę nazywał ‘faktami metateoretycznymi’.

W perspektywie metajęzykowej fakt metateoretyczny należy utożsamić ze zdaniem egzystencjalnym orzekającym występowanie *typowych* (powtarzających się) cech metateoretycznych (np.: „Naukowcy dokonują predykcji", „Fakty naukowe są obciążone teoretycznie", „Nauka nie potwierdza ostrego odróżnienia kontekstu odkrycia od kontekstu uzasadnienia"; „Teorie się zmieniają", „Mechanika kwantowa odchodzi od tradycyjnego ideału poznania jednoznacznego", „Twierdzenia limitacyjne metalogiki ukazują granice poznania matematycznego i logicznego", itp.), a konkretne zdarzenia z historii nauki z desygnatami spełniającymi lub nie to zdanie.

Stwierdzenie powyższe pokazuje, że wyrażenia ‘wyrażenie metateoretyczne’ i ‘wyrażenie metajęzykowe’ nie są tożsame. Fakty metateoretyczne można (i należy) traktować jako język przedmiotowy metanauki. Metanauka ma swój własny język przedmiotowy i swój własny metajęzyk, różny od języka i metajęzyka nauk przedmiotowych. Osobnym problemem jest pytanie, czy język przedmiotowy metanauki jest metajęzykiem nauki przedmiotowej i, w konsekwencji, czy metajęzyk metanauki jest meta-metajęzykiem nauki przedmiotowej. Szczegółowa odpowiedź na tak postawione pytanie wykracza poza ramy niniejszego artykułu. Ale w metodologistycznym podejściu Poppera do metanauki zdania metanauki mają za przedmiot wyrażenia metajęzykowe nauki przedmiotowej tylko w ich ujęciu ‘materialnym’ a nie ‘formalnym’. Metanauka tworzy własny język przedmiotowy na bazie metajęzyka nauki przedmiotowej. Ten język przedmiotowy nie utożsamia się z metajęzykiem nauk przedmiotowych, a jedynie powstaje na drodze odpowiedniej interpretacji metodologicznej metajęzykowych (i zarazem metanaukowych) zachowań naukowców (względem faktów, teorii etc.) jako zachowań metateoretycznych lub innych (np. psychologicznych). Nie należy również mylić metanauki jako dyscypliny filozoficznej z metanaukowymi *zachowaniami* naukowców. Te ostanie to te zachowania naukowców, które są przedmiotem metanaukowej interpretacji w filozofii nauki.

Warto przy tym zauważyć, że samo odróżnienie dwóch dziedzin faktów metanaukowych: dziedziny metateoretycznej (historia nauki rozumianej intersubiektywistycznie) i dziedziny empirycznej (świat, w tym także subiektywne reakcje i uwarunkowania naukowców) jest już wyrazem określonej interpretacji metanaukowej (albo jeszcze wyższego rzędu metanaukowego) określonych intuicji poznawczych.

Uwaga 3. Faktów metateoretycznych nie należy utożsamiać z *konkretnymi* przypadkami z historii nauki. Fakty te są przedstawieniem ogólnie ujętych, określonych cech – ontologicznych lub metateoretycznych – teorii naukowych i zachowań epistemologiczno-metodologicznych naukowców. Konkretne przypadki z historii nauki stanowią co najwyżej *ilustrację*, *przykład* lub *potwierdzenie* występowania tego typu cech w nauce. Swego czasu August Comte rozróżnił *fakty jednostkowe* od *faktów ogólnych*. Naukę empiryczną i filozofię o wiele bardziej interesują fakty ogólne – czyli to, co jest powtarzalne – niż konkretne jednostkowe przypadki (zdarzenia). Nawet w procedurze potwierdzenia lub obalenia empirycznego pojedyncze zdarzenia są bezwartościowe metodologicznie. Muszą być powtarzalne i muszą się pewną ilość razy powtórzyć – najlepiej niezależnie – by mogły zostać *zaakceptowane* w sposób intersubiektywny przez wspólnotę badaczy. Jak wiadomo, Popper nazywa te *fakty ogólne* zdaniami bazowymi i hipotezami niskiego rzędu.

Z tego względu fakt metateoretyczny – fakt pozaempiryczny, czyli fakt w perspektywie metody innej niż metoda empiryczna – to sąd egzystencjalny, który dotyczy nauki (względnie metanauki) rozumianej zarówno w jej aspekcie funkcjonalnym, jak i przedmiotowym. W pierwszym przypadku fakt metateoretyczny dotyczy zachowań metodologicznych naukowców (względnie filozofów nauki), w drugim wystąpienia określonych sądów, twierdzeń, teorii, hipotez, idei a także ich własności metateoretycznych (względnie meta-metateoretycznych).

Uwaga 4. Osobną kwestią jest problem zaklasyfikowania faktów psychologicznych odnotowujących reakcje psychologiczne w obliczu nowej nauki. W demarkacjonistycznym ujęciu Poppera należy je uznać za fakty *stricte* empiryczne nie należące do metanauki. W przypadku niedemarkacjonistycznego ujęcia Życińskiego – gdzie element *stricte* racjonalny współwystępuje z, i jest dookreślany przez element przyczynowy – należy przypuszczać, że podział nie jest tak ostry i także fakty psychologiczne z historii nauki są specyficznymi faktami metanaukowymi.

Inna sprawa, że w historii nauki te dwa typy „faktów" nie występują „oddzielnie", lecz tworzą *jeden złożony* fakt metanaukowy: bezpośredni przedmiot obserwacji. Fakt złożony (to znaczy fakt „surowy" – uteoretyzowany w mniejszym stopniu) można „rozłożyć" – to znaczy zinterpretować i wyjaśnić dedukcyjnie – na wspomniane dwa *typy* lub *aspekty* metanaukowe (na fakt empiryczny i na fakt *stricte* metateoretyczny, to znaczy pozaempiryczny) dopiero za pomocą odpowiedniej analizy metanaukowej.

Niezależnie od *faktycznej* struktury faktów metanaukowych i niezależnie od *faktycznego* charakteru metanaukowych faktów psychologicznych, rozróżnienie empirycznych i pozaempirycznych (metateoretycznych) faktów metanaukowych jest pragmatycznie użyteczne. Nie należy traktować tego rozróżnienia skrajnie demarkacjonistycznie, jak chciał Popper, lecz wyłącznie jako pierwsze przybliżenie metanaukowe tego, czym jest fakt metanaukowy. Jego użyciu, w perspektywie metafilozofii Życińskiego, musi towarzyszyć zawsze odpowiednie ograniczenie, że wszelkie demarkacje mają charakter wyłącznie idealizacyjny i nigdy nie stanowią ostatecznego ujęcia rzeczywistości racjonalnej.

Uwaga 5. Życiński nie przeprowadza tego typu analiz metateoretycznych w odniesieniu do stosowanej przez siebie terminologii metanaukowej. W konsekwencji nie rozróżnia on *explicite* pod względem metodologicznym ani faktu naukowego od metanaukowego, ani tym bardziej obu aspektów tego ostatniego. Konkretne znaczenie metateoretyczne, jakie z wiąże z terminem 'fakt', określane jest przez kontekst użycia.

Przyjmując wraz Życińskim, że w rekonstrukcji historii nauki i historii filozofii nauki związki logiczne są ważniejsze od uwarunkowań psycho-społecznych racjonalna rekonstrukcja jego rozumienia relacji między rewolucją naukową a rewolucją metanaukową należy przyjąć, że najważniejsze są w tym wypadku związki pomiędzy specyficznymi faktami metateoretycznymi z dziedziny nauki przedmiotowej (np. teoriami, założeniami) a specyficznymi faktami metateoretycznymi z dziedziny metanauki (np. wyjaśnieniami)[18]. Metanau-

[18] W perspektywie przyjętych rozróżnień między poziomami naukowości i teoretyczności należałoby nazwać te ostatnie fakty *faktami meta-metateoretycznymi*. Ale takie rozróżnianie nic nie wniesie do dalszych analiz poza zbędnym balastem precyzji. Życiński wydaje się nie przywiązywać dużej wagi do tego typu precyzyjnych rozróżnień

kowych faktów psychologicznych w takiej rekonstrukcji nie można pominąć, jeśli nie chcemy przypisać Życińskiemu koncepcji nazbyt uproszczonej i wyidealizowanej, ale też nie mogą być one, jak zobaczymy poniżej, dominujące[19].

Wedle takiej rekonstrukcji Życiński najpierw ustala odpowiednie zbiory faktów metateoretycznych obu typów a następnie poszukuje właściwych relacji merytoryczno-logicznych pomiędzy nimi. Wyznaczenie odpowiednich zbiorów faktów metateoretycznych jest stosunkowo proste. Czerpie je odpowiednio z historii nauki przedmiotowej (empirycznej i formalnej) z jednej strony i z historii metanauki z drugiej. Problematyczny jest natomiast sposób poszukiwania powiązanych merytoryczno-logicznie *uporządkowanych par* faktów metateoretycznych:

<fakt z historii nauki; fakt z historii metanauki>.

W tym celu Życiński wydaje się odwoływać się do idei powiązania faktów psychologicznych z faktami metateoretycznymi w postaci surowych faktów metanaukowych i meta-metanaukowych[20]. Idea ta kieruje *implicite* jego heurystyczną strategią metanaukową: psychologiczna idea *intelektualnego szoku* i kategorii jemu równoważnych nadaje się na stosunkowo łatwe kryterium wyróżniania tych faktów metateoretycznych z dziejów nauki, które doprowadziły do zmian w metanauce, czyli do nowych faktów metateoretycznych w dziejach metanauki. Ponieważ psychologiczny aspekt faktów metanaukowych jest łatwiejszy do konstatacji, zatem może służyć jako wygodne na-

poziomów analiz. W (Życiński, 1983, s. 123) przytacza sceptyczną uwagę J. Watkinsa odnośnie do tego typu rozróżnień u Poppera i Lakatosa, ale nie wydaje na ten temat własnej oceny.

[19] Uzasadnienie tych warunków zostanie przedstawione niżej przy okazji omawiania własnego rozwiązania metanaukowego Życińskiego.

[20] Fakty meta-metanaukowe to fakty tworzące rzeczywistość metanauki jako pewnej działalności poznawczej człowieka.

rzędzie wyróżniania odpowiednich pozaempirycznych faktów metateoretycznych obu typów i ich związków. Z jednej strony należy poszukiwać takich teorii naukowych i takich cech tych teorii, które wywołały szok wśród filozofów nauki, a z drugiej strony takich postaw metanaukowych i teorii metanaukowych, które były odpowiedzią na ów szok. Oczywiście, zgodnie z zasadniczo racjonalistycznym stanowiskiem Życińskiego, warunkowanie psychologicznie w żadnym wypadku nie może wyjaśniać ani zastępować związków logicznych, jakie występują między obu typami faktów metateoretycznych. W tym celu konieczna jest klasyczna analiza znaczeń i struktur logicznych.

Oprócz *szoku intelektualnego* Życiński wskazuje także na inne psychologiczne fakty metanaukowe w kontekście powiązania rewolucji naukowej z metanaukową. Jednym z nich jest odczucie *paradoksalności* nowych teorii. W *Strukturze rewolucji metanaukowej* pisze, że szok intelektualny towarzyszył zarówno odkryciu *paradoksalnych* geometrii nieeuklidesowych, antynomii w podstawach matematyki, jak i *paradoksów* teorii kwantów oraz teorii względności. W okresie rewolucji naukowej doświadczenie paradoksalności nauki było do tego stopnia powszechne, że niektórzy, jak na przykład Niels Bohr, uznali ją za synonim poprawności i prawdziwości rozwiązań teoretycznych[21]. Podobną reakcją psychologiczną było *zaskoczenie* spowodowane odkryciem nieoczekiwanych cech nauki, takich jak istnienie wewnętrznych, niepokonalnych logicznych ograniczeń. I to zarówno w obrębie nauk przedmiotowych (formalnych i empirycznych), jak i w obrębie metanauki: odkrycie niezupełności bogatych systemów logicznych, zasady losowości w fizyce czarnych dziur

[21] Zob. (Życiński, 1988b, s. 9, 132; 2013, s. 16, 233), gdzie mówi o wymogu stawianym teorii naukowej przez Bohra, by była „dostatecznie szalona”.

czy zasady nieoznaczoności Heisenberga oraz odkrycie metanaukowej zasady niedookreślenia teorii przez obserwacje (Życiński, 1988b, s. 11; 2013, s. 20).

Wewnętrzna logika kierująca przejściem od rewolucji naukowej do rewolucji metanaukowej wydaje się być zatem w rozumieniu Życińskiego następująca. Pojawienie się nowej teorii naukowej o określonych cechach (fakt metateoretyczny) wywołuje szok intelektualny (fakt psychologiczny), a ten z kolei *przyczynowo* prowadzi do zaproponowania nowych koncepcji metanaukowych (fakt metateoretyczny wyższego rzędu), przy czym treść tych ostatnich *z zasady* nie zależy od uwarunkowań psycho-społecznych, lecz od treści wyjściowego faktu metateoretycznego i od przyjmowanej tradycji badawczej[22]. Jest to zatem relacja logiczno-merytoryczna. W perspektywie analizy metanaukowej najbardziej fundamentalne wydają się być zatem fakty metateoretyczne zachodzące w obrębie nauk przedmiotowych. Ich zajście (np. pojawienie się określonej teorii naukowej) staje się *przyczyną* określonych faktów psychologicznych, takich jak intelektualny szok filozofów. Ten ostatni z kolei okazuje się możliwą *przyczyną* nowych faktów metateoretycznych, tym razem jednak w obrębie metanauki: pojawienie się nowych rozwiązań metanaukowych.

Życiński mówi w tym kontekście o potrzebie ‘racjonalnej *reakcji*’ filozofów (Życiński, 1988b, s. 143; 2013, s. 254, podkreślenie moje) na zaskakujące implikacje rewolucji naukowej. Ukazując znacząco istotniejszą obecność *elementu niepewności i subiektywizmu* w nauce od powszechnie zakładanego rewolucja naukowa uświadomiła filozofom *fundamentalne ograniczenia* racjonalności naukowej. Ale już treść tak psychologicznie uwarunkowanych nowych rozwią-

[22] Na temat związania z tradycją badawczą lub z paradygmatem będzie mowa niżej w punkcie 4b.

zań metanaukowych jest zasadniczo określana związkami logiczno-merytorycznymi. Reakcje te powinny być zdaniem Życińskiego *racjonalne*[23].

Przykładem takiej metanaukowej reakcji jest bez wątpienia wspomniana reakcja N. Bohra na doświadczenie paradoksalności nowych teorii naukowych. Była to intelektualna próba wyjścia z impasu w metanauce i sprowadzała się do przeformułowania koncepcji naukowości jako takiej. W celu obrony idei naukowości Bohrowi wystarczało proste uznanie cechy paradoksalności za kryterium naukowości. Życiński nie mówi jednak, czy była to reakcja racjonalna, czy nieracjonalna. Oceny takiej podejmuje się natomiast w odniesieniu do metanaukowych koncepcji internalizmu i eksternalizmu (zob. tamże).

W swej metanaukowej analizie postawy Bohra Życiński nie poprzestaje na stwierdzeniu zajścia prostej reakcji metanaukowej ze strony Bohra. Jego analiza idzie jeszcze głębiej. Poszukuje on w postawie Bohra *jeszcze ogólniejszych* faktów metateoretycznych, a zatem ukrytych na jeszcze wyższym poziomie dedukcyjnych warunków możliwości tej postawy. Pisze, że przywiązanie prezentowane przez Bohra do idei paradoksalności samo w sobie jest już „wyrazem odejścia od tradycyjnych założeń na temat roli zdroworozsądkowych kryteriów racjonalności w nauce" (Życiński, 1988b, s. 9; 2013, s. 16). Metanaukowa postawa Borha jest dla Życińskiego swego rodzaju antycypacją przyszłej rewolucji metanaukowej, jaka w filozofii nauki

[23] Życiński nie ma tutaj na myśli *racjonalności* w sensie *źródła* tej reakcji, czyli że jest to reakcja intelektu względnie rozumu, lecz w znaczeniu *normatywnym*. Normatywny charakter wynika z faktu, iż Życiński podaje jednocześnie kryteria tej racjonalności. Należy dodać, że Życiński zna i racjonalizuje na gruncie swej metanauki wyjątki od normatywnej reguły *racjonalnych reakcji*. W metanauce Życińskiego rządzi nimi tak zwana zasada aracjonalności. Zostanie ona omówiona w kolejnej części tego tekstu (zob. wyżej tekst i przypis nr 5).

nastąpi dopiero w drugiej połowie XX wieku. Jednocześnie jednak postawa ta stanowi wyraźny przykład racjonalnego związku faktów metateoretycznych z poziomu nauki (teza o paradoksalnym charakter nowych teorii naukowych) z faktem metateoretycznym na poziomie metanauki (nowa koncepcja naukowości). Można jedynie się zastanawiać, na ile związek ten jest zapośredniczony przez pozaracjonalny element podmiotowy, czyli przez jakiś fakt psychologiczny[24].

Przykład Bohra pokazuje jednak tylko jedną z wielu możliwych *rewolucyjnych zależności intelektualnych* pomiędzy zmianami na poziomie nauki a zmianami na poziomie metanaukowym, czy – ogólniej – na poziomie filozoficznym. Zdaniem Życińskiego bogactwo zmian, jakie zaszły i ciągle zachodzą w nauce w wyniku rewolucji naukowej, jest tak wielkie, że trudno przedstawić ich wszystkie możliwe *konsekwencje* filozoficzne, w szczególności *metanaukowe* (zob. J. Życiński, 1988b, s. 8, 2013, s. 15). Z konieczności ogranicza się więc do wyartykułowania tylko tych, które uważa za najbardziej istotne z punktu widzenia problemu racjonalności.

3. Rewolucja metanaukowa: *doxa* zamiast *episteme*[25]

Po omówieniu wewnętrznej logiki metanaukowych analiz Życińskiego czas na przedstawienie bardziej deskryptywnego elementu

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[24] Rozwijana przez Życińskiego koncepcja Polanyiego *wiedzy milczącej* i *wiedzy osobowej* pozwala przyjąć, że w jego przekonaniu oba te fakty metateoretyczne muszą być powiązane ze sobą podmiotowymi uwarunkowaniami Bohra. Metanaukowe rozwiązanie Bohra w najmniejszym bowiem stopniu nie jest konieczną, logiczną konsekwencją odkrytej przezeń paradoksalności nowych teorii naukowych. Na temat Polanyiego zob. (Życiński, 1983, s. 169n; 1985, s. 156–166; 1988b, s. 144.202; 2013, s. 218.351; 2015, s. 179–191) zob. też niżej przypis 53.

[25] Zob. tytuł rozdziału w (Życiński, 1983, s. 101).

jego rozwiązania, a mianowicie punktu wyjścia, jakim jest stwierdzenie metanaukowego faktu rewolucji naukowej i metanaukowego faktu rewolucji metanaukowej. Nie zachodzi tutaj oczywiście symetria. Idea *rewolucji naukowej* stała się częścią języka współczesnej kultury. Bliższego przedstawienia i uzasadnienia wymaga natomiast stwierdzenie faktu *rewolucji metanaukowej*.

Zdaniem Życińskiego (1988b, s. 7; 2013, s. 13) termin ‘rewolucja’ bywa nadużywany w analizach z zakresu historii nauki i filozofii. Nie każde psychologiczne odczucie nowości czy zmiany usprawiedliwia metanaukowe użycie tego terminu. By mówić o rewolucji w nauce oprócz psychologicznego faktu zaskoczenia i nowości konieczne jest spełnienie jakiegoś racjonalnego kryterium. W ujęciu Życińskiego kryterium takim jest przełomowy charakter zmian w podstawowych założeniach. Nie dziwi zatem, że utożsamia on dwudziestowieczną rewolucję naukową z pojawieniem się teorii względności i mechaniki kwantowej a jako nazwy własnej tej rewolucji używa wyrażenia ‘rewolucja Einsteina-Plancka’[26]. Osobno mówi też o rewolucji metamatematycznej związanej z pojawieniem się twierdzeń limitacyjnych[27] oraz ogólniej o rewolucji w podstawach matematyki, zapoczątkowanych zakwestionowaniem piątego postulatu Euklidesa (zob. Życiński, 1983, s. 196).

Analogiczne kryterium rewolucyjności ma zastosowanie także w odniesieniu do rewolucji metanaukowej. Zdaniem Życińskiego (1996, s. 126; por. 1983, s. 101) radykalne zmiany, jakie zaszły w fi-

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[26] Wyrażenie to pojawia się przykładowo w (Życiński, 1988b, s. 13.25; 2013, s. 24.45) i w (Życiński, 2015, s. 228, 258).

[27] Wyrażenie ‘rewolucja metamatematyczna’ pojawia się w (Życiński, 1988b, s. 101; 2013, s. 181). Epistemologiczną interpretację twierdzeń limitacyjnych Życiński przedstawia szczegółowo w (1985, s. 118–126; 1988a, s. 18–46). Tematyka ta jest także obecna w (Życiński, 1988b; 2013, rozdział IV; 1993, s. 61–68; 1996, s. 262–276).

lozoficznej refleksji nad nauką od lat trzydziestych XX wieku zasługują na miano *rewolucji metanaukowej*. W wyniku rewolucji naukowej konieczne okazało się porzucenie tradycyjnych, wygórowanych *założeń epistemologicznych* oraz naiwnego pojmowaniu samej natury poznania naukowego. Rewolucja ta wywołała „kryzys wiary" w przyrodoznawstwo jako pewną, niekwestionowaną i doskonałą wiedzę o rzeczywistości i jej prawach (zob. Życiński, 1983, s. 102).

Porzucenie tradycyjnych założeń epistemologicznych definiujących naukowość w kategoriach 'wiedzy pewnej', i 'doskonałej' Życiński przedstawia jako odejście od tradycyjnego ideału *episteme*[28]. Wszyscy teoretycy nauki, jacy nastali po Carnapie, zarówno indukcjoniści, jak i dedukcjoniści, porzucili ideał *episteme* na rzecz *doxa* (Życiński, 1983, s. 107), a jeśli uwzględni się dodatkowo odkrycie nieusuwalnej niedoskonałości poznania matematycznego, to jedyna możliwa konkluzja, jaka się narzuca Życińskiemu, jest następująca: „epistēmē kurczy się gwałtownie, ustępując wszechwładnej *doxa*" (Życiński, 1983, s. 109). Identyczna interpretacja pojawia się w (Życiński, 1988b, s. 12n; 2013, s. 22n), tyle że nieco inaczej wyartykułowana i poparta innymi przykładami. Życiński mówi o końcu „epistemetycznej" teorii nauki, w której rozwijano platońsko-arystotelesowską tradycję *episteme* rozumianej jako wiedza pewna i niepodważalna. Epistemetyczna teoria nauki została zastąpiona teorią „doksatyczną", kontynuującą platońską tradycję *doxa*, wiedzy prawdopodobnej. Dokonało się to na skutek odkrycia *istotnych ograniczeń* poznawczych w różnych dziedzinach poznania. Twierdzenie o niezupełności w logice, zasada losowości w fizyce

[28] Określenie to pojawia się w (Życiński, 1983, s. 102). Kategoria 'ideału *episteme*' jest analogiczna do ukutej w mniej więcej tym samym czasie kategorii J. Watkinsa 'ideału Bacona-Kartezjusza'. Watkins w przeciwieństwie do Życińskiego pomija jednak całkowicie starożytne źródła nowożytnych ideałów poznawczych, zob. (Watkins, 1989, s. 31–36). Angielski oryginał pochodzi z 1984 r.

czarnych dziur, zasada nieoznaczoności w fizyce kwantowej i epistemologiczna zasada niedookreśloności to tylko niektóre z takich „limitacyjnych" odkryć przytaczanych przez Życińskiego (zob. 1988b, s. 11; 2013, s. 22n)[29].

Porzucony ideał *episteme* pochodzi z arystotelesowskiej tradycji *episteme* (Życiński, 1983, s. 102; 1993, s. 46–55). Życiński nie utożsamia jednak tego ideału z oryginalnym pojęciem wypracowanym przez samego Arystotelesa. Wprawdzie takie cechy jak pewność i niepodważalność występują również w Arystotelesowskiej definicji *episteme*, niemniej są one niewystarczające, by ukonstytuować jego oryginalne pojęcie. Dodatkowe określenia takie jak konieczność i przyczynowość (Życiński, 1993, s. 46) oraz bazowanie na niewzruszonych podstawach (Życiński, 1988b, s. 143; 2013, s. 253)[30] także nie wyczerpują cech istotnych oryginalnej koncepcji Arystotelesa[31]. Z wypowiedzi Życińskiego (1993, s. 46) wynika, że chodzi mu raczej o to, co z oryginalnego pojęcia *episteme* przetrwało w nowożytnej tradycji filozoficznej i co inspirowało twórców terminu 'epistemologia' rozumianej jako teoria wiedzy[32]. Jest to ideał wiedzy pewnej wyzna-

[29] W (Życiński, 1996, s. 129) pojawia się stwierdzenie, że odejście od ideału *episteme* było *metanaukowym odpowiednikiem* rewolucji Einsteina-Plancka i że współczesna epistemologia nauk przyrodniczych stała się de facto *doxalogią*. Tematyka rewizji *episteme* występuje również w (Życiński, 1993, s. 55).

[30] Pisze na przykład o frustrującym (*disappointing*) odkryciu, że u podstaw nauki nie znajdują się jakieś *niewzruszone (unshakable) podstawy*, lecz rozmyty zbiór (arbitralnych) przedzałożeń.

[31] Arystoteles podaje i omawia je w pierwszej księdze *Analityk wtórych* w rozdziale 2 i 4.

[32] Warto zauważyć, że termin ten (ang.: *epistemology*) został ukuty według historyków dopiero w wieku XIX przez filozofa angielskiego J.F. Ferriera pozostającego pod silnym wpływem Fichtego i jego koncepcji *Wissenschaftslehre*. Ferrier wzorował się na innym, wcześniejszym terminie 'ontology'. Termin ten oznaczał u niego właśnie *theory of knowledge* (zob. Ferrier, 1854, s. 44). Na temat historii tego terminu zob. (Sinacœr, 1973, s. 63–66).

wany wspólnie przez nowożytnych autorów o bardzo różnych poglądach filozoficznych: Kartezjusza, Keplera, Galileusza, Newtona czy Leibniza. W przypadku Leibniza (Życiński, 1993, s. 51) tradycja arystotelesowska została dodatkowo połączona ze pewną wersją świata idei Platona. Ten specyficzny platonizm epistemologiczny przedostał się z kolei za pośrednictwem prac Fregego i młodego Russella do dwudziestowiecznej filozofii z kręgu Koła Wiedeńskiego[33]. W tak rozumianej tradycji *episteme* obrona rozumu naukowego wymaga odwołania się do Platońskiej ontologii głoszącej „niesprowadzalność idei do procesów psychofizycznych" (Życiński, 1996, s. 126). Życiński mówi również (Życiński, 1983, s. 102n), że w wieku XIX, gdy zakwestionowano naukowy charakter filozofii, ideał *episteme* został bezkrytycznie przeniesiony na nauki empiryczne. W epistemologii nastała era scjentyzmu, której epigonem było Koło Wiedeńskie, w szczególności Carnap, wraz ze swym pozytywizmem logicznym.

Odrzucenie nierealistycznego ideału *episteme* nie oznacza jednak dla Życińskiego porzucenia idei *racjonalności wiedzy*, i przyjęcia sceptycznego punktu widzenia na naukę. Przeciwnie, uważa że możliwa jest kontynuacja tradycji arystotelesowsko-platońskiej *episteme* i sam siebie do takiego nurtu filozoficznego zalicza. Tradycja ta wymaga jednak istotnych zmian. W miejsce nierealistycznych apriorycznych ideałów należy przedstawić koncepcje wyjaśniające faktyczne uwarunkowania „*rzeczywistości* określanej mianem nauki" (zob. Życiński, 1996, s. 126 – podkreślenie moje).

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[33] Zapewne to ten leibnizjański związek arystotelesowskiej *episteme* ze światem idei Platona ma na myśli Życiński, gdy rozszerza pojęcie *tradycji arystotelesowskiej* na pojęcie *tradycji platońsko-arystotelesowskiej*, zob. (Życiński, 1988b, s. 12; 2013, s. 22; 1996, s. 126).

4. Metanaukowe konsekwencje rewolucji naukowej

Za dwa najistotniejsze metanaukowe uwarunkowania rzeczywistej nauce Życiński uznaje obecność w nauce *elementów pozanaukowych*[34] oraz *czynników pozaracjonalnych*, a w przypadku tych ostatnich ich istotną rolę rozwoju nauki[35]. Odkrycie tych uwarunkowań przez filozofów należy uznać za metanaukowe konsekwencje rewolucji naukowej. Odróżnienie to wydaje się jak najbardziej na miejscu. W nauce ujmowanej przedmiotowo, a zatem jako pewien uporządkowany zbiór zdań lub sądów, elementy pozanaukowe nauki to zdania (sądy), których nie można uzasadnić metodą właściwą nauce empirycznej lub analitycznej. Życiński utożsamia je z przekonaniami ideologicznymi i/lub filozoficznymi, najczęściej przyjmowanymi w formie milczących założeń[36]. Funkcjonują one jako idee kształtujące *implicite* zarówno treść teorii naukowych, jak i zacho-

[34] Jakkolwiek paradoksalnie może brzmieć to wyrażenie, to jest ono jednak wyrażeniem sensownym. Pod warunkiem wszakże uzmysłowienia sobie faktu świadomej i w dużej mierze nieuniknionej odmienności znaczeniowej terminu 'nauka' (i jego przymiotnikowej odmiany) w dwóch jego wystąpieniach w tym wyrażeniu. Dla oddania tej dwuznaczności należałoby zastosować, na przykład, indeksy dolne: 'element *pozanaukowy1* w *nauce2*'. 'Nauka1' odnosiłaby się do intersubiektywnej, racjonalnej rzeczywistości artykułowalnej za pomocą języka. Byłaby to nauka rozumiana zarówno przedmiotowo, jak i funkcjonalnie. Z kolei 'nauka2' odnosiłaby się do pewnego złożonego, obserwowalnego *zjawiska*, na które składa się zarówno aspekt intersubiektywny nauki, jak i jej aspekt podmiotowy. Termin 'nauka2' odpowiadałby temu, co Życiński nazywa *rzeczywistą* lub *realnie istniejącą nauką*, względnie *rzeczywistością zwaną nauką*; 'nauka1' byłaby natomiast pewną *wyidealizowaną* i *uproszczoną* wizją nauki filozofów, mniej lub bardziej zgodną z nauką rzeczywistą.

[35] Od samego początku Życiński wiąże obecność czynników pozaracjonalnych w nauce z kwestią jej rozwoju (zob. Życiński, 1983, s. 143).

[36] Na temat *ideologii* i jej odróżnienia od *filozofii* zob. (Życiński, 1988b, s. 18; 2013, s. 33). Wśród elementów pozanaukowych Życiński odróżnia *ideaty* od *przedzałożeń* (*presumptions*) *filozoficznych*: „ogólnie rzecz biorąc, nie można postrzegać ideatów

wania epistemologiczno-metodologiczne naukowców. Z kolei w odniesieniu do nauki ujmowanej funkcjonalnie mowa jest o pozaracjonalnych czynnikach lub faktorach wpływających zarówno na sposób uprawiania nauki, jak i na treść wysuwanych hipotez naukowych. Ich charakter pozaracjonalny oznacza, iż nie są to uniwersalne *racje*, lecz konkretne *przyczyny* sprawcze zachowań naukowych i metanaukowych. Najczęściej Życiński wymienia przyczyny socjologiczne i psychologiczne[37].

4a. Założenia filozoficzne w nauce[38]

Jednym z istotnych elementów metanaukowej rewolucji było uświadomienie sobie przez naukowców, a jeszcze bardziej przez filozofów, filozoficznego zakorzenienia nowej nauki:

> nie można zaprzeczyć, że konsekwencje odkryć związanych z teorią ewolucji wszechświata, z fizyką czarnych dziur, z twierdzeniami limitacyjnymi w metalogice, czy z pojawieniem się wielu nowych dyscyplin naukowych są blisko zwią-

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jako wytworu filozoficznych przedzałożeń" (Życiński, 1988b, s. 29; 2013, s. 51). Podobnie nie każde założenie filozoficzne jest godne miana (fundamentalnego) przedzałożenia (*presumption*) (Życiński, 1988b, s. 143; 2013, s. 253).

[37] Zob. np. (Życiński, 1983, s. 142nn; 1988b, s. 9; 2013, s. 17 a także 1996, s. 190), gdzie mówi o *czynnikach psycho-społecznych*.

[38] Zagadnieniu założeń filozoficznych w nauce Życiński poświęca wiele miejsca, np. *Język i metoda* (1983, s. 246–261); znaczna część pierwszego tomu *Teizmu* (Życiński, 1985, s. 156–232); pierwszy rozdział *Struktury* oraz część druga *Granic racjonalności* (Życiński, 1993). Niezwykle istotne wypowiedzi na ten temat zwarte są również w *Strukturze* przy okazji omawiania epistemologicznej zasady niepewności (*uncertainty*) lub nieoznaczoności (Życiński, 1988b, s. 143n; 2013, s. 254–256) Zob. wypowiedź przytoczoną w przypisie 32.

> zane z wielkimi filozoficznymi pytaniami nurtującymi ludzkość od niepamiętnych czasów (Życiński, 1988b, s. 8; 2013, s. 16).

Podobne metateoretyczne implikacje dla filozofii nauki ma metanaukowy fakt stosowania przez naukowców określonej metody naukowej:

> nawet ci z przyrodników, którzy są nastawieni niechętnie wobec filozofii muszą – przynajmniej *implicite* – przyjmować jakieś założenia filozofii nauki. W przeciwnym razie przyrodoznawstwo byłoby uprawiane przez nich w stylu żywiołowo-naiwnym (Życiński, 1983, s. 254).

Odkrycia te zachwiały głęboko zakorzenioną modą na antymetafizyczne tendencje pozytywizmu. W wyniku rewolucji naukowej jedna z podstawowych metanaukowych tez pozytywizmu, teza o mitologicznym charakterze metafizyki i filozofii w ogóle, sama okazała się niekrytycznym mitem (Życiński, 1996, s. 228)[39].

W ujęciu mniej poetyckim odkrycie elementów pozanaukowych zostaje powiązane przez Życińskiego z rewolucją naukową Einsteina-Plancka za pomocą idei *zmiany teoretycznej* i ideą *zmiany wizji świata* implikowanej przez teorie. Zmiany te uświadomiły filozofom, że praktyka naukowa nie polega na protokołowaniu czystych faktów (Życiński, 1988b, s. 34; 2013, s. 59) i że jej rozwój nie jest

[39] W sposób szczególny Życiński poddaje krytyce koncepcję pozytywizmu logicznego, pokazując jego milczące założenia metafizyczne (Życiński, 1983, s. 246–252). Życiński obraca metodę racjonalistycznych „mistrzów podejrzeń" przeciw nim samym. Podobną taktykę stosował Popper, gdy mówił o *przesądzie empirystycznym* lub *indukcjonistycznym* w epistemologii. Miał przy tym na myśli niekrytyczną wiarę empirystów nowożytnych w indukcję. Przesąd (mit, poezja) wskazywany przez Życińskiego jest jeszcze bardziej fundamentalną niekrytyczną presupozycją nowożytnego empiryzmu. Do wyznawców tego mitu zalicza się także Popper, gdyż głęboko wierzył on, przynajmniej we wczesnym okresie swej twórczości, w ostrą demarkację nauki i metafizyki (Życiński, 1988b, s. 11; 2013, s. 19n; 1996, s. 228nn).

prostą kumulacją kolejnych odkryć (zob. Życiński, 1996, s. 229). W nowym paradygmacie metanaukowym neokantowskie w swej wymowie idee uteoretyzowania obserwacji i obecności elementu pozaempirycznego w hipotezach teoretycznych stały się niekontrowersyjnymi faktami metateoretycznymi (por. Życiński, 1996, s. 127).

Radykalne zmiany w wizji świata implikowane przez nowe teorie wyraźnie wskazują na nieusuwalny element pozanaukowy w rozwoju nauki, choćby przez to, że przeczą *zdrowemu rozsądkowi*[40]. Teoria względności i teoria kwantów zadały kłam niejednej zdroworozsądkowej oczywistości determinującej zachowania naukowców. Niezwykle wymowny jest w tym kontekście przykład Einsteina, który nie miał problemu z zaakceptowaniem nowatorskich idei relatywistycznych, a mimo to nie potrafił zaakceptować idei rozszerzającego się wszechświata, idei, która była logiczną konsekwencją jego równań pola. Silne przywiązanie do „oczywistej" idei statyczności wszechświata skłoniło go do wprowadzenia do równania całkowicie *ad hoc* członu lambda zapewniającego oczekiwaną statyczność rozwiązania. Bez wątpienia przywiązanie Einsteina do idei statecznego wszechświata wbrew racjonalnym argumentom stanowi silny argument za tezą o pozaracjonalnych uwarunkowaniach decyzji Einsteina[41].

[40] O sprzeczności ze zdrowym rozsądkiem zob. (Życiński, 1983, s. 249; 1985, s. 176n). Według Życińskiego teza Einsteina i jego współpracowników o załamywaniu się w mechanice kwantowej pojęć języka potocznego posiada istotne implikacje filozoficzne dotyczące racjonalności ontycznej i realizmu poznawczego. O nieusuwalności z nauki elementów pozanaukowych zob. (Życiński, 1988b, s. 34; 2013, s. 60), gdzie pisze że wszelkie próby wyeliminowania ideatów z nauki prowadziły do wprowadzenia nowych, ukrytych ideatów.

[41] Na temat Einsteina zob. (Życiński, 1983, s. 249; 1988b, s. 73; 2013, s. 130; 1996, s. 189). Zachowanie Einsteina pokazuje, że idea statyczności wszechświata nie była ideą „wyprowadzoną" z faktów, też ani empiryczną hipotezą poddawaną empirycznemu sprawdzaniu. Pojęcie *przywiązania* lub *związania* (*commitment*) Życiński stosuje w sposób analogiczny do Kuhna.

Innym faktem metanaukowym świadczącym dobitnie o obecności pozanaukowego elementu filozoficznego w praktyce naukowej jest to, iż praktyka falsyfikowania hipotez teoretycznych we współczesnej nauce nierzadko odbiega od idei empirycznej falsyfikacji w jej *uproszczonej* i *wyidealizowanej* wersji przedstawianej przez Poppera (zob. Życiński, 1996, s. 230)[42]. Zaistnienie logicznej sprzeczności pomiędzy hipotezą teoretyczną a faktem nie musi prowadzić do obalenia wyjściowej hipotezy, i to nie tylko dlatego, że brak innej, lepszej teorii. Życiński wskazuje na liczne przypadki z mechaniki kwantowej, gdy wystąpienie empirycznej niezgodności z podstawowymi założeniami teoretycznymi tych teorii nie doprowadziło do odrzucenia mechaniki kwantowej, a jedynie do prób zrewidowania głębokich założeń filozoficznych tychże teorii. Naukowe reakcje na eksperyment EPR i na odkrycie nierówności Bella prowadziły nierzadko do prób negacji założeń realizmu ontologicznego i epistemologicznego oraz tradycyjnej idei racjonalności, założeń konstytuujących tradycyjną koncepcję „rzeczywistości obiektywnej" (J. Życiński, 1996, s. 230)[43].

Równie istotne z punktu widzenia implikacji metanaukowych okazała się rewolucja w pojmowaniu procedury obserwacyjnej. Już Pierre Duhem wskazywał na głębokie uteoretyzowanie narządzi obserwacji i eksperymentu (zob. Duhem, 1991, s. 77–81, 85–89). Życiński idzie dalej i mówi o ich istotnym obciążeniu „bagażem tez ontologicznych", niezależnie od stopnia świadomości tego faktu przez

[42] Życiński nie ma raczej tutaj na myśli naiwnej wersji falsyfikacjonizmu przypisywanej wczesnemu Popperowi, czyli takiej, w której falsyfikatory empiryczne miałyby charakter bezwzględnie rozstrzygający. W (Życiński, 1983, s. 121) mówił bowiem, że naiwnego falsyfikacjonizmu Popper właściwie nigdy nie głosił.

[43] Przykłady te Życiński omawia szeroko w (1985, s. 175–180). Wykorzystuje je również metanaukowo w (Życiński, 1988b, s. 129n.137; 2013, s. 228nn.243; 1996, s. 230).

ich użytkowników (Życiński, 1983, s. 249). Szczególnie wymowne są tutaj jednak radykalne zmiany w pojmowaniu przedmiotu obserwacji, jakie pociąga za sobą teoretyczny rozwój fizyki czarnych dziur. W kontekście tej fizyki traci swój obiektywny sens takie tradycyjne pojęcie jak pojęcie *obiektu materialnego*. Jedynym uzasadnieniem jego użycia mogą być dzisiaj jedynie względy sentymentalne (Życiński, 1988b, s. 77; 2013, s. 137n).

Metanaukowy fakt odkrycia obecności nieusuwalnego[44] elementu filozoficznego w języku nauki eksploatowany jest przez Życińskiego do różnych celów filozoficznych. Jednym z nich jest jego żywe zainteresowanie nową metafizyką, która pozwoliłaby pogodzić religijny obraz świata z obrazem implikowanym przez współczesną naukę. Wiele ze swych publikacji poświęcił on opracowaniu nowej wersji teizmu, określanego mianem *panenteizmu* (zob. np. Życiński, 1988a). Na pytanie „Czy można żyć bez metafizyki?" – to znaczy czy można uprawiać naukę bez założeń filozoficznych – odpowiada, że na metafizykę tak czy inaczej „jesteśmy skazani" i to niezależnie od składanych na ten temat deklaracji (Życiński, 1983, s. 246.249). Jej całkowita eliminacja z języka nauki musiałaby skutkować *zupełnym milczeniem* naukowców. Z tego punktu widzenia odkrycie elementów pozanaukowych w nauce rozumiane jest przez Życińskiego jako rewolucyjna, w kontekście dominującego wcześniej pozytywizmu, rehabilitacja metafizyki. Jednocześnie jest też ono wezwaniem do poddania tradycyjnej metafizyki istotnym modyfikacjom w obliczu rewolucyjnych zmian w założeniach filozoficznych wprowadzonych przez nowe teorie naukowe.

[44] Mówiąc o nieusuwalności elementu pozanaukowego z nauki, a w szczególności założeń filozoficznych, należy wspomnieć o polemice Józefa Życińskiego (2009) z Janem Woleńskim (2009) na temat relacji nauki do filozofii i *vice versa*.

4b. Czynniki pozaracjonalne w nauce[45]

Z punktu widzenia rewolucji metanaukowej o wiele ważniejsze są jednak inne, filozoficzno-naukowe implikacje, jakie Życiński wyprowadza z faktu odkrycia w nauce nieusuwalnego elementu filozoficznego. Warto podkreślić, że nie chodzi tutaj o implikacje *stricte* metanaukowe, czyli w terminologii Życińskiego dotyczące logicznej struktury nauki i jej procedur uzasadniania, lecz o implikacje filozoficzno-naukowe, wykraczające poza teren analiz logicznych i wkraczające na teren opisu nauki rzeczywistej oraz jej rzeczywistego procesu rozwoju.

Dla filozofii nauki szczególnie doniosłe okazało się jednoczesne odkrycie rewolucyjnej *zmienności* przyjmowanych w nauce założeń filozoficznych. Jak w przypadku Einsteina, nowe idee stały w „rażącej sprzeczności ze zdrowym rozsądkiem" (Życiński, 1983, s. 249), z utrwaloną powszechnie *wizją* świata (Życiński, 1996, s. 229) i z „epistemetyczną" *tradycją* rozumienia tego, czym jest nauka lub racjonalność. Doświadczenie tego typu sprzeczności świadczy nie tylko o obecności elementu filozoficznego w nowych teoriach, ale także o jego radykalnej zmianie trudnej do wytłumaczenia na gruncie racjonalistycznych koncepcji metanaukowych. Tego typu doświadczenie towarzyszyło zarówno porzuceniu arystotelesowskiej idei doskonałego świata nadksiężycowego za czasów Galileusza, jak i pojawieniu się idei rozszerzającego się wszechświata jako zupełnie niespodziewanej przez Einsteina implikacji jego równań pola. Po-

[45] Kwestia czynników pozaracjonalnych jest omawiana przez Życińskiego równie szeroko jak kwestia założeń filozoficznych, zob. (Życiński, 1983, s. 127–154; 1985, s. 156–166; 1988b, rozdz. 5; 2013, rozdz. 5; 1996, rozdz. 7).

dobne sytuacje dotyczyły pojawienia się idei zbiorów nieskończonych Cantora, czy idei podzielności atomu, o twierdzeniach limitacyjnych nie wspominając[46].

Zderzenie nowych teorii ze zdrowym rozsądkiem, z milczącymi oczywistościami, z niewyartykułowaną intuicją tego, co normalne i milcząco oczekiwane, musi dawać do myślenia. I to nie tyle na temat samych teorii, ile na temat naukowców i filozofów reagujących w ten lub inny sposób na wspomniane zdroworozsądkowe sprzeczności. Zachowania naukowców wykraczające poza tradycyjne wzorce *racjonalności* czy *normalności* muszą być warunkowane przez coś, co wykracza poza racjonalną artykulację. To coś Życiński nazywa *czynnikami pozaracjonalnymi* lub *zewnętrznymi uwarunkowaniami*[47]. Wybór terminologii nie jest przypadkowy. Życiński przeciwstawia termin 'pozaracjonalny' terminowi 'irracjonalny' (zob. Życiński, 1988b, s. 137; 2013, s. 242)[48].

Życiński poddaje analizie na przykład „szokujący" fakt zmiany oceny racjonalności hipotezy tachionów we współczesnej fizyce. Mówi, iż jest to „niezwykle interesujący przykład przejścia od fan-

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[46] Podane przykłady pochodzą z (Życiński, 1983, s. 187) oraz z (Życiński, 1996, s. 247). W *Elementach* (Życiński, 1996, s. 267n) mówi o emocjonalnych reakcjach na Gödlowskie twierdzenie o możliwej wewnętrznej sprzeczności arytmetyki. Zob. też wcześniejsze uwagi na temat *intelektualnego szoku* i *paradoksalności*.

[47] Określenie 'czynniki pozaracjonalne' oraz równoważne 'elementy aracjonalne' pojawia się w (Życiński, 1983, s. 142). Nieco wcześniej (s. 127) mowa jest o 'czynnikach pozalogicznych'. Wyrażenia te Życiński wzoruje *explicite* na wyrażeniu Satosi Watanabego „les éléments arationnels" (s. 142). Wyrażenie 'uwarunkowania zewnętrzne' pojawia się w (Życiński, 1988b, s. 130; 2013, s. 230). Stosuje też inne określenia takie jak 'czynniki podmiotowe', 'czynniki subiektywno-osobowościowe', 'pozaracjonalne elementy (składniki)' nauki, czy w końcu 'elementy nieskonceptualizowane'.

[48] Nie tłumaczy jednak bliżej różnicy. Z kontekstu można wnioskować, że irracjonalność ma charakter *wewnętrzny* w stosunku przekonań człowieka, natomiast czynniki pozaracjonalne są wobec nich zewnętrzne, ale je mogą determinować, na przykład w kwestii wyboru spośród alternatywnych i równoważnych teorii

tastycznej koncepcji *science-fiction* do racjonalnej hipotezy” (Życiński, 1988b, s. 129; 2013, s. 228). To, co wcześniej uchodziło za *absurdalne* i wywoływało *psychologiczną awersję*, w stosunkowo krótkim czasie zostało *znormalizowane* w wyniku odpowiedniej *reinterpretacji* znaczeń. Dla wyjaśnienia tego faktu metanaukowego proponuje on, by rozróżnić racjonalność wewnętrzną teorii od *racjonalności zdroworozsądkowej*, odpowiedzialnej za doświadczenie szoku, absurdalności i potrzeby normalizacji. Mówi też że zdroworozsądkowa racjonalność zależy od *uwarunkowań zewnętrznych* wobec nauki (Życiński, 1988b, s. 130; 2013, s. 229n)[49].

Wspomniany fakt modyfikacji *ad hoc* wprowadzonej do równań pola przez Einsteina (zob. Życiński, 1996, s. 189) podlega analogicznej interpretacji. Określenie *ad hoc* wskazuje na irracjonalny – przynajmniej w odniesieniu do *racjonalności wewnętrznej* teorii – charakter modyfikacji Einsteina. A skoro idea rozszerzającego się wszechświata stała w sprzeczności ze *zdrowym rozsądkiem* determinującym zachowania naukowe Einsteina (Życiński, 1983, s. 249), to jasno z tego wynika, że modyfikacja określana jako *ad hoc* musiała być determinowana przez jakieś czynniki *zewnętrzne* względem samej teorii[50].

W *Języku i metodzie* Życiński ograniczył się do ogólnego utożsamienia czynników pozaracjonalnych z uwarunkowaniami socjolo-

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[49] Życiński omawia hipotezę tachionów już w (1985, s. 175–176), ale nie przeprowadza tam jej analizy metanaukowej i nie wyprowadza implikacji na temat czynników pozaracjonalnych. Przykład ten wykorzystuje wyłącznie w celu analizy kwestii obecności filozoficznych przedzałożeń w nauce, w szczególności realizmu ontologicznego i epistemologicznego.

[50] Podana tutaj metanaukowa interpretacja faktu modyfikacji *ad hoc* nie wyczerpuje wszystkich metanaukowych konsekwencji, jak Życiński wyciąga z tego faktu. Hipotezy *ad hoc* posiadają ambiwalentny status metanaukowy. Mogą być szkodliwe, irracjonalne, gdy są wprowadzane dla *ratowania* programu badawczego, albo korzystne gdy przyczyniają się do rozwoju nauki. Na ten temat zob. (Życiński, 1983, s. 162n),

gicznymi i psychologicznymi. Z czasem poddał je jednak dokładniejszej analizie w perspektywie koncepcji *wiedzy osobowej* M. Polanyiego i wprowadził szersze pojęcie *elementów nieskonceptualizowanych* obejmujące sobą oprócz czynników psycho-społecznych także elementy quasi-racjonalne *wiedzy milczącej*, *wiedzy osobowej* oraz *intuicji twórczej* (zob. Życiński, 1996, s. 179–191)[51].

Niezależnie jednak od ewolucji w pojmowaniu czynników pozaracjonalnych Życiński niezmiennie od samego początku wskazuje na Kuhnowską ideę *związania z paradygmatem* (*commitment*) jako niezwykle użyteczne, a może nawet najlepsze narzędzie metanaukowej interpretacji faktu nieuniknionej obecności czynników pozaracjonalnych w nauce a także w metanauce[52]. W (Życiński, 1988b, s. 136n; 2013, s. 241n) mówi o potrzebie uzupełnienia braków w czysto racjonalnym wyjaśnianiu podejmowanych rozstrzygnięć kwestii naukowych w różnych dziedzinach, od fizyki przez matematykę po filozofię nauki, odwołaniem się do czynnika osobowego *commitment* (*zaangażowanie*) charakterystycznego dla poszczególnych programów badawczych. Jest to konieczne, gdyż wbrew maksymalistycznym postulatom racjonalistów (dosł. wbrew „oczekiwaniom marzycieli”) nie było możliwe wskazanie w filozofii nauki powszechnie akceptowanych kryteriów rozstrzygania problemów w poszczególnych dzie-

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[51] Przez *wiedzę milczącą* Życiński rozumie nie tylko milczące przedzałożenia teorii, lecz także wiedzę zawartą *implicite* w równaniach pola czy w formalizmie mechaniki Newtonowskiej (zob. Życiński, 1996, s. 189). Idee Polanyiego Życiński wykorzystuje już wcześniej (zob. wyżej przypis 24) ale nie czyni tego w sposób tak systematyczny jak w (1996).

[52] W (Życiński, 1983, s. 153) mówi o niewątpliwej zasłudze Kuhna ukazania paradygmatycznych uwarunkowań filozofii nauki. W (Życiński, 1985, s. 160) mówi o subiektywnym charakterze *commitment* – przywiązania do jednej z wielu możliwych teoretycznie koncepcji poznania i o podmiotowym związaniu z określoną tradycją badawczą nazywanym *commitment to paradigms* (1985, s. 164). W (Życiński, 1996, s. 191–200) terminy ‘*commitment*’ oraz ‘paradygmat’ poddaje szczegółowej analizie, by pokazać, jak *rzeczywiście* funkcjonują one w *realnej* nauce.

dzinach. Również przyjęciu najbardziej fundamentalnych przedzałożeń filozoficznych w programach lub tradycjach badawczych musi towarzyszyć element „częściowej arbitralności, wyboru i zobowiązania" (*commitment*). Jest tak, gdyż ich uzasadnienie może być jedynie *częściowe* (zob. Życiński, 1988b, s. 143; 2013, s. 253n)[53].

Jednakże koncepcja związania z paradygmatem, jak zobaczymy niżej, wymaga odpowiednich modyfikacji w duchu tradycji *episteme* tak, by je oczyścić z nazbyt eksternalistycznej interpretacji samego Kuhna.

5. Metanaukowy spór o czynniki pozaracjonalne w nauce: internalizm *vs* eksternalizm

Sytuacja metanaukowa u progu rewolucji metanaukowej w ujęciu Życińskiego przedstawiała się zatem następująco. W wyniku rewolucji naukowej filozofia nauki odkryła nieusuwalną obecność w nauce elementów pozanaukowych oraz determinujących te elementy czynników pozaracjonalych[54]. Ponieważ odkrycia te stały w sprzeczności z dotychczasowym ideałem *episteme*, konieczna była reakcja ze strony filozofów i w celu ich „normalizacji". Zgodnie ze swym związaniem z szeroko pojmowaną tradycją *episteme*, Życiński sta-

[53] Oba ostatnie przykłady *commitment* w nauce Życiński podciąga pod ogólną nazwę *epistemologicznej zasady niepewności* (*uncertainty*). Taka nazwa pada również w (Życiński, 1985, s. 159). Polski przekład angielskiej *Structure* oddaje tę nazwę zarówno jako *zasada nieoznaczoności* (Życiński, 2013, s. 240), jak *zasada niepewności* (s. 245). Faktem jest, że Życiński wzoruje ją na słynnej zasadzie nieoznaczoności Heisenberga. Szczegółowe omówienie tej zasady będzie możliwe dopiero w kolejnym artykule. Zob. tekst i przypis nr 5.

[54] Ideaty często zależą wyłącznie od uwarunkowań psychologicznych i socjologicznych a nie od bardziej podstawowej ontologii, zob. (Życiński, 1988b, s. 29; 2013, s. 51).

wia wymóg, by były to rozwiązania *racjonalne*, wykluczając tym samym rozwiązania sceptyczne czy anarchiczne (Życiński, 1988b, s. 143; 2013, s. 254). Krytyczne związanie z tradycją *episteme* pozwala Życińskiemu na racjonalną ocenę wszystkich analizowanych rozwiązań.

Nowe rozwiązania metanaukowe w ujęciu Życińskiego były całkowicie rozbieżne. W tym miejscu Życiński z historyka metanauki zamienia się w teoretyka metanauki. Rozbieżne tendencje rozwiązywania problemu obecności czynników pozaracjonalnych w nauce klasyfikuje za pomocą dwóch przeciwstawnych kategorii. W pierwszych pracach mówi o rozwiązaniach *internalistycznych* i *eksternalistycznych*. Z czasem wprowadza specyficzne, teoretyczne kategorie epistemologiczne 'internalizmu' i 'eksternalizmu'[55]. Wydaje się, że właśnie te kategorie dostarczają klucza do najlepszej interpretacji filozofii nauki Życińskiego. Pełnią one w jego filozofii rolę uogólnionych teoretycznych kategorii meta-metanaukowych pozwalających poddać analizie merytorycznej (treściowej) wszystkie dwudziestowieczne teorie (rozwiązania) metanaukowe. Za ich pomocą Życiński odkrywa i nazywa najogólniejsze presupozycje teoretyczne kształtujące *implicite* charakter dwudziestowiecznych roz-

[55] Przeciwstawienie to funkcjonuje od samego początku w tekstach Życińskiego (1983, s. 141nn; 1988b, s. 12.16; 2013, s. 21.29). Termin 'eksternalizm' pojawia się w (Życiński, 1985, s. 120n), ale oznacza tam jedynie specyficzne stanowisko metamatematyczne, uznające matematykę za wynik uwarunkowanych genetycznie ludzkich intuicji logicznych oraz kulturowo uzależnionych zdolności do łączenia abstrakcyjnych tez. W sensie ogólnych kategorii metanaukowych oba terminy pojawiają się już w (1993, s. 242), ale dopiero w (1996, s. 133nn) Życiński wykorzystuje je do opisu ogólnej sytuacji problemowej w metanauce i w filozofii nauki.

wiązań metanaukowych. Można powiedzieć, że kategorie te służą mu do hipotetyczno-teoretycznej interpretacji *racjonalnej rzeczywistości* (*sic!*) metanauki[56].

Uwaga 6. Heurystyczna funkcja procedury klasyfikacyjnej na gruncie nauk przedmiotowych stosunkowo wyczerpująco została przedstawiona przez Herschela (1955, s. 131–139). Klasyfikacji zjawisk dokonuje się z nadzieją na „pośrednie" odkrycie głębszych, ukrytych przyczyn względnie praw przyrody. Klasyfikacja jest hipotezą teoretyczną. Ewentualne potwierdzenie empirycznych predykcji klasyfikacji uwiarygodnia jej status jako poprawnej hipotezy wyjaśniającej. Analogiczna sytuacja zachodzi w odniesieniu do klasyfikacji teoretycznych czyli klasyfikacji różnego typu teorii naukowych. Pozwala ona na odkrycie ukrytych, głębszych i bardziej ogólnych (w sensie dedukcyjnym) przesłanek względnie presupozycji teoretycznych warunkujących treść określonych rozwiązań teoretycznych. W fizyce przykładem takiej klasyfikacji może być podział rozwiązań problemu natury światła na rozwiązania korpuskularne i rozwiązania falowe z nadzieją na pokonanie tego podziału na „głębszym" poziomie teoretycznym.

Oba typy klasyfikacji, klasyfikacje zjawisk (faktów) i klasyfikacje teoretyczne, funkcjonują zarówno na poziomie nauki przedmiotowej, jak i na poziomie metanauki, a przynajmniej na poziomie metanauki w rozumieniu Poppera, Lakatosa, czy Życińskiego, której postulowana metoda (specyficznie transcendentalna) jest analogiczna do metody empirycznej.

Przez *internalizm* Życiński rozumie stanowisko metanaukowe (1996, s. 134), według którego „treść teorii i twierdzeń naukowych jest determinowana przez wewnętrzną zawartość ich racjonalnych

[56] Od dziewiętnastego wieku, od czasów J. Herschela, albo jeszcze wcześniej, od czasów Kanta, wiadomo jednak, że wszelkie kategorie opisowe i klasyfikacyjne mają jednocześnie charakter aprioryczno-teoretyczny, a zatem w dużej mierze idealizujący. *Racjonalną rzeczywistość* metanauki można rozumieć jako *historyczną artykulację racjonalności obiektywnej* typowej dla metanauki. Ideę *racjonalności obiektywnej* przedstawią analizy zawarte w części drugiej mojego artykułu.

uzasadnień, natomiast czynniki pozaracjonalne mogą okazać się istotne dla nauki dopiero w sytuacjach, gdy nie można przedstawić merytorycznych uzasadnień dla proponowanych tez”. *Eksternalizm* jest określany natomiast jako stanowisko przeciwne względem internalizmu. Tym, co łączy wszystkie koncepcje eksternalistyczne jest szczególne „wyakcentowanie” związków między treściową zawartością nauki a zewnętrznymi uwarunkowaniami jej rozwoju (Życiński, 1996, s. 134) [57]. Do rozwiązań internalistycznych Życiński zalicza koncepcje Koła Wiedeńskiego, Poppera i Lakatosa, z kolei do rozwiązań eksternalistycznych koncepcje wczesnego Kuhna, Feyerabenda, Szkoły Edynburskiej i różne koncepcje postmodernistyczne. Kategorie ‘internalizmu’ i ‘eksternalizmu’ podlegają jednak gradacji orzekalności w zależności od stopnia uwzględnienia czynników pozaracjonalnych. Sytuacja jest analogiczna jak w przypadku orzekalności kategorii ‘racjonalizmu’ i ‘irracjonalizmu’.

Uwaga 7. W toczonym w neokantowskiej perspektywie sporze o charakter referencjalny pojęć teoretycznych (zarówno nauki przedmiotowej, jak i metanauki) Życiński opowiada się po stronie umiarkowanego realizmu. Opowiada się za umiarkowanym realizmem teoretycznym nie tylko w odniesieniu do języka nauk empirycznych, lecz także w odniesieniu do języka metanauki. Umiarkowany realizm teoretyczny oznacza, że terminy teoretyczne oprócz aspektu konstruktywnego posiadają także aspekt realno-opisowy (referencyjny). W przypadku kategorii metanaukowych znaczy to, że odnoszą się one referencjalnie do *racjonalnej rzeczywistości* metanauki. Referencja terminów teoretycznych według realizmu umiarkowanego jest jednak tylko

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[57] W (Życiński, 1993, s. 242) pada bardziej lakoniczne określenie *internalizmu* jako ujęcia kładącego nacisk na racjonalność nauki. Z kolei określenie *eksternalizmu* jest bardziej radykalne: oznacza ujęcia traktujące naukę jako „wynik *zewnętrznych* pozaracjonalnych czynników”.

pośrednia. Nie można *wskazać* bezpośrednio na ich referenty. W przypadku terminów metanaukowych można jedynie wskazać ich historycznie zmienne *przejawy* pod postacią *różnych teorii* lub *koncepcji*.

Uwaga 8. Istnienie racjonalności obiektywnej jest zatem *odmiennym sposobem istnienia* od istnienia przedmiotów empirycznych. To już kwestia ontologii przyjmowanej przez Życińskiego. Wydaje się, że jego koncepcja sposobów istnienia „przedmiotów” będących desygnatami teoretycznych terminów metanauki bliska jest partycypacyjnej ontologii neoplatońskiej będącej próbą pogodzenia ontologii Arystotelesa i Platona. Racjonalność obiektywna mająca swe źródło w samym Bogu obecna jest *implicite* w świecie i w ludzkiej myśli, a ujawnia się poznawczo nie wprost, lecz w sposób uwikłany w historii tejże myśli ludzkiej. Jej poznanie wymaga odpowiedniej hermeneutyki.

W tej perspektywie koncepcja Lakatosa jest mniej radykalnie internalistyczna niż koncepcja Poppera, a ta z kolei mniej niż koncepcje Koła Wiedeńskiego, gdyż w różnym stopniu uwzględniają one rolę czynników zewnętrznych. Podobnie koncepcja Kuhna wydaje się być mniej radykalnie eksternalistyczna niż mocny program socjologii wiedzy[58].

W przypadku eksternalizmu Życiński rozróżnia (por. 1996, s. 135) między eksternalizmem umiarkowanym a skrajnym. Pierwszy nie neguje elementu racjonalnego w rozwoju nauki, lecz traktuje rozwój nauki jako wypadkową elementów racjonalnych i czynników pozaracjonalnych. Z kolei eksternalizm skrajny traktuje wewnętrzną racjonalność nauki jako funkcję samych tylko zewnętrznych czynników psycho-społecznych. W *Strukturze* funkcjonuje nieco inny po-

[58] Życiński pisze przykładowo (1988b, s. 135; 2013, s. 239): „Asercje o *różnym stopniu racjonalności* [podkreślenie moje] pojawiają się w obrębie poszczególnych programów, a nawet w obrębie poszczególnych teorii”. Pisze też, że wiele umiarkowanych tez Kuhna zawartych w jego *Strukturze rewolucji naukowych* zostało zradykalizowanych w socjologicznej interpretacji nauki (zob. Życiński, 1996, s. 207n).

dział na trzy możliwe rodzaje rozwiązań eksternalistycznych (1988b, s. 123n; 2013, s. 218). Pierwszy typ rozwiązań ogranicza się do uznania czynników pozaracjonalnych za pozaracjonalne *inspiracje* występujące wyłącznie w kontekście odkrycia, drugi mówi o *niezwykle ważnej roli* tychże czynników (elementów) *także* w okresie rewolucji naukowej, natomiast trzeci uznaje *dominującą* rolę tych czynników nad elementami racjonalnymi zarówno w okresie powstania, jak i rozwoju programu badawczego. To ostatnie rozwiązanie Życiński z góry odrzuca jako nieracjonalne.

Życiński poddaje szczegółowej analizie krytycznej wszystkie najważniejsze dwudziestowieczne rozwiązania racjonalistyczne (internalistyczne) i sceptyczne (eksternalistyczne). W jednym i drugim przypadku pokazuje ich wewnętrzne ograniczenia, a wielokrotnie wewnętrzne sprzeczności. Tego typu analizy zajmują znaczną część jego twórczości metanaukowej. Nawet pobieżna ich prezentacja musiałaby zająć zbyt wiele miejsca. Ograniczmy się zatem do konkluzji, jakie wyciąga on z tej analizy oraz do kilku przykładów. Konkluzje formułuje następująco:

> Długie dyskusje na temat natury wiedzy naukowej pokazały, że nauka nie jest ani tak racjonalna, jak chciał tego młody Popper, ani tak socjologicznie uwarunkowana, jak twierdził Kuhn w pierwszym wydaniu *Struktury rewolucji naukowej* (Życiński, 1988b, s. 145; 2013, s. 256).

W innym miejscu tę samą ideę rozwija w typowym dla siebie retorycznym stylu:

> Świadomość epistemologicznych ograniczeń nauki oraz uwarunkowania historyczne wpływające na niektóre determinanty racjonalności nie dają zatem żadnych podstaw do stwierdzenia, że wszystkie przekonania dotyczące wewnętrznej racjonalności nauki są przejawem nierealnych marzeń.

> Niewątpliwie takimi marzeniami były maksymalistyczne idee filozofów epoki Wiktoriańskiej, którzy na podobieństwo wcześniejszych metafizyków poszukiwali racjonalności sekretnej i tajemnej. Z faktu, że ich nadzieje okazały się płonne, nie wynika jednak, że nauka jest irracjonalna. Można jedynie powiedzieć, że [sama] racjonalność jest różna od tego, czego oczekiwano. Upraszczające zastępowanie racjonalności socjologią może okazać się równie nierealnym, marzycielskim przedsięwzięciem (1988b, s. 123; 2013, s. 217).

W dychotomicznym ujęciu stanowisk metanaukowych po rewolucji naukowej Popper zajmuje miejsce internalizmu, Kuhn miejsce eksternalizmu. Krytykę pierwotnej koncepcji dedukcjonizmu Poppera jako teorii nazbyt optymistycznej Życiński przejmuje od Lakatosa (zob. Życiński, 1983, s. 120n). Od siebie dodaje, że „wierze w racjonalność nauki i w potęgę falsyfikacji był również zawarty element uproszczenia idealizujący procedury stosowane w rzeczywistej nauce" (Życiński, 1996, s. 230). Przykładowo, w historii nauki istnieje wiele modyfikacji *ad hoc*, które podobnie jak modyfikacja Einsteina miały pozytywny wpływ na rozwój nauki, i to wbrew *irracjonalności* postulowanej przez Popperowską teorię falsyfikacji (zob. Życiński, 1996, s. 108). Przypadek Poppera i jego koncepcję modyfikacji *ad hoc* można zatem zinterpretować jako specyficzny fakt metafilozoficzny, który daje Życińskiemu do myślenia. Popperowska teza o irracjonalności modyfikacji *ad hoc* nie wynika logicznie z faktów opisujących rzeczywiste zachowania naukowców, lecz okazuje się wyrazem nazbyt wyidealizowanej i nazbyt apriorycznej koncepcji racjonalności. Koncepcja Poppera jest zatem wyrazem silnych założeń filozoficznych i jako taka wskazuje na obecność zewnętrznych uwarunkowań Poppera. W terminologii Życińskiego ten subiektywny czynnik wyboru i związania z tradycją filozoficzną przyjmuje postać optymistycznej *wiary* Poppera w racjonalność nauki i w potęgę falsyfikacji[59].

Analogicznej krytyce poddana zostaje koncepcja Kuhna i jego radykalnie eksternalistyczna interpretacja *związania z paradygmatem* z 1962 roku. W perspektywie tej interpretacji, jak zauważa Życiński (1983, s. 106). logika nauki zostaje niemal całkowicie zastąpiona socjologią wiedzy. Tymczasem bliższa analiza historyczna pokazuje, że w dłuższej perspektywie możliwe jest racjonalne wykazanie wyższości jednego programu badawczego nad innym gdyż, wewnętrzna logika rozwoju nauki jest silniejsza od emocjonalnych przywiązań do poszczególnych teorii (1983, s. 165). Z kolei w (1996, s. 195) stwierdza, że rzeczywista nauka obala tezę o dogmatycznym charakterze związania z paradygmatem, gdyż „zmiana paradygmatu jest nie tylko możliwa teoretycznie, lecz również zachodzi rzeczywiście w nauce".

Najsilniejszym argumentem przeciw skrajnie eksternalistycznej interpretacji związania z paradygmatem jest argument na rzecz epistemologicznej *konieczności* takiego związania w sytuacji, gdy niemożliwe jest precyzyjne rozstrzygnięcie pomiędzy alternatywnymi tradycjami badawczymi. W takiej sytuacji jedyną alternatywą dla idei podmiotowego *commitment* byłby jedynie sceptycyzm. Ten natomiast, jak pamiętamy, jest z góry wykluczony, gdyż nie jest reakcją racjonalną na fakt czynników pozaracjonalnych w nauce. Zatem zgoda na podmiotowy *commitment* jest jedynym racjonalnym rozwiązaniem w tej sytuacji[60].

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[59] W tym kontekście Życiński przywołuje też przykład naukowych reakcji na wynik eksperymentu EPR czy na odkrycie nierówności Bella, których nie potraktowano jako falsyfikatory, jak to powinno się stać w myśli koncepcji Poppera.

[60] Jest to argument rozwijający ideę epistemologicznej zasady niepewności. Zob. poprzedni punkt.

Skoro ani skrajny internalizm, ani skrajny eksternalizm nie wytrzymują krytyki, to Życiński konkluduje o konieczności zanegowania sensu zaistniałej dychotomii wśród tych rozwiązań. Co więcej, uważa że rozwianie marzycielskich tendencji metanaukowych skrajnego internalizmu i skrajnego eksternalizmu dokonało się nie tyle za sprawą wewnętrznych analiz metanaukowych, ile za sprawą samej nauki, która nie dała się wtłoczyć w sztywne ramy ideologiczne[61]:

> W tej sytuacji [gdy programy badawcze i teorie cechują się różnym stopniem racjonalności] próby absolutyzowania pojęcia racjonalności, jak również skłonność do dychotomicznego dzielenia interpretacji [metanaukowych] na racjonalne i irracjonalne stanowią wyraz pewnej filozofii bronionej w sposób dogmatyczny, niemniej sfalsyfikowanej przez samą naukę (Życiński, 1988b, s. 135; 2013, s. 239).

Konfrontacji z rzeczywistą nauką nie oparł się ani internalizm, ani eksternalizm. Dokładniejsza obserwacja faktycznej nauki pokazuje, „że rzeczywisty rozwój nauki przebiega w odmienny sposób niż sugerowali to przedstawicie normatywnych metodologii" (Życiński, 1996, s. 230). Z kolei eksternalizm radykalny wikła się w nieusuwalne „antynomie" w wyjaśnianiu faktów metateoretycznych z dziejów nauki[62].

Dychotomia rozwiązań metanaukowych będących reakcją na rewolucję naukową jest faktem. Pytanie, czy jest nieunikniona? Życiński podejmuje się pokazać, że ma ona jedynie charakter historyczny, a zatem niekonieczny. Możliwe jest unieważnienie sporu internalizmu z eksternalizmem. Powstaje pytanie, jak tego dokonać?

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[61] Jest to przykład sprzeczności transcendentalnej rozumianej jako sprzeczność metanaukowych hipotez z faktami metanaukowymi.

[62] Poświęcony im jest cały podrozdział w (Życiński, 1996, s. 147–156).

W myśl nadrzędnej zasady racjonalności – występującej *implicite* choćby w przytoczonym przed chwilą argumencie na rzecz racjonalności podmiotowego *commitment* w sytuacjach wyborów alternatywnych – skoro rzeczywista nauka unieważnia oba człony metanaukowej alternatywy *internalizm versus eksternalizm*, to racjonalną reakcją na tę sytuację nie może być sceptycyzm, lecz nowe zmodyfikowane rozwiązanie racjonalne.

Z powyższych tekstów można wnioskować (poszukując ich ukrytych presupozycji), że kategorie ‘internalizmu’ i ‘eksternalizmu’ służą do sproblematyzowania metanaukowej idei *normatywnej demarkacji elementów wewnętrznych (racjonalnych) i zewnętrznych (pozaracjonalnych) w rozwoju nauki*[63] jako szczególnego przypadku problemu *racjonalności jako takiej*. Nowe rozwiązanie może potraktować ten spór jako ślepą odnogę ewolucji pojęcia racjonalności.

Falsyfikacja teorii metanaukowych przez fakty metateoretyczne z rzeczywistej nauki nie implikuje ich całkowitej bezwartościowości i konieczności ich odrzucenia oraz zaproponowania całkiem nowych teorii metanaukowych. Życiński preferuje bardziej umiarkowaną strategię rozwoju teoretycznego w metanauce, zaczerpniętą od późnego Poppera i od Lakatosa. Wystarczy dokonać jedynie niezbędnych modyfikacji w dotychczasowych rozwiązaniach metanaukowych.

Modyfikacje w przekonaniu Życińskiego powinny polegać na „liberalizacji” radykalnych postaw metanaukowych po obu stronach sporu. Wprawdzie, należy przyznać rację Lakatosowi, że „żadne kryterium demarkacji nie ma charakteru absolutnego” (Życiński, 1996,

[63] Przypomnienie: treść idei, podobnie jak treść pojęć i koncepcji, dla ułatwienia lektury, podaję kursywą.

s. 230)[64], ale nie znaczy to, że należy odejść od metodologii normatywnej, lecz jedynie to, że należy rozwinąć i zliberalizować te normy, które doprowadziły do powstania wyidealizowanej koncepcji nauki. Podobnie, chcąc uniknąć antynomii eksternalizmu radykalnego „trzeba przyjąć jego umiarkowany wariant, który głosi, iż elementy racjonalne oraz zewnętrzne uwarunkowania psychospołeczne ‘przenikają się’ wzajemnie i uzupełniają w procesie rozwoju nauki” (Życiński, 1996, s. 156).

W obu przypadkach przyczyną (a zatem czynnikiem pozaracjonalnym) pojawienia się błędnych rozwiązań radykalnych była nieuświadomiona tendencja do nadmiernego *upraszczania* procedur naukowych na poziomie wyjaśniania metanaukowego i do idealizacji[65]. Tendencja ta stanowi cechę charakterystyczną „koncepcji niedojrzałych”, mitologicznych, opierających się na jednym uniwersalnym czynniku (przyczynie) mającym wyjaśnić złożoną rzeczywistość. Zarówno skrajny internalizm, jak i skrajny eksternalizm można zatem uznać w perspektywie analiz Życińskiego za metanaukową wersję myślenia mitologicznego na kształt mitu ostrej demarkacji przypisywanej Popperowi. Jako takie mają one jednak charakter wyłącznie historyczny. Według Życińskiego, długie spory o kryterium demarkacji doprowadziły do uświadomienia sobie idealizacyjnych uproszczeń i do sukcesywnego „odkrycia złożonej prawdy o bogactwie procedur badawczych i wzajemnych uwarunkowań występujących w *realnej*

[64] Ale w tym samym rozdziale (1996, s. 244) Życiński uznaje Lakatosa za przedstawiciela nazbyt wyidealizowanej, normatywnej koncepcji nauki.

[65] Życiński zarzuca mu nadmierną idealizację kwestii związania z paradygmatem (zob. 1983, s. 152).

nauce" (Życiński, 1996, s. 230). Dzięki temu odkryciu możliwe jest obecnie „wypracowanie *bardziej realistycznych* wzorców metanaukowych" (s. 237)[66].

Najlepszym kandydatem teoretycznym do poprawnego ujęcia racjonalnego rozwoju nauki jest zdaniem Życińskiego odpowiednio zliberalizowana koncepcja Lakatosa *naukowych programów badawczych* (zob. J. Życiński, 1996, s. 244n) (Życiński, 1996, s. 244n)[67]. Liberalizacja polega na rezygnacji z absolutyzowania norm metodologicznych wskazanych przez Lakatosa i uznaniu ich tylko za *przybliżony wzorzec* racjonalnych zachowań naukowych, adekwatny jedynie w odniesieniu do *określonych okoliczności*:

> Przejawem metanaukowego dogmatyzmu byłoby traktowanie normatywnej metodologii Lakatosa w tym samym stylu, w jakim przed laty narzucano nauce kanony Koła Wiedeńskiego. [...] Jego [tzn. Lakatosa] propozycje zdają się zawierać najbardziej rozwiniętą syntezę krytycyzmu i racjonalności na poziomie refleksji metanaukowej. Niezależnie od emocjonalnych deklaracji autora [...] metodologia programów badawczych, w porównaniu z konkurencyjnymi propozycjami, stwarza najlepszą szansę racjonalnej rekonstrukcji nauki i racjonalnego wyboru rywalizujących teorii. Metodologię tę można odrzucić a priori, np. z racji aksjomatycznego przyjęcia tezy o irracjonalnych mechanizmach rozwoju nauki. Jeśli jednak unika się równie radykalnej aksjomatyki, można z ostrożnością i krytycyzmem F. Suppego «skonkludować, iż Lakatos ukazał częściowo pewien wzorzec rozumowania w rozwoju wiedzy naukowej, który to wzorzec – w określonych okolicznościach – charakteryzuje poprawne rozumowanie naukowe» [*Afterword*, 670]. Konkluzja ta, a zwłaszcza położony w niej akcent

[66] W obu cytatach kursywa pochodzi ode mnie. Wskazuje ona na specyficzny charakter metodologii metanauki Życińskiego.

[67] Za przykład pozytywnych modyfikacji podaje on prace J. Waralla, E. Zahara, P. Urbacha, czy J. Watkinsa.

> na cząstkowy charakter opracowań Lakatosa, nie oznacza absolutyzowania przedstawionych opracowań metanaukowych, lecz wskazuje na potrzebę dalszych poszukiwań umożliwiających pełniejszą charakterystykę racjonalnych struktur nauki (Życiński, 1996, s. 245n)[68].

Zgodnie z tymi deklaracjami, proponowane przez Życińskiego rozwiązanie metanaukowe nie posiada charakteru prostej i precyzyjnej, uporządkowanej logicznie (dedukcyjnej) teorii metanaukowej wychodzącej od kilku prostych aksjomatów metanaukowych. Raczej jest to pewien niedookreślony logicznie obraz metodologiczny nauki składający się z wielu stosunkowo luźno powiązanych reguł metanaukowych. Do istotnych elementów rozwiązania Życińskiego należy zaliczyć jego koncepcję ideatów i ideologicznych programów badawczych, mających na celu wykazać zasadniczą ciągłość i tym samym racjonalność w historycznym rozwoju nauki pomimo występowania rewolucji naukowych i związania naukowców z tradycją badawczą. Do istotnych elementów należą również trzy zasady: zasada aracjonalności i naturalności interdyscyplinarnej oraz epistemologiczna zasada niepewności. Ich celem jest racjonalna normalizacja funkcji czynników pozaracjonalnych w nauce tak, by zachowana została nadrzędna funkcja elementu racjonalnego. Wśród tych elementów nie można zabraknąć również rozróżnień wielu typów racjonalności oraz idei ewolucji pojęcia racjonalności. Dopiero omówienie wszystkich tych elementów pozwoli poznać doksatyczny charakter proponowanego przez Życińskiego rozwiązania. Zagadnienia te ze względu na swą obszerność zostaną przedstawione w kolejnym artykule (zob. przypis nr 5 niniejszej pracy).

* * *

[68] Cytat w tekście pochodzi z posłowia Fredericka Suppego do znanego zbioru tekstów (Suppe, 1974).

Na zakończenie tej części chciałbym powrócić do fundamentalnej idei metafilozoficznej Życińskiego, jaką jest postulat, by metanaukowe i filozoficzne koncepcje na temat natury i struktury nauki odnosiły się do *rzeczywistej* nauki względnie do *rzeczywistości* nauki (zob. 1996, ss. 16 i 126) a nie do wyidealizowanych i uproszczonych konstrukcji filozofów. Idea ta pozwala zrozumieć nacisk, jaki nasz autor kładzie na rolę czynników pozaracjonalnych w nauce oraz na rolę tej kategorii metanaukowej w rozwiązaniach metanaukowych, a jednocześnie pozwala zrozumieć jego racjonalistyczny optymizm w obliczu różnych skrajnych interpretacji eksternalistycznych.

Mówiąc o tradycji badania realnej lub rzeczywistej nauki Życiński wskazuje Kuhna i Polanyiego jako prekursorów tego trendu (Życiński, 1983, s. 153.169; 1996, s. 231). Jeden i drugi kojarzą się wszakże jednoznacznie z ideą czynników pozaracjonalnych w nauce. To pokazuje, że w jego rozumieniu *rzeczywista nauka* lub *rzeczywistość nauki* to nie tylko logiczna konstrukcja językowa w perspektywie metody uzasadnienia, lecz także cały kontekst osobowy, w jakim konstrukcje teoretyczne funkcją. Co więcej, nie wystarczy tradycyjne rozwiązanie racjonalistyczne, by kontekst osobowy utożsamić z psychologicznym kontekstem odkrycia. Bez uwzględnienia czynników pozaracjonalnych także w kontekście uzasadnienia i akceptacji nie jest w ogóle możliwe pełne zrozumienie samej natury nauki.

Radykalny charakter tych stwierdzeń najlepiej ilustrują same wypowiedzi Życińskiego. W *Elementach* pisze, że „pomijanie roli czynników pozaracjonalnych w nauce prowadzi do wyidealizowanego i nierealistycznego obrazu struktur nauki", a nieco dalej, że „uwzględnienie ich roli stanowi *konieczny warunek* pełniejszego *zrozumienia istoty* nauki" (Życiński, 1996, s. 189.190, podkreślenia moje) Czynniki pozaracjonalne zostają powiązane w sposób nierozerwalny z naturą nauki. Jest to rzeczywista rewolucja metanaukowa.

W tej sytuacji trudno dziwić się praktyce Życińskiego podkreślania obecności czynników pozaracjonalnych w nauce i w metanauce za pomocą specyficznej, psychologicznie naznaczonej terminologii, jak choćby wspomnianej kategorii ‘szoku’. Życiński nie obawia się jednak posądzenia o psychologizm, gdyż wypowiada te słowa z wnętrza szeroko rozumianej tradycji *episteme*, z którą uważa się związany[69]. A ponieważ dotychczasowa tradycja *episteme* cechowała się nazbyt racjonalistycznym i przeidealizowanym podejściem do nauki, to podkreślanie roli czynników pozaracjonalnych wydaje się w pełni uzasadnione.

Także związanie z tradycją *episteme* przejawia się u Życińskiego specyficzną praktyką językową. Tym razem przeciwną w stosunku do poprzedniej. Gdy wypowiada się na temat nieusuwalnej obecności czynników pozaracjonalnych w nauce, dodaje od razu dla przeciwwagi, że obecność ta nie implikuje w żadnym wypadku konkluzji o *dominacji subiektywizmu* w nauce (J. Życiński, 1996, s. 187). Podobne zastrzeżenia czyni mówiąc o roli czynników pozaracjonalnych w poznaniu natury nauki:

> Obecność w nauce wiedzy osobowej oraz nieskonceptualizowanych intuicji nie upoważnia do kwestionowania wewnętrznej racjonalności nauki. Uświadamia ona natomiast, iż racjonalna refleksja, która inspirowała zarówno „Elementy” Euklidesa, jak i „Principia” Newtona, łączy się w kontekście rozwoju nauki z pozaracjonalnymi czynnikami psychospołecznymi. Uwzględnienie ich roli stanowi konieczny warunek pełniejszego zrozumienia istoty nauki (Życiński, 1996, s. 190). (J. Życiński, 1996, s. 190).

[69] Zob. (1996, s. 126), gdzie Życiński zalicza siebie do nurtu epistemologii kontynuującego platońsko-arystotelesowską tradycję *episteme*.

Natura nauki z perspektywy tradycji *episteme* jest zatem zasadniczo racjonalna, ale element racjonalny w rzeczywistym świecie zawsze występuje w kontekście osobowego *commitment*. Czysty rozum naukowy jest filozoficznym mitem. Nie jest to jednak w żadnym wypadku kapitulacja rozumu. Zgodnie z epistemologiczną zasadą niepewności w sytuacji, gdy mamy do czynienia z wyborem alternatywnym albo – albo, gdzie jednym z członów jest uznanie istotnej, ale nie dominującej roli czynników pozaracjonalnych w nauce, a drugim eksternalistyczny sceptycyzm, to wybór rozwiązania uwzględniającego rolę czynników pozaracjonalnych w nauce jest jedynym racjonalnym rozwiązaniem.

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How Many Kingdoms of Life? Eukaryotic Phylogeny and Philosophy of Systematics

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Abstract

According to contemporary understanding of the universal tree of life, the traditionally recognized kingdoms of eukaryotic organisms – Protista, Fungi, Animalia and Plantae – are irregularly interspersed in a vast phylogenetic tree. There are numerous groups that in any Linnaean classification advised by phylogenetic relationships (i.e. a Hennigian system) would form sister groups to those kingdoms, therefore requiring us to admit them the same rank. In practice, this would lead to the creation of ca. 25-30 new kingdoms that would now be listed among animals and plants as "major types of life". This poses problems of an aesthetic and educational nature. There are, broadly speaking, two ways to deal with that issue: a) ignore the aesthetic and educational arguments and propose classification systems that are fully consistent with the Hennigian principles of phylogenetic classification, i.e. are only composed of monophyletic taxa; b) ignore Hennigian principles and bunch small, relatively uncharacteristic groups into paraphyletic taxa, creating systems that are more convenient. In the paper, I present the debate and analyze the pros and cons of both options, briefly commenting on the deeper, third resolution, which would be to abandon classification systems entirely. Recent advances

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in eukaryotic classification and phylogeny are commented in the light of the philosophical question of the purpose and design principles of biological classification systems.

Keywords

taxonomy; systematics; philosophy of biology; eukaryotes; Protista.

Ever since Linnaeus formalized biological classification in his *Systema Naturae* (Linnaeus, 1788), there is the unresolved problem of how exactly should new taxa be created, and, more specifically, into how many subtaxa should higher-rank taxa be split. For instance, is there a convincing reason why there should be, say, 43 orders of ray-finned fish (class Actinopterygii), but only 3 orders of leeches (class Hirudinea)? Are there any objective arguments for one arrangement over another, or is it purely a matter of taste? There is a considerable body of work in philosophy of biology that discusses such issues (see Hull, 1965; 1970; Schuh and Brower, 2011 and especially Mayr and Bock, 2002).

While ongoing changes at lower ranks (i.e. species, genus, family and order) are of high importance to specialists, the public is much more likely to encounter changes at the higher ranks (from class up). Of special importance is the traditionally highest category of kingdom (now partially superseded by domain – see below) which delineates major "types of life" and is of enormous educational and heuristic value (Copeland, 1938; Cavalier-Smith, 1981). For centuries there has been slow incremental change in the classification of life at the level of kingdom. In the recent two decades, however, the rapidly increasing knowledge of the general topology of the universal tree of life, lead to the realization that traditionally distinguished kingdoms are interspersed in a thick "bush" of major branches (see e.g. Baldauf et al., 2000). In this paper, I wish to analyze this situation, with special attention to the structure of the eukaryotic tree of life.

1. The rank of kingdom

In biological systematics, starting from Linnaeus, the rank of kingdom has long been the highest of all taxonomic ranks. In Linnaeus' *Systema Naturae*, there were 3 of them – the animal, plant and mineral kingdom (Linnaeus, 1788), and the division of all living things into two kingdoms was retained in biology for a long time. Only recently the even higher rank of domain was introduced by Carl Woese (Woese, Kandler and Wheelis, 1990), who intended to accentuate the special character of Archaea, famously identified by him and George Fox (Woese and Fox, 1977) to be fundamentally different from both bacteria and eukaryotes; although it has been recently suggested that eukaryotes actually evolved from archaea (Williams, Foster et al., 2013). Even in Woese's 1990 revision, however, three "primary kingdoms" were named with the idea that the rank of kingdom remains the basic level at which major "types of life" are to be identified.

The number of the kingdoms of life discerned by biologists changes through time rather slowly. A contemporary student of both introductory and advanced biological texts is almost certain to encounter the following:

- either a single kingdom of bacteria, variously called Prokaryotae, Bacteria or Monera, or, in a more modern variation, two kingdoms: the so-called true bacteria (Eubacteria, sometimes Bacteria) and the archaeans (Archaea, sometimes Archaebacteria);

- the single kingdom of protists (usually Protista or Protoctista), which encompasses all eukaryotes not belonging to the three multicellular kingdoms (see below); in some modern sources (Ruggiero et al., 2015) there is a separate kingdom Chromista encompassing a large monophyletic subgroup of protists, including brown algae which are large, multicellular and quite complex;
- the three multicellular eukaryotic kingdoms: plants (Plantae, sometimes Viridiplantae), animals (Animalia, sometimes Metazoa) and fungi (Fungi, sometimes Mycota).

Note that the list is quite conservative, but the number of kingdoms slowly increases. Plants and animals are the two original Linnaean kingdoms of life. Protists were argued to be a separate kingdom by Ernst Haeckel in 1866 (Haeckel, 1866). Bacteria were given a separate kingdom in 1938 (Copeland, 1938) and, interestingly, the fungi have been proposed as a kingdom separate from plants only in 1969 (Whittaker, 1969). The distinct kingdom for Archaea, as mentioned, was proposed in 1977 and Thomas Cavalier-Smith first proposed elevating Chromista to the rank of kingdom in 1981 (Cavalier-Smith, 1981).

Arguably, the most common variant found in introductory texts is the one with five kingdoms: Bacteria, Protoctista, Plantae, Animalia, Fungi, and this very division has been employed in the highly influential popular book by Lynn Margulis and Michael Chapman, *Kingdoms and Domains* (Margulis and Chapman, 2009), which has become an authoritative source on biological megasystematics. In a recent proposal of a comprehensive classification of life, down to the level of order (Ruggiero et al., 2015), there are seven kingdoms: Archaea/Archaebacteria, Bacteria/Eubacteria, Protozoa, Chromista, Fungi, Plantae and Animalia. This proposal is a consensus view of over 3000 taxonomists (!) and is likely to become the standard point of reference for professional biologists for years to come, even though it has been criticized, especially with regard to specific taxonomic decisions (e.g. Tedersoo et al., 2018).

All in all, based on that quick review, it may seem that a general view of the diversity of life, and its representation in biological taxonomy, is now more or less understood, and major changes to the number of kingdom occur rarely. In reality, it not that simple. In the past decades there has been considerable controversy surrounding the number and identity of the highest ranks in systematics, and the summary presented above might be termed the "conservative view" by some. In fact, there are sources claiming that 11-12 prokaryotic kingdoms (Petitjean et al., 2014) and 20-27 eukaryotic kingdoms (Pawlowski, 2013; Tedersoo, 2017) should be distinguished.

The purpose of the present paper is to identify the cause of that confusion and its possible resolutions, using mostly examples related to the eukaryotic tree of life.

2. Paraphyletic taxa and classification systems

Starting from Darwin himself, it has become increasingly clear that biological systematics must somehow portray evolutionary patterns. This was formalized in Willi Hennig's *Phylogenetic Systematics* (Hennig, 1966) where the now-ubiquitous concepts of symplesiomorphy, synapomorphy and convergence, and the corresponding types of taxa – paraphyletic, monophyletic and polyphyletic – were formalized. Hennig forcefully argued that valid taxa should only be monophyletic groups, i.e. (true) clades, that is groups of species composed of *all* descendants of a given species, possessing a common derivative character (synapomorphy). In the science of cladistics this has since become gospel.

While admirable, this prescription leads to "taxonomical inflation" as more and more taxa are identified. Before we illustrate it with an example relevant for the present study, let's consider a case with more familiar taxa.

Vertebrates (subphylum Vertebrata of phylum Chordata) are divided into a number of classes. A popular list of vertebrate classes that you may hear even from a child (the specific reason for mentioning children in this context will be given below) is as follows: fish, amphibians, reptiles, birds and mammals. A better-educated person might cite the current scientific consensus that "fish" should be actually split into a number of separate classes: hagfish (Myxini), lampreys (Hyperoartia), cartilaginous fish (Chondrichthyes), ray-finned fish (Actinopterygii) and lobe-finned fish (Sarcopterygii), the reason being that "fish" is actually a paraphyletic grouping. This prescription to discuss five types of fish, which a layperson may identify as unnecessary and confusing (why five classes of fish and just one class of birds?), is a first sign of what happens when one attempts to divide a given taxon into monophyletic subtaxa.

If one were to portray the abovementioned vertebrate classes on a phylogenetic tree, something like Fig. 1 might be a reasonable representation of actual relationships.

This is, however, yet another simplification. A more careful analysis of *any* segment of the vertebrate phylogenetic tree will reveal that in between the well-known groups there are numerous groups, usually extinct, that would all require classes of their own, if the bordering taxa are given the rank of class – consult Fig. 2.

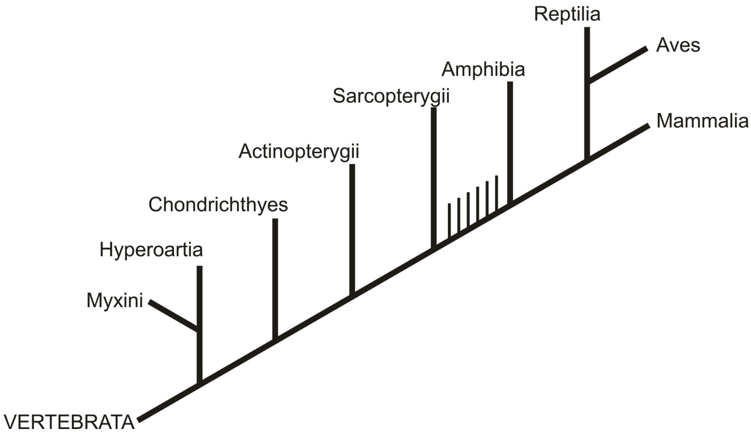

Figure 7: A highly simplified, and ultimately wrong, phylogenetic tree of extant vertebrates that attempts to include only the extant classes of subphylum Vertebrata. The 'comb' represents the area magnified in Fig. 2. It is clear that one cannot map a ranked classification of extant vertebrate classes onto a valid phylogenetic tree, especially because of the existence of extinct groups. Specifically, it is impossible to validly represent the actual relationships between (paraphyletic and now obsolete) Reptilia and Aves (which nest within reptiles).

It is clear that in order to divide the taxa listed in Fig. 2 into classes, one would have to give each genus presented in that figure its own high-level taxon (here: most likely class). The alternative is accepting paraphyletic taxa, for instance by grouping all stem tetrapods (Tiktaalik...Crassigyrinus) in a single class. It clearly goes against phylogenetic systematics, which is usually defended by biologists working with specific groups of organisms as the only method of creating taxonomies that is commensurate with the theory of evolution (see e.g. Williams and Kociolek, 2007)

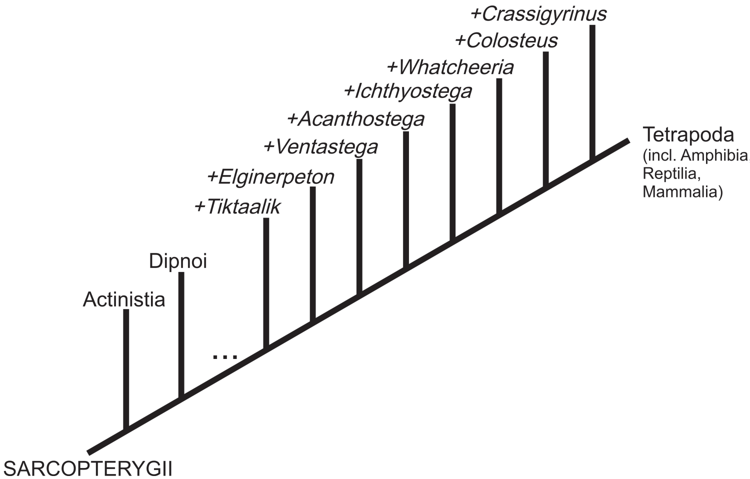

Figure 8: A slightly more realistic representation of a segment of the vertebrate phylogenetic tree, based on (Swartz, 2012) (with many taxa removed, most notably a large group of early sarcopterygians, denoted by an ellipsis). The improvement over Fig. 1 is mostly in that: a) the term Sarcopterygii is now correctly represented to also include all tetrapods, while in common usage, represented in Fig. 1, it refers only to coelacanths (which belong to Actinistia) and lungfish (which belong to Dipnoi); b) a number of extinct (marked by a cross) genera are presented.

There are ways to avoid that, one of which is the so-called sequencing convention proposed by Nelson (1972) and developed by Wiley (1981) and others, which partly automates the ranking process in various branches of the phylogenetic tree. But for new, let's assume that we want to hold on to strict Hennigian principles; what would be the consequences? Quite simple: the necessity to create a complete system of monophyletic taxa, in case of vertebrates, would lead to the erection of tens, if not hundreds, classes of vertebrates which defeats the simplifying purpose of classification. Another solution would be to erect a large number of taxa of intermediate rank – this has been done, for example, by McKenna & Bell in their influential classification of mammals (McKenna and Bell, 1997):

- class Mammalia
    - subclass Prototheria
    - subclass Theriiformes
        - infraclass +Allotheria
        - infraclass +Eutriconodonta
        - infraclass Holotheria
            - superlegion +Kuehneotheria
            - superlegion Trechnotheria
                - legion +Symmetrodonta
                - legion Cladotheria
                    - sublegion +Dryolestoidea
                    - sublegion Zatheria
                        - infralegion +Peramura
                        - infralegion Tribosphenida
                        - ...

It is worth noting that the authors of that classification are paleontologists and their system is clearly intended as an answer to the problem of finding a proper taxonomic space for extinct groups. As a result, a staggering number of ranks were created. In "core Linnaean" taxonomy classes are composed of orders, i.e. class and order are neighboring ranks. In McKenna & Bell's system one will find between them: subclasses, infraclasses, superlegions, legions, sublegions, infralegions, supercohorts, cohorts, magnorders, superorders, grandorders and mirorders. The term "taxonomic inflation" may thus have two meanings: the creation of an impractically large number of equally-ranked taxa and the creation of an impractically large number of ranks.

Let's now discuss these two alternatives in more detail.

3. The solutions

Solution I: classifications with only monophyletic taxa

As mentioned above, strictly adhering to Hennig's prescription to accept only monophyletic taxa leads to "taxonomic inflation" – which seems to go against the aesthetic intuition of those biologists who dread the idea of splitting a given, let's say, phylum, into 50 classes (Cavalier-Smith, 1998).

Surprisingly, the solution that has been commonly employed is to do something perhaps even more drastic: to drop altogether the Linnaean "logic" of classification which is based on the simple idea that taxa of a given rank are divided into a number of subordinate taxa of the same lower rank – i.e. kingdoms are divided *only* into phyla; phyla are divided *only* into classes; classes *only* into orders etc. In other words, every order belonging to a given phylum must *also* belong to a certain class (or, in some cases, be included as *incertae sedis*, i.e. temporarily awaiting class attribution). In many modern classifications, however, there are *missing ranks*. As an example, consider the classification of vertebrates presented by the eminent paleontologist Michael Benton in his *Vertebrate Palaeontology* (Benton, 2014, p.433nn)

- superclass Tetrapoda:

○ family Elginerpetontidae

○ *Ventastega*

○ *Acanthostega*

○ *Ichthyostega*

○ *Tulerpeton*

○ family Colosteidae

○ family Crassigyrinidae

○ family Whatcheeriidae

○ family Baphetidae

○ class Neotetrapoda [where amphibians, reptiles, birds and mammals can be found – L.L.]

This is a remarkable solution. Note that it clearly opposes the centuries-old tradition, formalized by Linnaeus, to create consistent, complete hierarchies. Here we have a superclass that is composed of a single class, 5 families and 4 genera.

The missing ranks may look unsettling at first, but this convention is quickly gaining popularity, as it seems to offer a welcome rescue from the otherwise inescapable alternatives discussed in the previous paragraph. A recent classification of eukaryotes, published first in 2005 (Adl, Simpson, Farmer et al., 2005), then in a revised form in 2012 (Adl, Simpson, Lane et al., 2012), employs precisely that methodology. Note that this is an extremely well-respected classification, created by a consortium that includes world-class experts in eukaryotic diversity (such as Alastair Simpson or Sergei Karpov). The pair of papers is now amongst the most oft-quoted articles in the field, which means in practice that it is now a point of departure in any discussion of eukaryote classification.

In both papers the ranks are altogether dropped. Curiously, the taxa are presented in a semi-ordered hierarchical form where subordinate taxa are given more black dots. For instance, in the earlier paper (Adl, Simpson, Farmer et al., 2005) the group Opisthokonta is presented as follows (numerous taxa have been omitted):

OPISTHOKONTA

• Fungi

•• Basidiomycota

•• Urediniomycetes

•• Ustilaginomycetes

•• Ascomycota

••• *Neolecta*

••• Taphrinomycotina

. . .

• Mesomycetozoa

•• Aphelidea

•• *Corallochytrium*

•• *Capsaspora*

•• Ichthyosporea

•• *Ministeria*

•• Nucleariida

• Choanomonada

• Metazoa

•• Porifera

••• Silicispongia

•••• Hexactinellida

•••• Demospongiae (. . .)

Let's note a few properties of this classification method. First of all, the four main groups of Opisthokonta (the ones with a single dot, i.e. fungi, mesomycetozoans, choanomonads and animals) are, to the best of current phylogenetic knowledge, monophyletic. This makes them perfect for the role of taxa in an openly Linnaean system, that is at the same time in line with Hennig's rules of phylogenetic classification. The taxa with the same number of dots, however, represent radically different ranks in previously published classifications. The group Mesomycetozoa, as presented here, is composed of three genera (*Corallochytrium*, *Capsaspora* and *Ministeria*), two classes (Aphelidea and Ichthyosporea) and one order (Nucleariida). One might of course erect classes for all of them, which however, often leads to taxonomic inflation, as mentioned in the previous section.

Let's now specifically discuss the problem of kingdoms. If we hold on to the idea that equal "ranks" in the classification by Adl *et al.* should be given equal Linnaean ranks, we would be forced to create two additional kingdoms Choanomonada and Mesomycetozoa, that would be placed alongside Fungi and Metazoa/Animalia in the, most likely, superkingdom Opisthokonta (plus other kingdoms in other superkingdoms). That is, in fact, a fairly popular point of view, expressed for instance by Tedersoo (2017), who includes kingdoms Choanoflagellozoa (essentially synonymous with Adl *et al.*'s Choanomonada) and Ichthyosporia (Ichthyosporea is a synonym of Mesomycetozoa in many classification systems). It is worth noting that Tedersoo's system might be thought of as a good demonstration of what happens if one attempts to create a Linnaean system adhering to Hennigian constraints, based on modern understanding of eukaryotic phylogeny (the classification by Adl *et al.* is listed by Tedersoo as one of his main sources of information). The result? His system has 32 eukaryotic kingdoms. Let's cite his own opinion on that fact: "In the proposed classification, the erection of 32 eukaryote kingdoms certainly catches and, perhaps, scratches the eye. I found adoption of multiple kingdoms necessary to follow the monophyly principle [...]." (Tedersoo, 2017, p.8)

Note that Adl *et al.* themselves stand *against* such practices, calling such artificially created higher-level taxa "superfluous" (Adl, Simpson, Lane et al., 2012, p.430), therefore openly arguing for classification with missing taxa.

Solution II: classifications with paraphyletic taxa

The most vocal contemporary proponent of that solution is probably Thomas Cavalier-Smith, a controversial figure among microbiologists, who, however, is at the same time undoubtedly one of the most influential personas in the field of eukaryotic systematics. His contributions include the early recognition, and naming, of Excavata, Opisthokonta, Rhizaria and Chromista – all of them being now largely accepted, and the latter three: most likely monophyletic, mega-grouping of eukaryotes. In his proposed classification of life (with six kingdoms) (Cavalier-Smith, 1998) there is a long section on "philosophical preliminaries", where the necessity for admitting paraphyletic taxa is forcefully defended. His two main arguments against the Hennigian requirement to limit taxa to clades are as follows:

1) It leads to instability. Each new discovery in biology – be it a paleontological or molecular novelty, or simply the discovery of a new species or a reinterpretation of anatomical data – may lead to the reassessment of a monophyletic taxon as paraphyletic, which would force the biologists to update classification systems. In practice it would mean hundreds, if not thousands, of revisions every year.

2) It is not practical. Let's quote Cavalier-Smith himself: "Whether a taxon is paraphyletic or not is irrelevant to its validity as a taxon. It is also irrelevant to many of the uses to which classifications are put, such as arranging specimens in a museum, organising

the chapters in a biology textbook, or providing a convenient label, e.g. bacteria or fungi, for a group of similar organisms." (Cavalier-Smith, 1998, p.212)

As a result, Cavalier-Smith for about two decades has become an active opponent of Hennigian classification, especially in the field of microbiology. Every year or two he proposes a new system of classification, sometimes general, most often specific to a group of eukaryotes (Cavalier-Smith, 2002; Cavalier-Smith, 2013; 2016), usually being a carefully crafted compromise between contemporary phylogenetic knowledge and practicality. His 1998 system (Cavalier-Smith, 1998) has six kingdoms:

- empire/superkingdom Prokaryota*
    - kingdom Bacteria* [note: Archaebacteria are to be found here, as an infrakingdom]
- empire/superkingdom Eukaryota
    - kingdom Protozoa*
        - subkingdom Archezoa*
        - subkingdom Neozoa*
    - kingdom Animalia
    - kingdom Fungi
    - kingdom Plantae
    - kingdom Chromista

All openly paraphyletic ("almost certainly paraphyletic" in his own words) taxa are marked with an asterisk. It is interesting to note that, while Cavalier-Smith openly opposes Hennigian phylogenetic systematics, his "illegal" taxonomies are highly popular. A quick look into any contemporary paper on eukaryotic systematics will reveal a number of high-level taxa formally defined by Cavalier-Smith, many of them known or suspected to be paraphyletic. The reason is simple: his classifications are extremely convenient, because they are invariably composed in such a way as to include only a minimal number of taxa *and* ranks which are *usually* monophyletic, but sometimes are paraphyletic if that makes for a convenient system. The reader is referred to the above-cited paper (Cavalier-Smith, 1998) where his philosophy of biological classification is explained in detail.

On a side, and probably more personal, note: the zoologists reading this paper may find it interesting to go through his proposed classification of the animal kingdom (Cavalier-Smith, 1998, pp.235–237) which offers, in the opinion of the author of this paper, a refreshing look at the list of animal phyla. As currently recognized, there is somewhere around 30-35 phyla – consult any modern textbook on zoology – that only recently began to be grouped into a few large monophyletic groups, such as Spiralia, Ecdysozoa or Deuterostomia. Other than that, there is a confusing diversity of tiny phyla, most of them unknown to the general public: how many non-zoologists know of Kinorhyncha, Loricifera, Gnathostomulida (jaw worms) or Acanthocephala (spiny-headed worms)? Cavalier-Smith groups all animals into 22 phyla – still a long list, but, especially with the aid of his clear, succinct diagnoses, is much more manageable. The classification includes some, but rather few, paraphyletic taxa.

Solution III: abandon classification systems

The third solution would be to run away from the problem, so to speak, and refrain from explicitly writing down classifications, and discuss biological diversity through phylogenetic trees only. This has been proposed from time to time by certain scientists and philosophers (the proposal has been reviewed and critiqued by Michael Benton (2000)). While reading biological literature, one finds this sentiment expressed from time to time, especially by specialists working with rapidly changing classification systems. There is a fascinating, heated dialogue that ensued during the 1970 First International Conference on Ephemeroptera, recorded in this conference's proceedings (Peters and Peters, 1973, pp.151–154), where a group of entomologists become increasingly frustrated by their inability to create a classification system based on the otherwise clear phylogenetic evidence presented by one of the participants (Edmunds Jr, 1973). In fact, all the problems discussed in this paper with regard to higher ranks are mentioned during that discussion, which is about families, subfamilies and genera of Ephemeroptera, which makes for a great read.

There are at least two large groups in biology that have abandoned updating the classification of the organisms they are working with: botanists working with flowering plants and malacologists.

In the first case, one would be hard-pressed to find a recent authoritative classification of flowering plants, because the focus of the communal effort has long been the creation of better and better phylogenies, not taxonomies. The Angiosperm Phylogeny Group regularly publishes the new view on plant phylogeny, employing formal taxa only to the level of order higher-rank taxonomies are officially accepted by APG.

The exactly same process had happened in the field of malacology, where for years there were no formal definitions of gastropod taxa above the level of superfamily (see Bouchet and Rocroi, 2005). Interestingly, the situation visibly upset some of the workers in the field who started spontaneously grouping the newly defined families and superfamilies into orders, those into superorders, subclasses etc. Last year in a revision of the 2005 classification (Bouchet, Rocroi et al., 2017), the authors surrendered and included higher-ranked taxa, although the system is now very "messy": it includes numerous intermediate-level ranks that correspond to the unranked clades in the previous classification – there are classes, subclasses, infraclasses, cohorts, subcohorts, superorders, orders, suborders and infraorders in the system, plus a handful of openly paraphyletic "grades". The struggle of malacologists to bring back Linnaean classification into the world of Hennigian unranked lists à la Adl *et al.* leads to exactly the same problems that were discussed in the previous sections.

The case of gastropod classification illustrates, however, that even specialists working in very narrow fields need balanced ranked classification systems. It is not only for the purposes of educating the young, writing books or organizing museum expositions that we need neat, logical classifications with no missing ranks and a small number of distinctive, easy to remember taxa. The specialists need them too. The option to abandon classification systems seems to be not viable, especially that it is both trivial and tempting to create a list of clades from a phylogenetic tree, adding ranks to some or all taxa, which would be a *de facto* classification, just like Benton did in his *Vertebrate Palaeontology*.

4. Summary

Our increasing knowledge of biodiversity, especially in the case of microbiology, both prokaryotic and eukaryotic, will inevitably lead to the escalation of the problems presented in this paper. Because it doesn't seem plausible that classification systems will be altogether abandoned (which would leave us only with phylogenetic trees, sometimes presented as unranked lists), it seems that we must somehow solve the problem of creating classification systems in the times of abundant phylogenetic data.

Broadly speaking, there seem to be two directions that one might take: 1) to follow phylogenetic data to the letter; 2) to follow intuition and convenience. Simply put, the option 1) would mean having only proper monophyletic taxa, but a highly impractical system; and the option 2) would mean having also paraphyletic taxa *and* a system that is practical.

In the special case of eukaryotic kingdoms, the first route would lead to a revolution in biological classification of life and numerous kingdoms currently unknown to the general public would be introduced (Tedersoo, 2017), such as Oxymonada, Breviatea or Filasteriae, that would now be listed alongside plants, animals and fungi as "major types of life". Alternatively, one might drop the traditional kingdoms altogether and define the currently recognized eukaryotic "supergroups" (e.g. Keeling et al., 2005) as kingdoms, and what we know recognize as kingdoms would have to be downranked into subkingdoms or "microkingdoms" (Pawlowski, 2013). This would be the resulting classification:

- kingdom Excavata
- kingdom Amoebozoa
- kingdom Opisthokonta (incl. fungi and animals)
- kingdom Archaeplastida (incl. plants)
- kingdom Rhizaria
- kingdom Alveolata
- kingdom Heterokonta

Obviously, this would *not* solve the problem, if one would stubbornly keep on sticking to Hennigian rules. First of all, Excavata may be paraphyletic (He et al., 2014), in which case we would have to split it into monophyletic groups, resulting in a classification system alike to this:

- kingdom Euglenozoa (former Excavata)
- kingdom Heterolobosea (former Excavata)
- kingdom Jakobea (former Excavata)
- kingdom Metamonada (former Excavata)
- kingdom Amoebozoa
- kingdom Opisthokonta (incl. fungi and animals)
- kingdom Archaeplastida (incl. plants)
- kingdom Rhizaria
- kingdom Alveolata
- kingdom Heterokonta

Secondly, there is at least a dozen groups of eukaryotes that don't fit neatly into any of the supergroups, including *Tsukubamonas* and *Malawimonas*, Cavalier-Smith's Varisulca, Apusozoa, but also much better-known groups such as Cryptophyta or Haptophyta. Consequently, in Tedersoo's system, there *are* kingdoms Malawimonada, Tsukubamonada, Apusozoa, Cryptista and Haptista which brings us to square one. Obviously, replacing traditional kingdoms with eukaryotic supergroups is *not* a solution.

The second option – to retain the general structure of the present classification of life into kingdoms – is not fully satisfactory, either, because the old kingdom Protozoa is now known to be a large, highly structured group that deserves proper recognition and can't be rightfully treated as an unstructured bunch of "amoeba and such" (see Patterson, 1999). Its representatives have very little in common with each other and include multicellular forms similar to fungi (mycetozoan slime molds, acrasids) and plants (brown algae), single-celled large predatory heterotrophs (ciliates), intracellular parasites (kinetoplastids) and endosymbionts (syndineas); colonial filtrators (choanoflagellates), multinucleate "superamoebae" (labyrinthulomycetes) and tens of other forms. Small steps, such as Cavalier-Smith's proposal for the kingdom of Chromista, might be seen a sign of a more conservative process of a slow, incremental change, not dictated by blind adherence to formalized ideals, but rather by educational values.

At the moment it is uncertain which approach will dominate, but it's clear that creating a top-level classification of life congruent with our contemporary knowledge of eukaryote phylogeny will require us to resign from at least some philosophical principles of biological systematics.

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Klasycy: teksty-komentarze

Classics: texts and commentaries

How is *philosophy in science* possible?

Michael Heller
Translated by Bartosz Brożek and Aeddan Shaw*

Abstract

The Michael Heller's article entitled "How is *philosophy in science* possible?" was originally published in Polish in 1986 (see Heller, 1986) and then translated into English by Bartosz Brożek and Aeddan Shaw and published in 2011 in the collection of essays entitled *Philosophy in Science. Methods and Applications* (Heller, 2011). This seminal paper has founded further growth of the 'philosophy in science' and become the reference point in the methodological discussions, especially in Poland. On the 40th anniversary of *Philosophical Problems in Science* we wanted to make this paper freely available to the international public by reprinting its English version. In this issue it is followed by two additional articles-commentaries (by Paweł Polak and Kamil Trombik).

Keywords

philosophy in science, philosophy of science, metaphilosophy, interdisciplinary research, science and religion, analytic philosophy.

* In this edition some quotations have been replaced by the translations of their original sources (if available). The references have been adjusted to the standards of the journal.

Zagadnienia Filozoficzne w Nauce, No 66 (2019), pp. 207–225

1. Introduction

'Philosophy in science' grew out of practice. Its most significant example is the phenomenon of the 'philosophizing physicists'. And even though the philosophical reflection of the representatives of the empirical sciences often falls short of the professional philosophical standards, it does not change the fact that the sciences are filled with philosophical contents.

In the recent years in the Polish philosophical literature such terms as 'philosophical issues in science' have appeared on the covers of several publications.[1] The English 'philosophy in science', through its contrast with, and similarity to 'philosophy of science', has been 'sanctioned' in the title of a new periodical.[2] The paper by W.H. Stoeger, published in the first volume of *Philosophy in Science*, may be considered a manifesto of the editorial board, as well as an attempt to provide a theory of 'philosophy *in* science'.

I am against any planning what kind of philosophy should be practised, i.e. determining *a priori* the method of analysis and its consequent application. It is more natural when the methodological reflection follows the period of abundant, sometimes instinctive or even chaotic research in a new discipline. I believe, however, that the time has come for an attempt to systematize what *de facto* is 'philosophy in science'.

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[1] Cf. *Zagadnienia Filozoficzne w Nauce*, a periodical published in Kraków since 1978; see also (Heller, Lubański and Ślaga, 1980).

[2] *Philosophy in Science* is published by Pachart Publishing House, Tuscon. The first volume appeared in 1983.

2. Philosophy in science and philosophy of science

Among the philosophers of nature (in particular those belonging to the neo-thomistic school) there is a commonly accepted doctrine of the non-intersecting planes. Generally speaking, it says that philosophical cognition lies at a totally different epistemological plane than the empirical sciences; they use different methods and operate with mutually untranslatable languages.[3] In order to justify this view the theories developed within the contemporary methodology are cited. It is sometimes tempting to say that the major motive behind such stances is to safeguard one's philosophy against any conflict with the sciences, as well as the theoretical justification of one's incompetence in the sciences.

The proponents of the two planes doctrine may protest against the 'philosophy in science' project as methodologically flawed and epistemological nonsense, an attempt at a comparison of the incomparable. I recall those objections not in order to dismiss them (the best way to reply to them is through the results already obtained in the 'philosophy in science' field), but to underline the relationship between 'philosophy in science' and philosophy of science. It is obvious that any philosophizing which is open for the dialogue with the empirical sciences must take into account their achievements. Otherwise it would be subject to the objection of anachronism. It is equally difficult to reject the claim that there exist serious differences between the 'cognitive plane' of the empirical sciences and some philosophical currents. I do not believe, however, in any strict isolation-

[3] This is a kind of philosophy advanced in two books: (Mazierski, 1969; Kłósak, 1980). Both these authors seem to see the need for the mutual influences of philosophy and the sciences and develop subtle distinctions in order to open the way for such influences despite the non-intersecting planes.

ism: of the philosophy in relation to the sciences, or *vice versa*. The methodological bans will be breached anyway, and it is often through the violation of the received canons that new paradigms emerge, i.e. some progress is made in our attempts to understand the world: the two non-intersecting planes may turn out to be elements of the same stratification of a more-dimensional space.

'Philosophy in science' has *de facto* been practised from the beginnings of the empirical sciences. For example, looking at the Newton's oeuvre, it is difficult to determine whether it is a case of science in philosophy, or already of philosophy in science. Thus, an attempt to categorize *ex post* the problems of 'philosophy in science' is possibly realizable; however, in face of the richness of this problematic, I shall concentrate on a succinct analysis of three exemplary issues. Although they do not exhaust the content of 'philosophy in science', they remain typical examples so that they enable to reconstruct its nature and methods. In what follows I shall present (A) the influence of the philosophical ideas on the development and evolution of scientific theories; (B) the traditional philosophical problems intertwined with empirical theories; (C) philosophical reflection over some assumptions of the empirical sciences.

3. The influence of philosophical ideas on the development and evolution of scientific theories

Empirical science originated through the separation from the old, all-embracing philosophy and still bear the imprint of this origin. Contemporaneously, various philosophical ideas often serve as an inspiration for developing new conceptions in the empirical sciences. However, many methodologists defend the 'purity' of science by introducing the well-known distinction: indeed, in the context of discovery philosophical ideas often influence the development of science, however it is not their role only—other factors, even irrational ones, may be influential in the process of arriving at new discoveries; on the other hand, in the context of justification, i.e. the sphere of the proper science-creating activities, philosophy has no bearing—it is an 'alien body', effectively eliminated by the built-in mechanisms of science. It is the disregard for this distinction that led to the phenomenon of the 'philosophizing physicists'—the representatives of the empirical sciences who, wrongly taking the context of discovery for the discovery itself, believe to have something philosophically interesting to say, while in fact they reveal only their psychological associations.

In the recent years, the distinction between the two contexts has been severely criticized. A case in point is the following passage from Stefan Amsterdamski's study:

> Metaphysics, myths or superstitions are in some manner as immanently a part of science as the facts which we attempt to include into the rational reconstruction. The neoplatonic metaphysic of Kepler and Copernicus were as much an element of the rational organization of the universe which they attempted to reconstruct as the strictly empirical statements of their astronomic systems. (Amsterdamski, 1973, p.99; 1975, pp.65–66)

To put it more succinctly:

> Therefore, science consist not only of statements about the universe under study, but also of assumptions about the knowing subject. (Amsterdamski, 1973, p.100; 1975, p.66)

If this line of argument is sound, 'philosophy of science' is simply a part of the science itself.

It is worth underlining, that the psychological or sociological accounts of the philosophy of science—which have recently gained in strength and prestige—almost completely dispense with the distinction between the 'logic of science' and the 'external circumstances' of that logic (Amsterdamski, 1983; 1992; cf. Życiński, 1983). It is not my goal to engage in a philosophical discussion. However, I personally consider the distinction between the context of discovery and the context of justification useful under the condition that it is understood in a flexible way, which paves the way to a gradual passage from one context to the other. All in all, the impossibility of drawing a sharp demarcation line between 'inspirations' and 'justification' is a sufficiently strong argument in favour of the 'philosophy in science'.

Another conception of the contemporary methodology which clearly points towards some philosophical elements in science is the so-called thematic analysis, proposed by Gerald Holton (cf. Holton, 1998). He believes that in many concepts, methods, claims and hypothesis of science there are certain elements he calls *themata*, which as if from hiding influence or even determine the development of new scientific ideas. *Themata* often come in pairs (of opposites), sometimes in triplets, and have surprising durability over the centuries—they are capable of surviving many scientific revolutions. Here are some examples of *themata*: unity—multiplicity; determinism—indeterminism; continuity—discontinuity; symmetry; invariance, complementarity, etc. Holton is surprised with the relatively small number of *themata*—in physics he identified some 100 thereof—and underlines their interdisciplinary and philosophical character. *Themata* may constitute the pivotal ideas for the studies in the history of science, but considered from the perspective of their philosophical load they are nothing else but 'philosophy in science'.

4. Traditional empirical problems intertwined with empirical theories

One can enumerate a number of such problems or rather clusters of problems. Here, I shall limit myself to examples pertaining to time and space. It would be difficult to find a philosophical system that has nothing to say about time and space; and it would be difficult to identify a relatively comprehensive contemporary physical theory that would assume no theses pertaining to time and space. A classical objection against such bonding of philosophy with empirical theories consists in stressing the fact that any doctrine which 'migrates' from philosophy to the 'specialized' disciplines loses irrevocably its philosophical character, and the only thing that speaks to its philosophical origins are words, which—even though they sound the same—have completely changed their old meanings. As elsewhere, the doctrine of planes guards here the purity of philosophy. As I remarked earlier, it is not my goal to fight this doctrine; I would like to show, however, that philosophy exercises much more direct influence over the development of empirical theories than granted by the traditional wisdom.

Sometimes, in philosophy a view or a complex set of ideas—we shall say: a doctrine—is established which becomes a kind of paradigm or a research programme for one or more empirical theories. It so happens that philosophical paradigms are incorporated into some empirical theories (possibly in violation of the rule that forbids trespassing from one 'plane' to the other, while changing its 'meaning content'); but it happens also that a paradigm resists all such attempts, which leads to partial effects or side-effects only. When an empirical theory succeeds in realizing such a philosophical programme, one may say that the given empirical theory is a model of the given philosophical doctrine. The conception of empirical models of philosophical doctrines is still awaiting a more thorough analysis. Below, I confine myself to examples pertaining to the philosophy of space and time.

In the famous *Scholium* at the beginning of his *Philosophiae Naturalis Principia Mathematica*, Newton formulated a philosophical doctrine of the absoluteness of time and space:

> Absolute, true, and mathematical time, of itself, and from its own nature flows equably without regard to anything external, and by another name is called duration.—Absolute space, in its own nature, without regard to anything external, remains always similar and immovable. (Newton, 1687, Scholium B)

Today one would say that these definitions functioned within the context of discovery of the classical mechanics. It is certainly true, but this was not their only role. It was Newton's intent to incorporate the doctrine of the absolute time and space into the new mechanics. Newton himself, as well as generations of physicists that followed him, believed that he had succeeded in doing so. However, a careful analysis, with the use of the contemporary mathematical tools, reveals that—indeed—the absolute time plays an important role in the structure of the classical mechanics, but the structure does not include an element that would correspond to the philosophical intuitions pertaining to the absolute space (Raine and Heller, 1981, pp.57–81). Thus, one must carefully distinguish between Newton's own views concerning space and time and the structure of space and time presupposed by the Newtonian mechanics. The fact that Newton's views are incompatible with the 'views' of his mechanics is clear evidence that philosophical ideas are active not only in the contexts of discoveries, but are also intimately linked to the history of justifications of scientific theories.

To sum up this stage of our reflection, one may succinctly say: the classical mechanics is a physical model of the philosophical doctrine of the absolute time; however, it is not a physical model of the doctrine of the absolute space.[4]

The 'other side' of this story is equally instructive. Long before Newton there was known a philosophical doctrine rival to the conception of the absolute time and space. Its most famous incarnation was formulated by Leibniz:

> As for my own opinion, I have said more than once, that I hold *space* to be something *merely relative*, as *time* is; that I hold it to be an *order of coexistences*, as time is an *order of successions*. (Leibniz, 1717, p.57)

Despite the clear attractiveness of the Leibnizian philosophy of time and space, it belonged the philosophy textbooks only till the development of the theory of relativity (cf. Heller and Staruszkiewicz, 1975). The obvious reason for this was that neither Leibniz nor any of his followers managed to create a physical model of the philosophical doctrine of the relative character of space and time (cf. Raine and Heller, 1981). There is a deeply rooted conviction that such a model was provided by the general relativity theory. This conviction proved essentially wrong,[5] but the analysis led to a new, interesting observation. In the past, the doctrines of the absoluteness and relativeness of time and space were treated as mutually exclusive; only

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[4] In connection to the problem of the logical structure of the classical mechanics analysed with the use of the contemporary mathematical techniques, it is also worth mentioning two studies (Friedman, 1983; Torretti, 1983).

[5] The problem is more subtle than the above considerations suggest. One would need to identify at least several senses of 'relational' and 'absolute'. There is no place in this essay to go into the details, thus I recommend the cited works (Raine and Heller, 1981), as well as (Friedman, 1983).

one of them would turn out true, *tertium non datur.* The general relativity theory falsified this view: it is a model of a partially relational (as it depends on the bodies that populate it), and a partially absolute (in the Newtonian sense) space-time (cf. Raine and Heller, 1981, chap.13).

This example illustrates again in which way a philosophical doctrine reveals its presence (or absence) in empirical theories; it is completely independent of the beliefs of the authors of these theories (i.e., the problem lies beyond the context of discovery), and often in violation of such explicit beliefs. An empirical theory may be—or not—a physical model of some philosophical doctrine: it is its fully objective feature, which may be analysed with the contemporary formal means.

The elements of the conception of absolute time and space stubbornly remain *inside* the theories of the contemporary physics, despite many attempts at their removal and creating a physical model of a doctrine of fully relative space and time. One may even say that the drive towards such a model is one of the determinants of the tendencies in the contemporary theoretical physics. It is in this sense also that philosophical doctrines are present in the evolution of science.

5. Philosophical reflection over some assumptions of the empirical sciences

This type of analysis has long been applied in the contemporary philosophy. For example, it is the general framework of the important part of Husserlian phenomenology. Here however, a different aspect of this problematic is interesting. Again, it is suitable to use examples.

I shall sketch the problems surrounding the following assumptions of the empirical sciences: (a) the assumption of the mathematicity; and (b) of the idealizability of nature, as well as (c) the assumption of the elementary character and (d) the unity of nature. These assumption may in a natural way be joined in pairs (a-b and c-d), which should be analysed together. A number of remarks and short commentaries concerning these assumption has already been formulated; however, they still await a more thorough, monographic study that would provide a precise formulation of the fundamental questions to which the assumptions inevitably lead.

(a) **The assumption of the mathematicity of nature.** From the most general point of view, the mathematicity of nature boils down to the fact that nature can be described mathematically. It may be considered a fact since it is 'empirically' confirmed by the development of the sciences from the times of Galileo and Newton. Moreover, this development is extremely efficient, documented with a sequence of successes, both theoretical and pertaining to the 'technical' conquest of nature.

The mathematicity of nature may be considered a counterpart of the medieval *intelligibilitas entis*—the comprehensiveness of being. In this context, Wigner discusses "incomprehensible comprehensibility of the universe", and Einstein remarks that "the most incomprehensible thing about our universe is that it is comprehensible." In order to better grasp this problem one should distinguish between at least three senses in which nature could have been non-mathematical:

1. Nature could have been amathematical, i.e. non-describable with the use of any mathematics. This would mean that nature is irrational and would probably exclude it from existence.[6]
2. Nature could have been mathematically transcendent in relation to our cognitive capacities, i.e. mathematics needed to adequately describe nature would require such formal means that are in principle inaccessible to our cognition. Simple models of universes that are non-mathematical in this sense were constructed by Kemeny (1959; see also my study Heller, 1974, pp.112–119) and Staruszkiewicz (1980).
3. Nature could have been mathematically too complicated in relation to our capacities, but not in principle—only regarding the level of difficulty. Some level of difficulty would make impossible or very unlikely the rise and development of the empirical sciences. For example, the fact that the Newtonian equation
$$F = G\frac{m_1\, m_2}{r^2}$$
approximates well the gravitational force between two point masses, facilitated or even enabled the development of the theory of universal gravitation at the end of the 16th Century. If the exponent in the denominator did not equal 2, but, say, 2.009, the orbits of planets would be so complicated that Kepler would most probably fail to discover any significant regularities.

[6] It must be stressed that I am speaking of the mathematicity of nature only. The complicated problem of the relationship between 'mathematicity' and mental phenomena cannot be addressed in this essay.

This final understanding of mathematicity of nature is strictly connected to the next tacit assumption of the contemporary empirical method, i.e.:

(b) **the assumption of the idealizability of nature.** It is worth noticing that the modern empirical method proved successful not when it began its experimental game with nature, but when people learnt to ignore a number of 'inessential' factors of that game. The failure of the Aristotelian physics as an empirical science was connected to its insistence on accounting for the entire complexity of nature (friction or drag were not ignored). One may even say that the 'creation' of 'non-existent', but mathematically simple 'entities' was a prerequisite of the success of the empirical method, to mention but the class of inertial coordinate systems, energetically isolated systems, etc. The possibility of approximating nature with sufficiently simple mathematical models is the mathematical manifestation of the idealizability of nature.[7]

The assumption of the idealizability of nature accommodates also the assumption of its stability of a certain kind. For example: if small perturbations of an observable measurement led to significantly different (non-equivalent in certain respect) mathematical models of the studied domain, then—given the fact that observable parameters are always measurable with some perturbations (measurement error)—the study of nature would be impossible. By excluding such situations, one assumes the observational stability of nature. The observational stability of nature is a special case of a more general concept, that of the structural stability of nature. By postulating such a kind of stability, one needs to determine an equivalence class of structures, kinds and magnitude of their perturbations and assume that a small perturbation does not exclude the given structure from

[7] Some aspects of this problem are discussed in my paper (Heller, 1983).

the equivalence class.[8] The role of structural stability was stressed by René Thom (1977), but a systematic discussion of this problem in relation to the philosophy of science is still missing.

In the contemporary empirical sciences a significant role is reserved for probabilistic models. When operating with them, one needs to assume a special kind of stability, known as frequency stability. In the standard probability calculus, the probability measure of the elementary events is taken to be represented by the numbers close to their observed frequency. Such a definition of probability assumes that the future series of similar experiments shall, in the long run, give relative frequencies substantially similar to the relative frequencies observed currently. This assumption—which is verified both in our ordinary experience and in the scientific practice—is called the assumption of frequency stability. It attributes to the world a certain feature, thanks to which it can be studied probabilistically (cf. Heller, 1985).

The problems of the mathematicity and idealizability of the universe are connected to one additional issue. Both these assumptions attribute to nature a feature, which is responsible for the nature's mathematicity and idealizability, but they also say something about the human mind, which is capable of accounting for nature as mathematical or idealizable. Thus, the assumptions in question may be considered both from the ontological and the epistemological perspectives. It is also possible that one cannot take one of the perspectives, while excluding the other. This problem must also be scrutinized.

The assumptions of the mathematicity and idealizability of nature are strictly connected to:

[8] On the subject of the concept of structural stability and its applications in the methodology of the sciences see (Szydłowski, 1983).

(c) and (d) **the assumptions of the elementary character** and **unity of nature.** These assumptions are counterparts of two essential features of the mathematical method. Understanding in mathematics may proceed either in the direction of analysis (towards axioms and primitive concepts of the given mathematical theory) or in the direction of synthesis (i.e., towards 'embedding' the given mathematical 'entity' within some global structure, from which it can be—artificially?—extracted). The reductionist and holistic explanations outside of mathematics have their sources in the same two opposite tendencies of the human mind.

The assumption of the elementary character of nature urges us to uncover the 'elementary level' in reality. At the first sight, it seems that the process of descending towards more elementary levels never ends (as the drive 'to understand' requires to reduce any 'data' to something more elementary) or must be 'artificially' terminated by a conventional acceptance of some 'rudimentary' level. In the contemporary theoretical physics there is a strong tendency to reduce physics to pure mathematical structures. In this sense, the 'mathematical material' becomes elementary for physics.[9]

The problem of the unity of nature has been analysed in detail (cf. Weizsäcker, 1980). Doubtless, it has many dimensions. One of them is the clearly visible tendency of the contemporary physics to develop unification theories. However, from the philosophical point of view a deeper dimension of the problem is constituted by the unity postulated by the very mathematical-empirical method of studying the universe.

[9] This is illustrated by the example of the concept of matter, which—during the evolution of physics—was replaced by purely formal structures; cf. my paper (Heller, 1982).

In this context a question arises: may totality (i.e., unity in one of its meanings) turn out to be an elementary category? Even if it is not the case, I believe that the assumptions of unity and the elementary character of nature must be analysed together. Possibly, one has no definite sense without the other.

6. A Proviso and an appeal

It goes without saying that the above mentioned problems are only a preliminary catalogue of questions delineating 'philosophy in science'. Under no condition the above considerations should be considered an attempt to provide event partial answers.

It was also not my intent to provide a theory of 'philosophy in science', although I am not against such undertakings. I would only protest against calling 'philosophy in science' some meta-considerations which are not rooted in the scientific practice. However, this proviso is barren: philosophical issues in science are so interesting that they will be contemplated irrespective of any appeals or restrictions. They require interdisciplinary research and thus only one appeal is in place—an appeal for a responsible cooperation between philosophers-methodologists and the representatives of the empirical sciences. Only through expertise in both disciplines it may be guaranteed that 'philosophy in science' will not transform into commonsensical (and hence: naïve) considerations, but will become a truly creative domain of knowledge, one indispensable in the contemporary intellectual ambience.

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Philosophy in science: A name with a long intellectual tradition

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Abstract

This paper presents Michael Heller's notion of "philosophy in science" and re-introduces Michael Heller's classical text that first presented this concept of philosophy entitled *How is Philosophy in Science Possible?*. The paper discusses the historical context of Heller's idea as it emerged from the discussions and works of the Krakow philosophical scene and discusses the basic tenants of this philosophy, its analytic character, the role of intellectual tradition in the development of this philosophy, and the critical role played by an interdisciplinary dialogue between philosophy, science, and theology. Despite the idea of philosophy in science having emerged about 40 years ago, this concept still inspires and fuels innovative research. The notion of "philosophy in science" lies at the foundations of the philosophy published in two journals: *Philosophical Problems in Science* (*Zagadnienia Filozoficzne w Nauce*) and *Philosophy in Science*.

Keywords

Michael Heller, philosophy in science, metaphilosophy, analytic philosophy, Lvov-Warsaw School, non-fundational philosophy, interdisciplinary research, science and religion.

The term "philosophy in science" has been in use for at least 40 years. It was first proposed in the late 1970s during the seminars held in Kraków by the scientists and philosophers working

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Figure 9: Frontmatter of *Philosophical Problems in Science* (*Zagadnienia Filozoficzne w Nauce*), no. 10.

with Michael Heller and Józef Życiński. Over the years, these seminars evolved into the Center for Interdisciplinary Studies (Trombik, 2019), which had its own journal, namely *Zagadnienia Filozoficzne w Nauce*. The term "philosophy in science" was used to denote the distinctive character of the philosophical topics discussed in his journal. Since the first issue, in addition to its Polish title *Zagadnienia Filozoficzne w Nauce* (1978/1979), also featured on its cover page the English equivalent "Philosophy in Science" (see fig 9).[1]

One article that defined the concept of "philosophy in science" also became a reference for methodological and metaphilosophical discussions about the roles of philosophy in science and of science in philosophy, specifically in Poland. This was Michael Heller's paper titled *Jak możliwa jest filozofia w nauce?* (*How is philosophy in science possible*?) (Heller, 1986).[2] The impact of this article, despite its historic significance, has been limited because the text has only

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[1] The same concept of philosophy was also applied in a second journal edited by Heller and Życiński (also co-edited by W.R. Stoeger) entitled *Philosophy in Science*. This journal was also published by the Center for Interdisciplinary Studies (Vatican Observatory and Pontifical Academy of Theology in Kraków) by the Pachart Publishing House (during 1983–2003). The periodical *Zagadnienia Filozoficzne w Nauce* was initially published in Polish, while *Philosophy in Science* was published in English. The current editions of the former periodical are now bi-lingual and cover both the English and Polish versions. The English title, *Philosophical Problems in Science,* is a direct translation of the original Polish title *Zagadnienia Filozoficzne w Nauce.* We keep this name because it reveals an important aspect of this approach, namely a focus on philosophical problems relating to science. The significance of this difference will become clear after reading this paper.

[2] Józef Życiński shared Michael Heller's concept of philosophy in science, but he focused on different aspects. A good example of this distinction is the co-authored article in which Życiński takes many parts of Heller's text verbatim and exposes in them new epistemological aspects of science and philosophy relationships that are not obvious in Heller's original text (Heller and Życiński, 1987).

been available in Polish. Hopefully this publication of an English translation of Heller's article, together with a commentary, will fill this gap.

1. The historical context of Heller's publication

Heller's paper needs to be viewed from the historical context. In sixties of the 20th century, Polish philosophy was entangled in a debate about the concept of the philosophy of science that was later described as counter-productive. The debate was provoked by Kazimierz Kłósak's papers about the traditional neo-scholastic philosophy of nature that were published around this time (Heller, 1995, p.150). Heller considered this debate misguided, however. In his view a new approach to the philosophy of science was needed that would differ from Kłósak's *a priori* method. The new approach also required a name that would differentiate it from older approaches.[3]

Together with Życiński, Heller aimed to create a philosophy grounded in science but harmonized with the Christian faith. This philosophy was supposed to compete with Kłósak's traditional philosophy of nature, and it was conceived as a modern, non-standard interpretation of the *ad mentem St. Thomae* metaphilosophical rule

[3] It is worthwhile noting that Michael Heller used the traditional name of the *philosophy of nature* (*filozofia przyrody*) in the sense of philosophy in science when it does not lead to misunderstandings, and he sometimes used this term in the broader sense of the "philosophical theory of nature." He formulated two necessary conditions that this "theory of nature" must satisfy: "(a) it cannot be a theory that ignores the natural sciences in whatever domain it studies, and (b) it cannot ignore at least the fundamental methodological rules elaborated by the contemporary philosophy of science." The meaning of these requirements is clarified by the following remark: "Violations of the first condition make the given philosophical conception an anachronism; neglect of the second condition threatens methodological anarchy" (Heller, 2011, p.15).

(Leo XIII, 1879). The new philosophy was intended to create a framework for science–religion studies that accounted for the most modern scientific knowledge.

In those years, Krakow, with its tradition of interdisciplinary debates, was a special place for engaging in the philosophy of science. At the center of these disputes was one Karol Wojtyła (Życiński, 1999, p.8nn), who later became Pope John Paul II . He organized and encouraged seminars and informal discussions among scientists and philosophers. Philosophical discussions that were initially rather informal continued with great success at more formal interdisciplinary meetings and seminars. The debates convinced Michael Heller that a new philosophy of science, which he denoted as "philosophy in science", was needed, one that should be founded on the assumption that philosophy must have an interdisciplinary character (Heller and Mączka, 2006, p.50), because it could not exist in isolation from other scientific disciplines. The second assumption, one inspired by positivist philosophy, was that modern science should serve as a tool for clarifying or solving classical philosophical problems (Heller and Mączka, 2006, p.50). However, contrary to positivist philosophy, which perceives classical philosophical problems (e.g., metaphysical issues) as fully reducible to science, the *philosophy in science* proposed by Heller saw these problems as having their own nature, one that transcended scientific methods.

2. Philosophy in science: A metaphilosophical concept

"'Philosophy in science' grew out of practice" (Heller, 2019, p.208): This sentence in the opening paragraph of Heller's paper reveals the source of this philosophy. The term "practice" may mean two things. It may denote philosophical reflection by scientists on their own research (e.g., philosophizing physicists), or it may refer to the concept of *philosophy in science*, which is a specific mode of philosophical reflection practiced by the philosophers and scientists in the Kraków milieu.

The former interpretation shows that the intuitions and the actual praxis both play equally fundamental roles in *philosophy in science*, and this philosophy should be undertaken with knowledge of hard facts and be enlightened by intuitions. Heller (2011, p.86) pointed out that neglecting scientific results and a lack of scientific intuition led philosophy astray. The former was the source of poverty in the German romantic philosophy of nature, while the latter led neo-scholastics to a false interpretation of the role of St. Thomas Aquinas's philosophy. In the "Introduction" to the *Philosophy in Science* journal, we find the following statement:

> One of the most dangerous movements of traditional philosophy has been its attempt to develop philosophical analyses independently of scientific results. And one of the most hopeless illusions of 19th century science was its desire to replace philosophy by science and to give scientific answers to questions posed by classical philosophy (Heller, Stoeger and Życiński, 1983, p.7). (Heller, Stoeger and Życiński, 1983, p.7).

Grounding *philosophy in science* in scientific praxis was behind Heller's aversion to the formalization of philosophy. Philosophy should take place spontaneously as part of scientific work. One may claim that *philosophy in science* has in fact been practiced from the very beginning of the history of empirical sciences. For example, looking at Newton's work, it is difficult to determine whether it is of a scientific or philosophical nature, or was it already a form of *philosophy in science* (Heller, 2019, p.210)?

For a long time, Heller avoided publishing any formal declarations or manifestos. He preferred to have tangible results that would speak for themselves (and his philosophy) rather than developing a complete theory. In his view, philosophy could generally be accurately characterized only *ex post,*[4] and any *a priori* claims about philosophy may be misleading and dangerous. He formulated only one necessary condition for practicing *philosophy in science*: "I would only protest against calling some meta-considerations that are not rooted in scientific practice 'philosophy in science'" (Heller, 2019, p.222). This statement could suggest an approval of a chaotic approach to philosophical work. However, a critical assessment of the traditional rigid methodology does not imply methodological anarchy or philosophical *laissez-faire*. Anticipating this interpretation, Heller clearly stated that "developing philosophy in science cannot generate an epistemological chaos" (Heller, Stoeger and Życiński, 1983, p.8). Thus, to correctly understand Heller's idea, we need to keep this declaration in mind.

The supremacy of practical science over purely intellectual pursuits as a source of philosophical insight reveals another facet of Heller's philosophy, namely its evolutionary character. Heller viewed philosophy as being engaged in an endless process of adjusting to science. Science is largely not static, so *philosophy in science* also should not be, because philosophy in science cannot remain blind to what occurs in the sciences. For Heller, the continuous development of philosophy is possible, because he conceived his philosophy

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[4] Michael Heller wrote: "I believe, however, that the time has come for an attempt to systematize what *de facto* is 'philosophy in science'" (Heller, 2019, p.208).

as non-foundational.[5] His philosophy is consequently minimalistic in the sense that it, as a rule, avoids the creation of a closed, static, defined, and rigid philosophical system (Heller, 2006b, p.34).

Michael Heller also stressed another metaphilosophical assumption behind *philosophy in science*, namely the possibility for a dialogue between philosophy and science. This assumption led him to reject the "doctrine of non-intersecting planes." The paradigmatic representation of this doctrine was a neo-scholastic philosophy of nature based on Jacques Maritain's concept of separation science from philosophy and theology. This principle, even if it seemed to be more logical or better than the non-separation methodologies, was unable to resolve the conflicts between science and philosophy precisely because of its *a priori* assumed separation. Thus, because of this *a priori* assumption, as well as its heavy historical baggage from its origins in scholastic philosophy, Jacques Maritain's approach to philosophy has been generally criticized and rejected by both philosophers of science and scientists. Scientists, in reaction to the poverty of classical philosophy, attempted to create their own form of philosophy, one inspired by their own scientific practices using their own scientific methods. Unfortunately, these philosophies made by scientists for scientists have been frequently judged by philosophers as being uncritical and sometimes naïve, at least from the philosophical point of view of course. Philosophy in science should be grounded in sci-

[5] In Heller's view, fundationalist philosophy is a philosophy that provides indubitable knowledge, and it is grounded in the incontestable fundamentals (Heller, 2006a). This kind of fundationalism is called *methodological fundationalism* by Heller, because it describes a philosophical method. The methodological fundationalism is opposed by psychological fundationalism, which requires indubitable grounds for knowledge. Anti-fundationalist philosophy rejects incontestable fundamentals and replaces them with hypothetical claims. This type of philosophy could never create a conceptually closed (complete) system. However, this philosophy improves philosophical methodology and clarifies conceptual resources.

entific practice, but at the same time it should be critical and firmly rooted in philosophical analysis. Of course, philosophy in science could not be reduced to just a few such rules, because its scope is largely determined by the ongoing dialogue between philosophy and science.

Heller's text from 2019 outlines three main assumptions of philosophy in science (Heller, 2019, p.210). These are:

(a) The development and evolution of scientific theories should be influenced and informed by philosophical ideas.
(b) The traditional philosophical problems are closely interwoven with modern empirical theories.
(c) Some assumptions in empirical sciences are open to philosophical reflection.

Philosophy in science has been also analyzed in a larger context, namely the epistemological (Heller and Życiński, 1987), methodological (Życiński, 1988), and even axiological (McMullin, 1982; see also Rodzeń, 1999).

While Michael Heller's text laid the foundations for philosophy in science, it left some of its aspects poorly defined. By mentioning Gerald Holton's concept of *themata*, Heller turns our attention to the role of the history of science in philosophical analysis. In Heller's view, the history of science is a rich repository of cases for analyses of the philosophy–science relationship. For Heller, the paradigmatic cases for the importance of history of science in philosophical analysis are Newton's and Leibniz's studies into the concepts of space and time. Taking a lesson from these examples, Michael Heller redefined the fundamental condition of practicing philosophy in science, namely a personal involvement in scientific praxis:

> There are two ways to clarify the intricacies of the empirio-mathematical scientific method: Practice the particular science by yourself or look at the history of science. The second method could be more effective, because it is not restricted to the perspective of a single person, and it enables someone to learn from the best scientists (Heller, 2005, p.156, all Polish quotations are translated by PP).

By drawing on the history of science, a philosopher or scientist can become involved in philosophy in science. Historical studies open up the possibility of dialogue between philosophers and scientists, thus creating an opportunity for historians of ideas, philosophers of science, and scientists to practice philosophy in science. With Heller's seminal works on the role of the history of science in philosophical analysis, detailed studies of the history of science have become the hallmark of philosophy in science at Kraków (Polak, 2018).

3. Is philosophy in science analytical?

The unique character of philosophy in science is revealed in its analytic nature. It is well known that the boundaries between the analytic and other types of philosophy are rather poorly defined, despite the fact that the concept of analytic philosophy is well entrenched in philosophy.[6] Describing analytic philosophy, historians of philosophy frequently use a strategy like the one presented by Aaron Preston (2019):

> Even in its earlier phases, analytic philosophy was difficult to define in terms of its intrinsic features or fundamental philosophical commitments. Consequently, it has always relied on contrasts with other approaches to philosophy. . .

For Preston, analytic philosophy is defined by its opposition to phenomenology, "continental," or "postmodern" philosophy. (It is also frequently separated from pragmatism.) One could also say the same about philosophy in science. The concept of analysis is fundamental for philosophy in science[7] but not at the exclusion of other methods. Heller suggests that while the precise concepts and language of science require the employment of an analytic method, the research methods should be much richer.[8]

The analytical character of philosophy in science could also be attributed to the role played by mathematics in scientific and philo-

[6] An interesting example is the description of analytic philosophy in Encyclopedia Britannica, which describes it as "a *loosely related set of approaches* to philosophical problems, dominant in Anglo-American philosophy from the early 20th century that emphasizes the study of language and the logical analysis of concepts. Although most work in analytic philosophy has been done in Great Britain" (Preston, 2019). The emphasized fragment of this text shows that analytic philosophy is not defined by their properties. For Keith S. Donnellan, it is rather "a *loosely related set of approaches*" distinguished on the basis of unclear and problematic criteria (mostly historical or intuitive).

[7] "Analytical approach of philosophy of science was conceived as a very important constituent—we would say a necessary precondition—of such research in a new style." (Heller, Stoeger and Życiński, 1983, p.8). (Heller, Stoeger and Życiński, 1983, p.8).

[8] In a later text, Heller stated his view on the role of definitions in this context: They play secondary roles and they could help with analysis, but they are less important than the analysis of mathematical structures. Clarifications could be made only through interpretation of the mathematical structures of the laws of nature.

sophical studies. It is true that philosophy in science employs mathematics in its analyses, but it focuses more on mathematical structures and their roles rather than on the mathematical language itself.[9] Heller explains the relationship between philosophy in science and analytic methods as follows:

> An empirical theory may be—or not—a physical model of some philosophical doctrine: it is its fully objective feature, which may be analysed with the contemporary formal means (Heller, 2019, p.216).

He further explains:

> Analytic philosophy (at least in the field of philosophy in science [filozofia przyrody]) is I consider consequently as a useful tool in philosophical work, but it is not an independent research method (Heller, 1995).

However, the analytic approach of philosophy in science differs from the methods of analytic philosophy proper, at least if analytic philosophy is assumed to exist. The analytic approach of philosophy in science focuses on factual philosophical problems within science rather than on the careful language analysis characteristics of classical analytic philosophy. Philosophy in science could not be developed without using an analytical approach, yet it is not reducible to analytic philosophy. One may therefore ask a rhetorical question: In future, will the analysis of mathematical structures be interpreted as the analysis of language, because mathematics plays the role of the language of science, and according to Heller, mathematics is the best language (or linguistic form) to describe reality?

[9] One of the latest examples is (Heller, 2016).

4. Discovering the role of tradition

The analytical character of philosophy in science is to a large extent rooted in the central European analytic tradition. Some aspects from the analytic tradition of the Lvov-Warsaw School were brought in by Zygmunt Zawirski, a member of this school (Jadacki, 2009). The Polish analytical school of philosophy and philosophy in science has drawn on the traditions of the 19th century Kraków school of philosophy (Polak, 2011) and the works of the Krakow circle for analytic philosophy (Wolak, 2005).

Historical studies have shown significant similarities between the methods employed in Heller's philosophy in science and the philosophy practiced in Kraków before the Second World War, which had its roots in the19th century. With the continuation of late-Enlightenment philosophy, the Kraków Scientific Society (Towarzystwo Naukowe Krakowskie), which later became the Polish Academy of Arts and Sciences (*Polska Akademia Umiejetności*), developed a specific methodology to compete in some aspects with the positivist philosophy that was prevalent at the time. The leading role in this school may be attributed to Józef Kremer, a former Hegelian, who was inspired by his scientist colleagues to create the concept of a non-foundational philosophy of science, traditionally called the "philosophy of nature (*filozofia natury*)" (Polak, 2019). This minimalistic, non-systematic approach focused on scientific problems and methods, having been developed at the onset of the Second World War in 1939.[10] Another center of analytic thinking in the first decades

[10] During Nazi Germany's occupation of Poland (1939–1945), science and philosophy could not develop officially and were banned as a part of Polish culture. Following the Second World War, roughly speaking, communism enforced the "official" Marxist philosophy, and the representatives of other philosophies, especial the analytical, were persecuted. The tradition of philosophy in Krakow could therefore not

of the 20th century was that of analytic philosophy in Lwów (Polak, 2016; Woleński, 2019). Of course not everyone in Krakow, or other centers of philosophical studies in Poland, was practicing this philosophy. For example, Franciszek Gabryl and Feliks Hortyński were still adhering to the neo-scholastic philosophy of science.

5. Philosophy in science: The role of an interdisciplinary approach

"Mutual interdisciplinary enrichment" (Heller, Stoeger and Życiński, 1983, p.7). was an important core methodological assumption of this philosophy. The short manifesto opening the first volume of Philosophy in Science declared:

> An interdisciplinary dialog among science, philosophy, and the philosophy of science seems to be the best way to avoid the traditional pseudo-solutions that are often created in the climate of epistemological isolation (Heller, Stoeger and Życiński, 1983).

In Heller's view, philosophy should be "critically sensitive and open to the resources available to it from the sciences and other dis-

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normally develop for some decades. In the long term, however, communism's efforts turned out to be counter-productive. Heller's text is one that appreciates an unofficial philosophy when it became possible. I t is worth noting that creating a new philosophy needs a critical assessment of the existing philosophy. This role was played by Józef Życiński's article that was published in the first volume of the *Philosophy in Science* periodical. This article did not formally fit with the other publications in this volume, and it can be understood only by looking at the local situation in Poland during the early 1980s. A remark from the "Introduction" confirms this interpretation: "Through such case histories, we may hopefully avoid simplistic, uniformed, but often commonly held assessments regarding the encounter between science and philosophy, as well as the pitfalls of the past" (Heller, Stoeger and Życiński, 1983, p.19).

ciplines" (Heller, Stoeger and Życiński, 1983, p.9). The interdisciplinary approach of philosophy in science was further analyzed by Stoeger (1983). He listed, among the other important features of this philosophy, the inclusion of metaphysical problems in the philosophical debate (i.e., problems that cannot be reduced to epistemology or meta-scientific analyses), its evolving character because this philosophy must be both critical and self-critical, and a strong reliance on interdisciplinary cooperation, for which this philosophy seems to be a natural platform. Heller generally agreed with Stoeger. He wrote that "[Stoeger's article] may be considered a manifesto of the editorial board, as well as an attempt to provide a theory for 'philosophy in science"' (Heller, 2019, p.208). However, Heller and Stoeger's visions differed in some small but important ways. For Stoeger and Heller, the goal of the new philosophy was to overcome the divide between philosophy and science, but Heller stressed the need for cooperation between disciplines, something that was not an important factor for Stoeger. He stated that:

> They require interdisciplinary research and thus only one appeal is in place—an appeal for a responsible cooperation between philosophers–methodologists and the representatives of the empirical sciences.

Heller also added:

> Only through expertise in both disciplines can it be guaranteed that "philosophy in science" will not transform into commonsensical (and hence naïve) considerations but instead become a truly creative domain of knowledge. . . " (Heller, 2019, p.222).

When formulating the conditions of this interdisciplinary cooperation, Heller drew on his own experience as a scientist and as the organizer of interdisciplinary seminars in Krakow since 1970s:

> The conditions of this interdisciplinary dialogue are: (1) not just intellectual openness but also a willingness to learn from co-debaters, (2) the aversion of any kind of indoctrination, (i.e., imposing one's own philosophical assumptions on co-debaters) (Heller and Mączka, 2006, p.50). (Heller and Mączka, 2006, p.50).

Confidence in the fundamental role of the interdisciplinary approach of philosophy in science was a heritage of a long tradition in Krakow's philosophy. Since the beginnings of the Krakow Scientific Society (*Towarzystwo Naukowe Krakowskie*) in 1815, philosophical problems were always discussed in cross-disciplinary seminars (Polak, 2019). The interdisciplinary studies continued later in the Polish Academy of Arts and Sciences, as well as in the Polish Copernicus Society of Nature Studies (later the Philosophical Society in Krakow). Michael Heller summarized over 30 years of experience of interdisciplinary dialogue in this sentence:

> This dialogue brought, I dare to say, results better than expected, mainly because it was founded (although initially we were not aware of it), on a rich [intellectual] tradition in Krakow, that goes back at least to the turn of the $19^{th}$and 20^{th} centuries (Heller and Mączka, 2006, p.50).

6. Concluding remarks

The notion of philosophy in science is needed to understand the specific nature of the philosophy published in the *Philosophical Problems in Science* (*Zagadnienia Filozoficzne w Nauce*) journal. This philosophy was developed in an interdisciplinary dialogue between philosophy and science. The concept of "philosophy in science" had its origins in Heller's broad understanding of the philosophy of nature. Michael Heller described it as a new philosophy for the philosophical interpretation of science. The interdisciplinary nature of this philosophy was also at the foundations of research into the relationships between science and religion. The essence of this dialogue lies in the non-isolationist epistemological perspective that philosophy in science takes toward theology (Polak, 2015).

The metaphilosophical foundations of philosophy in science are grounded in the long philosophical tradition of Krakow's philosophy and seem to be very stable. Heller's text could even be interpreted as a renewal of this tradition. After over 40 years, philosophy in science still inspires new research at the junction of science, philosophy, theology, ethics, metaphysics, and even the philosophy of computing and information. Its unique interdisciplinary character, its methodology, and its openness to new avenues of research have proved quite successful in the past and will certainly inspire new studies for years to come.

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The origin and development of the Center for Interdisciplinary Studies. A historical outline by 1993

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Abstract

The paper concerns the origin and early stage of development of the Center for Interdisciplinary Studies at the Pontifical Academy of Theology in Kraków. Center for Interdisciplinary Studies was founded by Michał Heller and Józef Życiński in the late 1970s. It was an informal institution which focused on conducting scientific activity in the area of philosophy of nature, relationship between mathematical & natural sciences and philosophy, history of science, as well as relationships between science and religion. In this paper I would like to present how this institution developed, I will discuss various forms of its activity and present—very generally—what kind of philosophy was promoted by M. Heller, J. Życiński, as well as their pupils and close associates. An important element of the paper will also be presenting the Center for Interdisciplinary Studies as a unique institution, which developed—in difficult historical period in Poland—philosophical research in the spirit of freedom and respect for the new achievements of science, and also promoted interdisciplinary dialogue between scientists and philosophers.

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Keywords

Center for Interdisciplinary Studies, Michał Heller, Józef Życiński, History of Polish Philosophy, Philosophy of Nature, Philosophy in Science.

40 years ago the first issue of journal *Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)* appeared.[1] Over the past four decades, the magazine has obtained a reputation in Poland, appearing continuously to the present days. Despite the transformations in science and technology, changing political realities, and sometimes also debatable attempts to modify scientific standards by academic establishment, the journal remained the mainstay of independent and creative thinking in the field of philosophy practiced in the context of modern mathematics and natural sciences. Therefore, there is an opportunity to consider the future of the periodical, and also to summarize and at least provide initially outline the development of the Center for Interdisciplinary Studies (Polish name: Ośrodek Badań Interdyscyplinarnych, OBI), an institution closely related to this journal.[2] Regular publication of *Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)* was in fact one of the

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[2] Due to the length of this paper, I will provide the outline of the early period of activity of the Center for Interdisciplinary Studies, illustrate the history of this institution until 1993. The regular organization of interdisciplinary conversations in the form in which they were held since the end of the 1970s, has been stopped this year. Due to the fact, that these seminars had a significant impact on the creation and development of the Center for Interdisciplinary Studies (as will be discussed later in this article), it seems appropriate to adopt this date, symbolically closing the early stage of the Center for Interdisciplinary Studies activity.

key factors of activity of this institution, and the content of individual numbers testified to the area of research interests of the members of the Center, with the founders: Michał Heller and Józef Życiński.

Center for Interdisciplinary Studies operated at the Faculty of Philosophy at the Pontifical Academy of Theology (PAT) in Kraków. PAT was an ecclesiastical university established by pope John Paul II (*motu proprio* "Beata Hedvigis", 1981). In fact, this academic institution derived from the former Faculty of Theology of the Jagiellonian University, erected in 1397, and formally liquidated by a unilateral decision of the communist authorities of the Polish People's Republic in 1954. Despite the decision of the communist government, intellectual traditions in Kraków have survived, as a result of which the former Faculty of Theology has not stopped trying to regain its rights and conducting autonomous activities based on catholic church law. Over the years it has resulted, *inter alia*, in structural changes within the department, caused by the gradual development of the group of philosophers. The efforts of catholic philosophers brought desirable effects, which resulted in the creation of the Faculty of Philosophy, established in 1976, but for various reasons—mainly of a political nature—functioning as a separate scientific unit exclusively from 1981.

The Center for Interdisciplinary Studies was not established as a completely autonomous and ideologically independent institution, devoid of references to important intellectual traditions—the origin and initial development of Center can even be considered in the context of the specificity of the Kraków scientific milieu, including the philosophers associated with the Faculty of Theology of the Jagiellonian University. So, before I begin to discuss the first stages of the

Center's development, I will briefly present how the interdisciplinary tradition in Kraków was born and shaped in the period preceding the initiatives of M. Heller and J. Życiński.

1. Interdisciplinary traditions in Kraków – from S. Pawlicki to K. Wojtyła

Due to historical reasons, Kraków can be considered as the place of the greatest philosophical traditions in Poland. These traditions were also cultivated and developed by the Faculty of Theology of the Jagiellonian University. Christian philosophy was developed for several centuries in this Faculty, although its rapid development when encyclic "Aeterni Patris" by pope Leon XIII was published in 1879. Then, the pioneer of modern philosophy in Kraków became Stefan Zachariasz Pawlicki, who was the first scholar appointed in 1882 to the head of the Department of Christian Philosophy at the Faculty of Theology at the Jagiellonian University. Pawlicki turned out to be one of the most important Polish philosopher of the second half of the 19th century, who tried to develop modern, non-thomistic philosophy based on the models taken from ancient philosophy and acceptance of modern science results (Polak, 2017b).

Another philosophers of the Department of Christian Philosophy—the neoscholastic philosopher of nature (Franciszek Garbryl), historian of the philosophy (Konstanty Michalski) or famous logician, representative of the Kraków Circle (Cracow Circle) (Jan Salamucha), laid the foundations for the philosophy trying to show a coherent image of the broadly understood modern culture and faith. Parallel to their activities, Kraków's philosophy also developed among non-scholastic scientists and philosophers. These scientists were interested in philosophy, although many scholars at that time shared the opinion of the positivists, questioning the cognitive value of the philosophical reflection on nature. Meanwhile, in Poland, especially in Kraków, even in the interwar period (1918-1939), the philosophy of nature had its representatives who addressed specific issues for it, e.g. during the Polish Philosophical Conventions (Polskie Zjazdy Filozoficzne). It should be noted, however, that the Kraków milieu—starting from the initiatives of Władysław Heinrich and Maurucy Straszewski—stressed the development of interdisciplinary dialogue between scientists and philosophers. Philosophical issues were mainly taken up in the context of formal sciences, physics, biology and psychology. Concepts and propositions developed at that time are considered to be crucial in the development of the Kraków philosophy of nature (Heller and Mączka, 2007; Mączka, 2007; Polak, 2018b).

After the Second World War (1945), Polish philosophers faced the challenges of Marxist ideology. In that difficult period of the Polish history, Kazimierz Kłósak and Tadeusz Wojciechowski were active in the field of philosophy of nature in Kraków[3]. They tried to develop the philosophy of nature in the neoscholastic context (methods, terminology etc.). However, they differed from orthodox thomists, because they attached more importance to the achievements of natural science, trying to reconcile new scientific theories with classical philosophy. As a result, they become key representatives of "open thomism" in Poland.

The intellectual efforts of these philosophers coincided with the activity of Karol Wojtyła in Kraków. Already in the 1950s, Wojtyła readily participated in informal discussions among physicists from

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[3] Both, Kłósak and Wojciechowski were philosophers from Faculty of Theology in Kraków.

the Jagiellonian University. Over time, these meetings began to take on a more organized form, which favored the exchange of ideas between Kraków's philosophers and scientists. The year of 1963 was important, because Wojtyła was appointed a Kraków bishop. He quickly began to develop the local milieu, initiating a series of interdisciplinary events. One of this event can be mention here, for example, the philosophical symposium devoted to the analysis of the kinetic point of departure and the teleological argument for the existence of God, which took place in the residence of Cardinal K. Wojtyła in Kraków (January 10-11, 1968), and in which participated scientists from Faculty of Theology in Kraków, Academy of Catholic Theology in Warsaw (Akademia Teologii Katolickiej w Warszawie) and Catholic University of Lublin (Katolicki Uniwersytet Lubelski), as well as professors and professors of physics and biology from the Jagiellonian University.[4] Among the speakers were: K. Kłósak, J. Iwanicki, S. Kamiński, M.A. Krapiec, M. Lubański, A. Stępień, S.J. Zdybicka, Sz. Ślaga, B. Bejze, et al.. Scientist were represented, among others, by S.J. Twarowska, L. Balczewski, Z. Chyliński, J. Janik.

Scholars and philosophers meeting was not the only such event in Kraków in this period. Wojtyła initiated further symposia—thanks to that, philosophy continued to develop in the spirit of interdisciplinary cooperation. One can mention in this context the symposium, which was held on January 9-10, 1970 in the archbishop's residence of Cardinal Wojtyła. The participants of the event were professors and lecturers of Catholic universities in Poland and scientists from the Jagiellonian University, mainly physicists, though there were also philosophers—an active participant was, for example, Roman Ingarden (Heller and Mączka, 2006). During the meeting a few topics

[4] Extensive report from this event, see (Morawiec, 1968).

emerged in the field of natural philosophy, including metaphilosophical question concerning the way of doing the philosophy in the context of modern scientific knowledge. The problem of theory of philosophy of nature was discussed, especially the issue of the language of this discipline in the context of terminology used by physicists. The symposium was interdisciplinary and constituted another attempt to maintain the Kraków ambience of cooperation between philosophers and scientists. Cooperation, which did not always bring results in the form of working out a common position of scientists and philosophers. Michael Heller, who participated in the symposia initiated by Wojtyła, mentioned, for example, that "one of the philosophical lectures rose up one of the physicists and said to the speaker: 'I don't understand this'. You must know that in the physicists language the sentence 'I don't understand this' means that it is impossible to understand, that this is nonsense. But the philosopher did not know the habits of physicists and began to explain again what he meant. It struck me then that the languages spoken by them are so divergent" (Heller, Bonowicz et al., 2016, pp.227–228).

M. Heller was a young philosopher at the time and he has just begun his academic career in Kraków. Although he was associated with the local community earlier, he was officially employed at Faculty of Theology in 1972. His activity in Kraków was also related to the efforts of K. Wojtyła, who tried to develop the scientific and didactic facilities of Faculty of Theology (since 1974 Pontifical Faculty of Theology) related to the philosophy of nature. Shortly after, Józef Życiński (K. Kłósak's pupil) became a close associate of M. Heller.

Heller and Życiński got to know each other at the beginning of the 1970s, but they started a more profound cooperation a few years later, when Życiński was preparing his doctoral dissertation (Heller, Bonowicz et al., 2016, p.230). Heller was in that time a philosopher with considerable scientific achievements, who was not interested in developing the traditional, neoscholastic philosophy of nature. In contrast to the thomists, he emphasized to identify the philosophical problems in the sciences and then analyze them using methods of contemporary logic and science. He was looking for inspiration to work in the field of philosophy in natural science, not in closed philosophical systems (Polak, 2019). It is not excluded that the space of dialogue and freedom of scientific research promoted by Wojtyła favored the development of the philosophical views of Heller and Życiński, who wanted to maintain a dialogue between science and philosophy. In such circumstances, the idea of organizing interdisciplinary seminars arose.

2. Development of the Center for Interdisciplinary Studies

Interdisciplinary seminars were initiated by M. Heller and J. Życiński in the autumn of 1978. These meetings between philosophers and scholars inaugurated the activity of the Center for Interdisciplinary Studies, which aimed to conduct research in the field of philosophy of nature, philosophy and history of science, the relationship between science and religion, etc. The seminars organized by M. Heller and J. Życiński in Kraków referred to the earlier initiatives of K. Wojtyła.

Starting from the first seminar on October 27, 1978, many scientists participated in the meetings organized by M. Heller and J. Życiński—not only philosophers and theologians, but also biologists, physicists or mathematicians—who discussed problems arises on the borderline of philosophy and science, faith and art. Speakers and listeners met initially at Franciszkańska 3 street on the first Friday after the fifteenth day of each month, and when the group of participants began to grow (lectures were heard by up to 200 people sometimes), meetings were moved to the Augustinian monastery at Augustiańska street. Despite the poor conditions, lack of financial resources and official statutes, and at the same time against the reluctance of state authorities to the Church, the philosophical milieu at Pontifical Faculty of Theology developed extremely well, with its initiatives acting as a "clear primacy of the spirit over matter" (Skoczny, 1999, p.13). According to Włodzimierz Skoczny, a deponent of those meetings, "the ability to not notice shortcomings, and only to notice the positives has been perfected by the participants. Indeed, it is difficult today to reflect the unique atmosphere of those days. Something imperceptible hovered in the air, something that had a taste of self-denial, friendship and Truth" (Skoczny, 1999, p.15; see also Życiński, 1999).

The seminars were organized in two thematic series: "Science—Faith" (1978-1991 and 1992-1993) and "Science—Philosophy—Art" (1983-1985). The first meeting in October 1978 was opened by M. Heller and J. Życiński.[5] Over time, the seminars began to gather speakers representing other scientific centers, also from outside Poland.[6] In the mid of 1980s, the meetings organized by M. Heller and J. Życiński were permanently included in the calendar of important scientific initiatives in the country. The lectures were given primarily by physicists and astronomers, among others: Andrzej Staruszkiewicz, Konrad Rudnicki, Zygmunt Chyliński, Jerzy Rayski, Carl Friedrich von Weizsäcker, Bronisław Średniawa, Jan Mozrzy-

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[5] M. Heller gave a lecture entitled „The problem of extrapolation in theology" („Problem ekstrapolacji w teologii"), in turn J. Życiński—"Contemporary tendencies in the philosophy of science" („Współczesne tendencje w filozofii nauki"), see (Liana and Mączka, 1999, p.133).

[6] E.g. Ch. W. Misner, J. Dougherty, W. Greenberg (USA), E. Barth (Netherlands), M.A. McCallum (Great Britian), L. Michel (France).

mas, Jerzy Janik, Charles W. Misner, Andrzej Fuliński, Leszek M. Sokołowski, Małgorzata Głódź. Despite the significant advantage of papers in the field of physics and the philosophy of inanimate nature, as part of the seminars lectures were also delivered by mathematicians (including Krzysztof Maurin, Stanisław Krajewski), chemists (Zbigniew Grabowski) and philosophers (including Jan Woleński, Stefan Amsterdamski, Barbara Tuchańska). The subject matter of the presentations coincided with the interests of M. Heller and J. Życiński: lectures devoted to philosophical problems in physics, relationship between science and religion, fundamental problems of the philosophy of mathematics, there were also presentations addressing contemporary issues of the philosophy of science. Philosophy of biology generally played a smaller role—over several years only a few papers devoted to the topic of the origin and evolution of life were declaimed.[7]

In later years, seminars were held with less regularity, however, still attracted many interested scholars who could participate in discussions on current problems occurring at the borders of science and philosophy or science and religion. Guests representing other research centers from Poland and abroad continued to appear (including A. Plantinga, S. Desjardinis, L. Kostro, and E. Mickiewicz), which allowed to strengthen Center's position on the national and international arena. In 1990, the continuation of the seminars was stopped for some time, and then returned in 1992-1993 at Jagiellonian University. At that time, several meetings took place, initiated by M. Heller and the physicist and philosopher Andrzej Fuliński. Af-

[7] It is worth mentioning at this point that also the seminar entitled "Science-philosophy-art" (although some sources give a different title: "Art-religion-science", initiated in May 1983, was very popular. The first session was attended by 130 participants. It is noteworthy that scientists such as K. Maurin and Z. Chyliński also declaimed their papers as part of the seminars.

ter 1993, the idea of interdisciplinary meetings was no longer carried out in the same scope as before, and the role of seminars was taken over by the Methodological Conferences, organized annually.

The seminars organized by M. Heller and J. Życiński resulted in the creation of the periodical *Zagadnienia Filozoficzne w Nauce* (now entitled *Philosophical Problems in Science*)—the first strictly philosophical journal published by Pontifical Faculty of Theology in Kraków. The first issue of this journal appeared in 1979 and contained eight articles. The journal, edited by M. Heller and J. Życiński as a yearbook, initially contained primarily materials delivered as part of the seminars, although there were also content independent of the Kraków's meetings.[8] From the first issue, the journal was focused on publishing papers from the frontiers of philosophy, formal and natural sciences. An important part of the magazine were reviews of the scientific books, those published in Poland and abroad. Philosophers and representatives of other scientific disciplines, interested in broadly understood philosophical issues, published in this journal. Already in the first issues of the magazine, there were also translations of well-known scholars, including A. Einstein or K.R. Popper. The scope of the journal reflected the research interests and

[8] Publishing of this periodical was initiated by M. Heller together with J. Życiński for two reasons. First, both philosophers wanted to create their own independent journal on philosophy and science. The second reason was practical—people who delivered papers as part of interdisciplinary seminars gave M. Heller and J. Życiński texts that formed the basis of their lecture. There was, therefore, a need to publish the texts of the speeches, although it was not possible to obtain official permission from the authorities to publish the periodical. *Philosophical problems in science (Zagadnienia Filozoficzne w Nauce)* appeared, therefore, as *samizdat* (Heller, 2017). *Samizdat* (Russian origin word, coined from *samodielnoje izdatielstwo* or *sam izdaju*, means "self-publish") was a clandestine print, published as a form of dissident activity in countries where communist censorship was active. Samizdats were non legal and not allowed to be distributed, therefore individuals reproduced underground publications and passed them from person to person.

approach to practicing philosophy that began to emerge in Kraków in the vicinity of M. Heller and J. Życiński. This is especially visible in relation to other periodicals in the field of natural philosophy issued by catholic centers in Poland—in relation to the series *Z zagadnień filozofii przyrodoznawstwa i filozofii przyrody* (Warsaw) published by K. Kłósak, or journal *Roczniki Filozoficzne* (Lublin), the Kraków periodical distinguished a greater focus on contemporary philosophy of science (especially the philosophy of physics) and a departure from taking classical problems in philosophy in the context of the methods and language of the neo-thomist philosophy of nature.

M. Heller and J. Życiński were focused on developing an interdisciplinary milieu also outside the Poland. An expression of progressing internationalization was creation of the English-language equivalent of *Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)* published in Polish in that time. From 1983, the Center for Interdisciplinary Studies began publishing the journal *Philosophy in Science* in cooperation with the Vatican Observatory and the Pachart Publishing House in Tucson.[9] The editors of the newly created periodical were M. Heller (affiliated to the PAT and the Vatican Astronomical Observatory), J. Życiński (PAT) and William R. Stoeger SJ (Vatican Observatory). The publisher of the magazine was a Polish

[9] It is noteworthy that on the front-page of the magazine the name "Center for Interdisciplinary Studies" was mentioned as the organization initiating the publication of this periodical. This proves that the Center for Interdisciplinary Studies was already an organization operating in the field of science and cooperating with other research institutions.

astronomer living in the USA Andrzej G. Pacholczyk (University of Arizona), founder and chairman of the Pachart Foundation and director of Pachart Publishing House.[10]

The aims of the editors of this periodical were convergent with the philosophical program of M. Heller and J. Życiński. The main task was to develop an interdisciplinary dialogue between science and philosophy, especially the contemporary philosophy of science (Heller, Stoeger and Życiński, 1983, p.8). The journal was conceived as a forum for discussion of philosophical issues emerging in the natural sciences. The article that opened the first issue of the magazine was W. G. Stoeger's text entitled "The Evolving Interaction Between Philosophy and the Sciences: Towards a Self-Critical Philosophy", in which author argued that contemporary philosophy must be internally open to changes generated by scientific achievements. In this sense, the constant nature of self-criticism should be inscribed in the nature of philosophy (Stoeger, 1983, pp.39–43). Interestingly, this view was supposed to be a sort of editorial program, and the article itself was considered one of the first attempts at the theory of "philosophy in science" (Heller, 1986, p.7; Heller, 2019, p.208).

The journal *Philosophy in Science* was not the only manifestation of the opening of Kraków philosophers to the foreign audience[11]. Although the Center for Interdisciplinary Studies operated at the Faculty of Philosophy of the Pontifical Academy of Theology in Kraków, M. Heller and J. Życiński also cooperated with the Vatican Observatory at Castel Gandolfo. Thanks to this cooperation, the organization of an international session was possible. On May 24-25,

[10] A few years later, the editors of *Philosophy in Science* started to publish the book series *Philosophy in Science Library*, in which philosophical works of M. Heller, A.G. Pacholczyk, Józef Życiński and G. Tanzella-Nitti were published in English.

[11] Five volumes of this periodical had been published by 1993. Unfortunately, currently the magazine is not published—the last, 10th issue appeared in 2003.

1984 the English-speaking symposium "The Galileo Affair: a Meeting on Faith and Science" took place in Kraków. Many scientists and philosophers from outside Poland actively participated in this conference (W.A. Wallace, J. Dietz-Moss, J. Casanovas, G. Coyne, O. Pedersen, U. Baldini, F.M. Hetzler, P. Mitra), and also scholars representing native universities (apart from M. Heller and J. Życiński, speeches were given by M. Lubański, J. Dobrzycki, and K. Rudnicki). The symposium was also to be attended by P. K. Feyerabend, but finally he did not appear in Kraków.[12] Individual sessions were held in the archbishop's palace, made available by Cardinal Franciszek Macharski, as well as in the rooms of the Faculty of Philosophy of the PAT at Augustiańska street and the Collegium Maius at Jagiellonian University (Skoczny, 1984).

It should be noted that the symposium was organized in connection with the anniversary of the Galilean trial. For philosophers of nature from the PAT, it was also an opportunity to take up issues that coincided with the interests of the local environment—the history of science and the issue of the relationship between science and faith are key research areas of philosophers and scientists from the circles of M. Heller and J. Życiński. Finally, the symposium was a great opportunity to manifest the existence on the international academic map of PAT as a scientific center in which advanced research in the field of philosophy is conducted. The event turned out to be an organizational success, which was confirmed by W. Skoczny, writing about the atmosphere of "full care of the hosts and unconcealed admiration from foreign guests. For many of them it was the first meeting with Poland, which although poor in material means, was able to fascinate people" (Skoczny, 1984, p.73). However, since the symposium

[12] P. K. Feyerabend was represented in Kraków by his co-worker I. Sieb-Madeja, who recreated the contents of a paper by an Austrian philosopher from a tape recorder.

was organized by an illegal university—from the point of view of the Polish authorities at the time—post-conference materials could only appear as samizdat, in this case issued in cooperation with Specola Vaticana (Coyne, Heller and Życiński, 1985).

In the following years, the Center for Interdisciplinary Studies organized many scientific conferences, as well as national and international symposia. In the years 1987-1993 Center organized: international symposium "Newton and the New Direction in Science" (1987) , the conference "Why is nature mathematical?" ("Dlaczego przyroda jest matematyczna?", 1989), the international conference "Universals and Particulars in the Context of Modern Science" (1990) , conference "Relationships between science and religion in catechesis. Problems of biological evolution" ("Relacja nauka—wiara w katechezie. Problematyka ewolucji", 1991), international conference "Theology, Philosophy and Cosmology: On West and East "(1991), symposium "Cosmos and philosophy" ("Kosmos i filozofia", 1992), scientific session "Theology and science from ancient to the Renaissance" ("Teologia a nauki od starożytności do renesansu", 1993). Many scholars outside Poland participated in these events, like W. B. Dries, S. Jaki, J.D. Moss, M. Novak, D. Park, O. Pedersen. The associates of M. Heller and J. Życiński actively participated in the organization of scientific meetings: W. Skoczny, A. Michalik, Z. Liana, J. Mączka, A. Fuliński. Post-conference materials appeared after almost every scientific event organized by Center.

Surprisingly, despite the difficulties presented by the government, activity in the field of publishing turned out to be an important element of the Center for Interdisciplinary Studies. M. Heller and J. Życiński also cooperated in this field and published common books in early 1980s by the Polish Theological Society (Polskie Towarzystwo

Teologiczne) in Kraków[13]. Although the beginnings were modest, in later years the publishing activity of Center was much greater—books and scripts signed with the name of this research institution or issued in cooperation with other publishing houses were published regularly every year (e.g. Heller, Michalik and Życiński, 1987; Coyne, Heller and Życiński, 1988; Heller, Życiński and Michalik, 1990; McMullin, 1990; Heller, Skoczny and Życiński, 1991; Wolak, 1993).

The early process of development the new approach to practicing the philosophy of nature in PAT was also related to the scientific activity of students of M. Heller and J. Życiński. The diploma thesis of students concerned the topics related to the research interests of the creators of Center, i.e. contemporary problems of philosophy of nature, philosophy of science, philosophy of language and the relationship between science and religion.[14] It is significant that the first doctorate defended at the Faculty of Philosophy of the PAT in January 1983 was prepared under the supervision of M. Heller. The dissertation entitled "Strukturalne relacje między językiem, myśleniem a rzeczywistością" ("Structural relations between language, thinking and reality") prepared by Krzysztof Turek received positive reviews of J. Życiński and M. Lubański, and its author became the first doctor

[13] (Heller and Życiński, 1980; 1983). M. Heller commented the way of making common books: "The method of writing was such that we first made a rough plan of the book and then shared it—this chapter is written by me, this one by you. We didn't write together, but after we wrote the chapters, we read them to each other and made minor adjustments. When you take our books in your hand, you can immediately see who wrote which chapter, because the style of each of us is different (Heller, Bonowicz et al., 2016, p.231).

[14] Various aspects of the Center research activities have already been discussed (see e.g. Krauze, 2008; Mączka, Skoczny et al., 2012; Skoczny, 2012).

appointed by the authorities of the newly established university.[15] In the initial period of the Faculty of Philosophy of the PAT, the bachelor's thesis—based on the work "Argument kinetyczny za istnieniem Boga w ujęciu K. Kłósaka" ("Kinetic argument for the existence of God in K. Kłósak's concept")—in turn, a student of J. Życiński, W. Skoczny. The interest in the problem of philosophy of physics was continued by W. Skoczny in his doctoral dissertation from 1986, entitled "Filozoficzne aspekty zasady antropicznej" ("Philosophical aspects of the Anthropic Principle").[16] In the same year, Z. Liana graduated the title of MA in philosophy[17], and the canonical licentiate was obtained by J. Dadaczyński[18]—another students of J. Życiński, who will become permanently associated with Center and PAT in the following years.[19] Over the years, a group of students gathered around

[15] PhD defense was preceded by the publication of two articles in *Philosophical Problems in Science (Zagadnienia Filozoficzne w Nauce)*: (Turek, 1978; 1981). Recently, these works met with interest and were analyzed (Krzanowski, 2016).

[16] W. Skoczny's article was related to the subject of his doctoral dissertation (Skoczny, 1985).

[17] The Master's thesis prepared by Z. Liana was entitled: "Rola matematyki w poznaniu naukowym w ujęciu Rene Thoma" ("The Role of Mathematics in Scientific Knowledge from the Rene Thom's Point of View").

[18] Canonical licentiate was awarded owing to dissertation entitled "Problem racjonalności teizmu chrześcijańskiego w *Neues Glaubensbuch*" ("The problem of rationality of Christian theism in Neues Glaubensbuch's view"). Paper related to this dissertation see (Dadaczyński, 1984).

[19] It is worth mentioning here that the academic degrees awarded by the PAT were not acclaimed by the state authorities at this time. It happened that doctoral students defended dissertations at other universities. For example: Jan Woleński, then associated with the Jagiellonian University, became the promoter of Krzysztof Gurba, who defended his dissertation entitled "Methodological aspects of the representation of linguistic knowledge" in 1986. This dissertation was created at the seminar of J. Życiński, although his defense took place at the Jagiellonian University (Woleński, 2017).

M. Heller and J. Życiński began to grow.[20] Academic degrees were soon obtained, among others, J. Dembek, J. Mączka, A. Olszewski, T. Sierotowicz, Z. Wolak, W. Wójcik.

The early development of the Kraków philosophical circle reminds, in some respects, the early stage of the Lvov milieu around Kazimierz Twardowski, from which the Lvov-Warsaw school emerged. Why do I make such an analogy? As in the case of the Lvov-Warsaw school, one can point to the factors determining the intellectual formation of the trend, which I would call initially the Kraków school of "philosophy in science": the genetic factor (activities of M. Heller and J. Życiński), geographical factor (scientific activity in Kraków), temporary (development in the 1980s and 1990s) and substantive[21] (although students of M. Heller and J. Życiński conducted research in various fields—such as philosophy of nature, philosophy of science, logic, philosophy of mathematics, history of science, and Polish philosophy of nature—it combined their conviction, taken from their teachers, that philosophy should be practiced in a strict, critical way and in connection with the scientific and methodological knowledge, which at the same time does not exclude possible references to metaphysics and Christian theology). These criteria for belonging to the "philosophical school" obviously do not have

[20] It is worth mentioning that students and collaborators of M. Heller and J. Życiński undertook many initiatives to build unity and develop the scientific community in Kraków, e.g. they prepared reports on the activities of interdisciplinary seminars, discussed important academic events or undertook initiatives aimed at reaching agreement between various currents philosophies practised at the Pontifical Academy of Theology, see e.g. (Michalik, 1984; Głódź, 1987; Samborski and Wójcik, 1987; Liana, 1989; Dembek, 1990; Wolak, 1992; Samborski and Wójcik, 1992; Wolak, 1993).

[21] According to Woleński, all these four factors (genetic, geographical, temporal and factual) were to determine the intellectual formation of the Lvov-Warsaw school (Woleński, 1985). It is debatable whether these criteria are not too broad, considering also the difficulty of clearly defining what a "philosophical school" is.

to be exhaustive; however they suggest the existence of a specific, still developing intellectual community in Kraków, which stands out in comparison with other philosophical trends in Poland and abroad with a specific approach to analyzing philosophical issues that appear in the mathematical and natural sciences (Heller, 1986; Heller, 2019).

3. Summary and research perspectives

The Center for Interdisciplinary Studies played a key role in developing the academic dialogue between science and philosophy in Poland, and the range of its initiatives extended far beyond the local context. M. Heller and J. Życiński proved to be not only continuators of intellectual traditions, developed in Kraków from at least the second half of the nineteenth century (Polak, 2013), but also proposed their own original scientific attitude, which involved a number of research initiatives in the area of relations between science and philosophy, as well as science and religion (Polak, 2019). Large-scale publishing and organizing activities turned out to be a unique phenomenon on a national scale, and we can still use the fruits of M. Heller and J. Życiński's labor to this day.

It should be noted that the organizational activity of M. Heller and J. Życiński has not yet been sufficiently examined in terms of historical and philosophical aspects, including the political context and in reference to the activity of other scientific centers in Poland and abroad. Undertaking such research seems justified—they could show and emphasize the specificity and originality of Center on the national and international background, while providing answers to the question of the role played by this institution in upholding a reliable scientific discussion at a time when the ideal of interdisciplinary cooperation was not yet so common, and in some places—such as Poland—was even treated by the authorities as undesirable.

Certainly, it is also worth considering whether the history of the Center—along with its later face in the form of the Copernicus Center for Interdisciplinary Studies—no longer deserves a book study. 40 years of activity in the field of science, organization and publishing is a extensive material for historical analysis, in which one could also take into account the large-scale activity of students and associates of M. Heller and J. Życiński. Considering the significance and scope of the Kraków interdisciplinary environment in Poland and abroad, it would be important not only from the scientific point of view, but also for the promotion of Kraków as a special place in Poland, where interdisciplinary intellectual traditions are still successfully nurtured and developed.[22]

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[22] An example of the creative development of ideas initiated by M. Heller and J. Życiński is, among others, conducting research in the area of the so-called theology of science (Mączka and Urbańczyk, 2015), philosophy of computer science (Polak, 2016; 2017a; 2018a; Krzanowski, 2017), methodological aspects related to the language of theology (Olszewski, 2018) or evolutionary theology (Grygiel, 2018).

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Recenzje

Book reviews

Parallel Worlds of Faith and Science in the Russian Intellectual Milieu

Teresa Obolevich, *Faith and Science in Russian Religious Thought*, Oxford University Press, Oxford-New York 2019, pp.240.

The new book written by Teresa Obolevich carries on the details and nuances of the thoroughness of the perception of the world by the orient society in the relation of science and religion. Admitted emphasis opens a door to the peculiar development of the scientific and religious spheres as well as a tendency of accepting the world by the Eastern society from the 10th century up to our times. So, the book opens the door for understanding the specificity of development of the world by the mind which managed to fight with secular intentions. As author shows, that the starting point of the outlook in the East was spirituality which through ages had improved its wise nature and was ready to accept the philosophical tendencies along with scientific ones. Notably, that geographically the most complete would be say that the orient or the Eastern Christian society here means the lands which in the 18th century were encompassed by the Russian Empire but could be started as separated centers.

The unique clue of the book is that the author pays attention to the relationship of faith and science, not to faith and reason. It is crucial to admit while the represented relation has a special stress, is actual and with precise nuances, while former is more narrow than the later. Hence, this postulation describes the picture of how the scientists contributed into their field keeping God as be a source of inspiration.

It worth noting that the author successfully selects the parts of the curriculum giving access to reach the full extent of the possibility to enter to the relationship between two disciplines. A reader can admit that this linkage is not monotonous and obtains a very different character with numerous interpretations and meanings, as well as that religious attitude, still, which sometimes even reached fundamentalist approach. Another thing, it becomes clear from the text the contrast of the developing of this approach in the West and the East.

In the represented orient world the author pays most attention to two eras: the Enlightenment and the

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Silver Age each of which got here a special signification. The former associated with the enlightenment of Christ (and here is clear contradiction to Western enlightenment), the latter is admired by the new brief of philosophy as a pillar for development of scientific view too.

During the history the interest in the relationship between science and religion in the East had been included in the interest of researchers from different fields, church and religious figures, and experts in the natural sciences. Such effort demonstrates that the Eastern world was open to the western influences, yet usually it accepted them through spiritual filter. Even a "pure materialistic" position (for instance, represented by Tsiolkovsky) signs about a like-God's presence in the world. Generally speaking, any materialistic wind came from the secularized West mostly was described as a quasi-religious.

Obolevich establishes a few incentives of such cause grounded first of all on theological and consequently philosophical background among which are Biblical recourses and patristic tradition, sacred art and literature and which mainly originated the Eastern thought. Taking them into consideration, thereafter a reader can find out in the numerous proposed ideas and concepts the presence the controversy of St. Basil the Great and St. Gregory Palamas formula which originally distinguishes between God's essence and the presence of His energies in the world. What follows from this standpoint, is that the researchers in this case work not with just matter, but with the expressions of God's potentiality and through which it is able to recognize Him. Thus, field of science does not contradict to the existence of Creator but through the acknowledgment of creation is opened as one of the ways to recognize God. Therefore, science is a kind of common deal which could be also called as a cosmological liturgy.

Still, it would be a mistake to say about uniformity of the outlooks in the relationship between science and religion in the Russian philosophical perspective, while in the book the author takes a clear attempt to show the diversity of approaches. Even they are organized in the streams with numerous representatives including concordism, cosmism, the Neopatristic synthesis, panpsychism, Pantheist and panentheism concepts, etc.

The book invites to make one view wider accepting the ability to look at the science as well as on religion from a distinctive prospect showing the presupposition of rational and supra-rational order. On the one hand, such recognized persons like Mikhail Tugan-Baranovsky, Mikhail Lomonosov, Nikolai Fedorov, Vladimir Vernadsky demonstrated their ability to combine science and spiritual experiences. On the other, such the famous religious figures as Vladimir Soloviev, Fr. Pavel Florensky, Nikolai Lossky, and others thinkers had a tendency to keep in mind the discoveries in the scientific fields and correlate them with the religious understanding of the world. Then, the book is useful for those who are open to go deeper into the understanding of the Russian Religious Philosophy and get precise and correct knowledge about it unique ability and quest to harmonize faith and science representing them as two polls of humane activity. These numerous projects could saturate the curiosity of the reader prompting to continue to prospect the Eastern Christian religious tradition.

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