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48

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Science Networks. Historical Studies
Founded by Erwin Hiebert and Hans Wußing
Volume 48

Edited by Eberhard Knobloch, Helge Kragh and Volker Remmert

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The translation has been funded by the Foundation for Polish Science.

ISSN 1421-6329 ISSN 2296-6080 (electronic)
ISBN 978-3-0348-0830-9 ISBN 978-3-0348-0831-6 (eBook)
DOI 10.1007/978-3-0348-0831-6
Springer Basel Heidelberg New York Dordrecht London

Library of Congress Control Number: 2014946972

Printed on acid-free paper

Springer Basel is part of Springer Science+Business Media (www.birkhauser-science.com)

Dedicated to
my wife Hania and daughter Zosia

Contents

Introduction

The aim of this book is to present and analyse a number of philosophical concepts concerning mathematics and logic as formulated by Polish logicians, mathematicians and philosophers in the interwar period. Why was this period chosen? It was a special time in the history of Polish science, especially in the history of Polish logic and mathematics. This period saw the development of the Lvov-Warsaw School of Philosophy and the Warsaw School of Logic, which was related to the former, as well as the Polish School of Mathematics, which set the direction of advance of mathematics, logic and philosophy (in particular, analytic philosophy) for many years. Moreover, investigations conducted in these schools and the results obtained won world recognition and belong to the most important achievements in the particular domains. Therefore, it is worth asking the question whether this development of logic and mathematics was accompanied, and to what extent, by a philosophical reflection. Specifically, it is worth formulating the following questions: (1) Was the research in mathematics and logic conducted in interwar Poland connected with some philosophical concepts, more precisely methodological or more generally epistemological ones, concerning mathematics and logic, or did their sources have any philosophical motivations and convictions or were they autonomous fields? (2) If they were autonomous domains what were the 'private' philosophical preferences of their authors and why did they not exert any influence on the research itself in logic and mathematics? (3) If this logic and mathematical research was based on certain philosophical assumptions then what were these assumptions? (4) Did the close collaboration between philosophers and mathematicians in interwar Poland (in fact, it also had an institutional dimension apart from the personal and private ones) force the latter to become interested in philosophical issues? (5) Were the achievements and results in mathematics, and especially logic, the starting point of formulating some philosophical concepts concerning these domains? (6) Were there any original concepts in the philosophy of mathematics and logic in Poland? (7) What was the attitude of Polish logicians, mathematicians and philosophers towards the concepts of the philosophy of mathematics and logic, which were formulated and intensively developed in the first half of the twentieth century, namely logicism, intuitionism and formalism?

This book seeks to answer these questions by analysing both the philosophical statements and research practice of the outstanding Polish mathematicians and logicians living in the interwar period. From among all possible methods of presentation, we have chosen the way according to the people in question, i.e. subjective way—not forgetting the influences, especially the environmental or institutional ones to which scientists were receptive. Aiming at a certain systematisation and ordering of our analyses we have decided to divide the scientists in question into the following groups: the Polish School of Mathematics embracing the Warsaw centre and the Lvov centre (discussed in Chap. 2), the Lvov-Warsaw School of Philosophy and the Warsaw School of Logic, which was related to the former (Chap. 3), as well as a group which we have called (conventionally) the Cracow Centre (Chap. 5). The original views of Benedykt Bernstein have been presented in a separate Chap. 4 as he worked outside all groups and schools although he grew out of the Lvov-Warsaw School.

This division does not solve all problems as far as the places of particular scientists are concerned. Specifically, one should ask the question which group should include Leon Chwistek and Zygmunt Zawirski. The former worked in Cracow and then in Lvov whereas the latter began in Lvov, then moved to Poznań and, finally, was appointed professor (in the year 1937) in Cracow. Since Zawirski was connected with the Lvov-Warsaw School all the time, we have decided to include him in Chap. 3. As for Chwistek, we have placed him in Chap. 2, in the section dedicated to the Lvov School of Mathematics. Formally, Chwistek belonged to this school (he was professor of mathematical logic at the Faculty of Mathematical and Natural Sciences of the Jan Kazimierz University) although the style of his work was slightly different from the remaining Lvov mathematicians. In fact, he grew in the Cracow environment, where he began his research, but he developed his concepts in Lvov, trying to create a school in this city.

Before the interwar period there was no substantial philosophical reflection on mathematics and logic in Polish science, but in order to show the background in which interwar concepts originated, Chap. 1 is dedicated to the philosophical ideas related to mathematics and logic formulated by six scientists who worked from the eighteenth century to the beginning of the twentieth century, namely Jan Śniadecki, Józef Maria Hoene-Wroński, Henryk Struve, Władysław Biegański, Samuel Dickstein and Edward Stamm.

Short biographical sketches of the discussed scientists have been added at the end of the book. They should allow readers to get to know these figures and their views as well as the contexts (including the institutions) they worked in.

Discussing the views of particular scientists, we document our assertions and theses, abundantly quoting their original works. The authors' words themselves best reflect their views (and at the same time illustrate ways of formulating thoughts and argumentation) and thus they cannot be replaced by any discussion. On the other hand, the quotations generally come from works, which are difficult to access, and thus they can in some way substitute for the non-existing anthology of the philosophical texts of Polish logicians and mathematicians working in the 1920s and 1930s. As the discussed texts come from a rather distant period and since that time the rules of the Polish spelling have changed many times the quotations are given (in footnotes) in modern Polish spelling in such a way that readers can understand them and at the same time see the style of old Polish. Naturally, we have not changed the original titles.

This book is an enlarged English version of my monograph *Filozofia matematyki i logiki w Polsce międzywojennej* published in the year 2011 by Scientific Press of Nicolaus Copernicus University as part of the series of the Monographs of the Foundation for Polish Science. The works on the enlargement of the book were conducted within the framework of the research grant of the National Science Centre (grant N N101 136940).

I would like to thank all those who helped me prepare this book. I thank the Foundation for Polish Science for its financial support for the translation and I thank the translator Doctor Maria Kantor for our fruitful and good collaboration. I thank also Doctor Izabela Bondecka-Krzykowska for the help in preparing the index. Special thanks should be directed to Professor Jan Woleński (Jagiellonian University) for our conversations and discussions thanks to which I have learnt a lot about the history of Polish logic, in particular the history of the Lvov-Warsaw School. I am also grateful for all his suggestions, which allowed me to improve the text of the above mentioned Polish monograph being the base for this book.

Chapter 1
Predecessors

In fact, before the 1920s and 1930s no serious philosophical reflections on mathematics and logic existed in Polish science. Naturally, this does not mean that philosophical concepts concerning mathematics and logic, developed in interwar Poland—being the main topic of this book—were formulated in an intellectual vacuum and that earlier there had not been any reflections on mathematics and logic in Poland. Therefore, let us mention six figures that exerted certain influences—each made a completely different impact—on the further development of the concepts in question. These figures are Jan Śniadecki and Józef Maria Hoene-Wroński working at the turn of the eighteenth and nineteenth centuries as well as Henryk Struve, Władysław Biegański, Samuel Dickstein and Edward Stamm at the turn of the nineteenth and twentieth centuries, i.e. closer to the interwar period.[1]

1.1 Jan Śniadecki

Discussing Jan Śniadecki's philosophical views on mathematics we must begin by stating that he was an advocate of Empiricism. He claimed that mathematics was a science about the reality surrounding us, and that the source of this science was experiment. In his *Rozprawa o nauk matematycznych początku, znaczeniu, i wpływie na oświecenie powszechne* [A Treatise on the Beginnings, Significance and Influence of Mathematical Sciences on Common Enlightenment] (1781) he wrote:

> I do not say that the most alienated truths of reason do not take their beginnings from the effects that strike the senses: but the mind by its activities could separate these truths from

[1] One can add Władysław Gosiewski who among other things wrote about the theory of probability, desiring to make it in a way a universal tool of the mathematical theory of physical phenomena. Cf. Gosiewski (1904, 1906, 1909a, b).

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6_1

> their elements so much that leaving them only at the furthest properties it did not relate them to any particular natural cases but only to its own righteousness in action. Such an object was quantity, property interspersed all over nature, detached by reason from all species of things, left only with its essential mark, which depends on the possibility of increasing or decreasing (1837–1839, vol. 3, p. 172).[2]

And further:

> The first bases of mathematics are certain assumptions, clear and infallible definitions, which are nothing else but the effects of quantity drawn from nature and elevated to the furthest possible generality (1837–1839, vol. 3, p. 173).[3]

Thanks to that mathematics is suitable for describing physical reality by its branch called applied mathematics. However, the objects of mathematics are not identical with the objects in the real world. In *Rozprawa o nauk matematycznych początku* Śniadecki writes:

> All physical truths always approach the truths of mathematical images but they can never reach them, which is expressed by the language of geometricians that physical truths are limited by these truths which elementary geometry analyzes as all the truths of elementary geometry are limits of the truths of higher geometry [. . .] (1837–1839, vol. 3, p. 179).[4]

Abstraction serves to uncover essential characteristics common to various phenomena and not to detach science from reality.

Śniadecki stressed the symbolic language of mathematics. In his opinion this language was one of the characteristics that differentiated mathematics of his times from the older mathematics, especially the ancient one. In his work *O rozumowaniu rachunkowem* [On Calculus Reasoning] (1818) he wrote, 'therefore, the whole difference between the science of ancient people and contemporary science depends on language' (1837–1839, vol. 4, p. 244). He saw three rules in using the symbolic language: (1) 'unknown things should be regarded as known and both should be made equal, used and related'; (2) 'reasoning and its cases should be shown in general signs and made known in brief and short words'; and (3) 'unknown things should be separated from the known ones and the former should be expressed by the latter' (1837–1839, vol. 4, p. 244). The symbolic language should be general,

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[2] 'Nie mówię ja, aby najoderwańsze rozumu prawdy nie brały swego początku ze skutków o zmysły bijących: ale te prawdy umysł swoim działaniem tak potrafił od swych pierwiastków oddalić, iż zostawiwszy je tylko przy najodleglejszych własnościach, nie przywiązał ich do żadnych szczególniejszych przyrodzenia wypadków, ale tylko do własnej swej w działaniu prawości. Takim była obiektem wielkość, własność rozrzucona po całej naturze, oderwana rozumem od wszystkich gatunków rzeczy, zostawiona jedynie przy istotnym swym piętnie, które zależy na sposobności powiększania się lub zmniejszania.'

[3] 'Pierwsze grunta matematyki są pewne przypuszczenia, definicje jasne i nieomylne, które nic innego nie są tylko skutki wielkości wyciągnione z natury i do najodleglejszej wyniesione ogólności.'

[4] 'Wszystkie prawdy fizyczne zbliżają się zawsze do prawd obrazów matematycznych, ale ich nigdy nie mogą dosiądz, co się językiem geometrów wyraża, że prawdy fizyczne mają za granicę te prawdy, które geometria początkowa roztrząsa, tak jako wszystkie prawdy geometrii elementarnej są granicami prawd geometrii wyższej [. . .].'

brief and it should support memory. Patterns and symbols should be, however, just tools supporting the mind and—though they have some magic power—they cannot live their own lives. In *Rozprawa o nauk matematycznych początku* he depicted his ideas (cf. 1837–1839, vol. 3, p. 176):

> Today many people expostulate with geometricians about this indecency in calculus that using calculus the human mind grows slack in its activities; since being absorbed in some *symbolic* expression of thoughts and their *mechanical combination* it stops reasoning and reflecting on their real relationship. This accusation offends only those simple calculators who taking the last cases and rules of great theories and reflections use them without thinking, without knowing their beginnings, and thus in complete inaction of their minds; but a geometrician and the one who can earn the name of a true mathematician always know the whole metaphysics of their activities: if he passes from one truth to another he can see the whole theory and follows the range of profound relationships and connections. The written calculus is the effect of his most obvious thought, most undoubted argumentation, which he has made and tied in his mind.[5]

Symbolism, 'calculation, i.e. the way of expressing *symbolically* many thoughts and *combinations*' (1781, cf. 1837–1839, vol. 3, p. 174) is only to facilitate the process of discerning the relations between statements and 'to connect one truth with another' (cf. 1837–1839, vol. 3, p. 173), is to be an external expression of deeper truths and should lead us to discover them. In his work entitled *O rozumowaniu rachunkowem* (1818) he wrote that 'an expression that was successfully introduced by a man of great talent [. . .] can become in the mathematical language either a great simplification of science or art to reach the truth and a source of important inventions' (1837–1839, vol. 4, p. 243).

According to Śniadecki mathematics embraces two methods, which he calls synthetic and analytic, and characterises them in his dissertation *O rozumowaniu rachunkowem* in the following way:

> Therefore, proving some truth or solving some questions in mathematics by drawing figures we follow the synthetic method. Even if we used algebraic signs—but they only shorten ordinary speech—the method would be still synthetic. Yet, if we use letters and general signs to express some truth or solve some question, and if we draw conclusions from reflecting on these letters, from their algorithm, we follow the analytic way in mathematics. [. . .] They are just two ways of reason that reveals itself by its action (1837–1839, vol. 4, pp. 245–246).[6]

[5] 'Wielu dziś wyrzuca geometrom tę nieprzyzwoitość w rachunku, że przy nim rozum ludzki w swych działaniach gnuśnieje; zaprzątniony bowiem symbolicznym wyrazem myśli i *mechaniczną* ich *kombinacją*, przestaje rozumować i zastanawiać się nad prawdziwym ich związkiem. Ten zarzut razi tylko owych prostych rachmistrzów, którzy wziąwszy ostatnie wielkich teorii i refleksji wypadki i reguły używają ich bez żadnego myślenia, bez wiadomości ich początków, a przeto w zupełnej bezczynności ich umysłu; ale geometra i ten, który sobie zasłużyć może na imię prawdziwie uczonego w matematyce, zna zawsze całą metafizykę swego działania: jeżeli przechodzi z jednej prawdy do drugiej, widzi całą teorię i idzie za pasmem głębokich stosunków i związków. Rachunek na papierze wypisany jest to już skutkiem najoczywistszej jego myśli, najpewniejszych rozumowań, które on u siebie uczynił i związał.'

[6] 'Dowodząc więc jakiej prawdy albo rozwiązując jakie pytania przez rysunek figur, postępujemy w matematyce sposobem syntetycznym. Choćbyśmy nawet znaków algebraicznych użyli, ale gdy

The usage of symbols should not cover the fact that in mathematics the essential things are not algorithm and calculation methods but logical relations. According to Śniadecki a mathematician is not 'that craftsman who pays attention only to rules while assembling some *machine*,' but 'is the master of those works, being guided to assemble a similar *machine* by the rules he formulated or by all these *combinations* and thoughts' (1781, cf. 1837–1839, vol. 3, p. 176).

Śniadecki recognised the role and meaning of the then new mathematical discipline, which was probability theory and which he called the 'theory of lots' [rachunek losów] or 'theory at random' [rachunek chybi-trafi]. He regarded it as important although 'not merely the most difficult part of applied mathematics, and [this is] through the subtlety of thoughts it requires and through the depth of high and difficult calculations to which it leads.' He also realised how much one 'should expect from the theory of lots adjusted to other sciences.'

Śniadecki also made a large contribution to creating and popularizing Polish mathematical terminology. In his opinion the language of mathematics is—as he wrote in his work *O języku narodowym w Matematyce* [On the National Language in Mathematics] (1813)—'a language for the eye; we also need a language for the ear, to translate these sciences orally and in writing, and thus we need words and names from our national language' (1837–1839, vol. 3, p. 195). He claimed, 'the language of mathematics, like of any other science, should be as close to ordinary language as possible' (1837–1839, vol. 3, p. 195). Consequently, he lectured—often against the contemporary customs—in Polish. He also proposed numerous Polish terms—some of them, however, were not accepted.

To sum up our reflections on Śniadecki's philosophical ideas related to mathematics, one should state that he saw mathematics as one of the most important sciences, one of the most important expressions of the human spirit. In his remarks on Józef Twardowski's review of his *Trygonometrya kulista analitycznie wyłożona* [An Analytic Treatment of Spherical Trigonometry] (1817), published in *Pamiętnik Warszawski*, he wrote:

> Mathematics is the queen of all sciences, its bridegroom is the truth and its robe is simplicity and obviousness. But the shrine of this monarchess is planted with thorns, through which we must pass. Thorns have no charm—only for those minds that love the truth and like struggling with difficulties, which also shows man's unique and higher order inclination towards really intricate but strong and elevated intellectual delights, strengthening human nature.
>
> Mathematics, which has rendered a service many a time to the society, sciences and arts, will again become a leader of the human mind in all its cognitive activities.[7]

te znaki nic więcej nie robią tylko skracają mowę pospolitą, sposób nie przestaje być syntetyczny. Jeśli zaś do dowiedzenia jakiej prawdy lub do rozwiązania jakiego pytania używamy liter i znaków ogólnych i z rozumowania nad tymi literami, z ich algorytmu, wyciągamy wnioski, postępujemy w matematyce sposobem analitycznym. [...] Zgoła są to dwie drogi objawiającego się swym działaniem rozumu.'

[7] 'Matematyka jest to królowa wszystkich nauk, jej oblubieńcem jest prawda, a prostość i oczywistość jej strojem. Ale przybytek tej monarchini jest obsadzony cierniem, po którym przechodzić trzeba. Nie ma on powabu—tylko dla umysłów zamiłowanych w prawdzie i

Whereas in his treatise *O rozumowaniu rachunkowem* (1818) he wrote:

> The development of mathematics is great and it will never end. It is a purely true ability because it rules independently over the whole area of human cognition. Almost all sciences need mathematics but it does not need any of them as Jan Bernoulli said rightly: *Omnes scientiae mathesi indigent, mathesis nulla, sed sola sibi sufficit* (1837–1839, vol. 4, p. 249).[8]

1.2 Józef Maria Hoene-Wroński

Let us proceed to the second figure discussed in this chapter, namely Józef Maria Hoene-Wroński. His philosophy of mathematics should be examined in relation to all his philosophical reflections. It belongs to the Messianic philosophy. He was in some sense a forerunner of the Polish Messianists. His philosophy originated mainly under the influence of such German thinkers as Kant, Schelling and Hegel.

One of the common characteristics of the Polish Messianists (including Bronisław Trentowski, Józef Gołuchowski, August Count Cieszkowski, Karol Libelt, Józef Kremer) was their interest in metaphysics, in which they focused on spiritualism and not on idealism, the latter being realised in German idealism. Their metaphysics stressed the conviction of the existence of the personalistic God, the eternity of soul and the absolute predominance of spiritual powers over the corporal ones. They wanted philosophy to reach cognitive and reformative aims and, indeed, even the soteriological aim since philosophy is not only called to cognise the truth but also to reform life: to save mankind. They believed in the metaphysical significance of a nation, ascribing a special significance and task to the Polish nation—it was to be a Messiah.

We are not going to discuss the details of Wroński's philosophical system.[9] We must only stress that his whole mathematics grew out of his philosophical concepts and moreover, it was subordinate to them. His main purpose was to reform mathematics by working out certain fundamental principles and laws, in particular the so-called law of creation.

Wroński presented his basic concepts in the book entitled *Introduction à philosophie des mathématiques et technie d'algorithmie* (1811)—the Polish

lubiących walczyć z trudnościami. Co także pokazuje niepospolitą i wyższego rzędu skłonność człowieka do zawiłych zaiste, ale trwałych i wyniosłych rozkoszy umysłowych, wzmacniających naturę ludzką.

Matematyka, która tyle zrobiła przysług towarzystwu, naukom i sztukom, stanie się jeszcze wodzem ludzkiego umysłu we wszystkich poznawaniach.'

[8] 'Wzrost matematyki jest wielki i nigdy się nie kończący. Jest ona tylko sama prawdziwą umiejętnością, bo samowładnie panuje nad całą krainą poznawań ludzkich. Jej bowiem wszystkie prawie nauki potrzebują, a ona żadnej, jak to dobrze powiedział Jan Bernoulli: *Omnes scientiae mathesi indigent, mathesis nulla, sed sola sibi sufficit.*'

[9] We write about it in 'System filozoficzny Hoene-Wrońskiego' (2008). See also Murawski (2006).

translation *Wstęp do filozofii matematyki oraz technia algorytmii* was published in the year 1937. He distinguished two branches in mathematics: algorithmie and geometry. Algorithmie is divided into the science of the laws of numbers (algebra) and science of the facts of numbers (arithmetic). The laws of lengthiness are subject to general geometry, the facts of lengthiness—detailed geometry. The task of the theory of algorithmie is to define the nature of all 'elementary algorithms' and their mutual influences and relations, the so-called 'systematic algorithms.'

His plans to reform the whole of mathematics in the spirit of the philosophy he promoted were presented in his work *A Course of Mathematics* (1821)—the Polish translation *Wstęp do wykładu matematyki* appeared in the year 1880.[10] This little work was to popularise his plans to large audiences. According to Wroński any positive knowledge is based on mathematics or at least uses mathematics. Hoene-Wroński distinguished four periods in the development of this science. The first one is the period when mathematics was exercised *in concreto*, i.e. there was no abstraction from the material reality and mathematics had only a practical character, like in ancient Egypt and Babylon. The second period is the time of Greek mathematics. It is characterised by the use of abstraction. According to Wroński mathematical truths 'constituted only particular facts [cases] and have not reached the characteristic of general truths.' The third period embraced the times from Cardano and Fermat until Kepler and Wallis. Then some general truths did appear but 'the harvest obtained in this new period, though very general, constituted only separate truths, i.e. in a way *individual* [samosobny] mathematical *products* [uzbiory].' For instance, the formulas for solutions of equations of degree 3 and 4 were found, but 'there was no idea about the universal setting of these solutions or even about the thing that is today called their development in a series.' The fourth period that Hoene-Wroński distinguished began with Newton and Leibniz. Then the methods allowing the use of mathematics in 'all appearances of nature' were invented. The period was characterised by the usage of series, which—according to the scientist—were the only hitherto common tool.

Relative principles were the basis in all of the described periods. However, mathematics should be based on absolute principles. Hence the prediction of a new period of the development of mathematics was made. Wroński's reform was to be its basis. It should consist in the division of mathematics into theory and technie [technia]. All mathematical truths should be deduced from the only highest law thanks to which they should gain the absolute certainty. Hoene-Wroński stressed the importance of this fact, writing:

[10] At this point, it is interesting to add that the Polish translation includes the fashionable tendency of that time to replace all—even those sanctified by tradition—scientific foreign terms by Polish ones that were coined artificially. For example, instead of 'logika' the term 'słoworząd' was used; instead of 'teologia'—'bożoznawstwo'; 'psychologia'—'duszoumnia'; 'ontologia'—'jistwoznawstwo'; 'geometria'—'ziemiomiernictwo'; 'mechanika'—'rozsilnia'; 'statyka'—'równoważnia'; 'dynamika—'siłorządnia,' etc.

> We cannot evaluate better this most elevated function of Mathematics only by confessing that its absolute characteristic, *predictability*, is a kind of divine manifestation. And in this sense, this benefit, this creative gift, is right next to God's revelation of religious truths.[11]

Hoene-Wroński's ideas concerning mathematics did not arouse wide-ranging interests, the reason being their ambiguity and imprecise, factually obscure language. That is why—although Wroński was an outstanding man and erudite speaking several languages—his works were not studied by mathematicians and philosophers of mathematics.[12] Yet, his ideas awoke the interests of occultists—though Wroński himself wanted nothing to do with them—and the interests of those who did not deal with philosophy in a professional way.[13] Wroński did not gather a group of followers and students, thus he was alone all his life.

1.3 Henryk Struve

Henryk Struve was a philosopher living at the turn of the nineteenth and the twentieth centuries. He is regarded as one of the most important figures of Polish logic in the nineteenth century (cf. for example Woleński 2008, p. 30)—yet he has been forgotten. In the interwar period he was frequently referred to but his works were not analysed and reprinted.[14] He was a professor at the Main School [Szkoła Główna] and at the Russian Imperial University of Warsaw. He taught logic. He was the author of numerous textbooks on logic and wrote a history of logic (1911) as well.

We are first of all interested in Struve's views on logic as a science and his conception of logic, which is important because in a way, Struve stood on the threshold of the new way of understanding and cultivating logic, combining the old and new paradigms.[15] As Twardowski wrote about him:

> So Struve was as if a link connecting this new period with the previous one. Between the generations of the Cieszkowskis, the Gołuchowskis, the Kremers, the Libelts, the Trentowskis and the contemporary generation there appears the distinguished figure of

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[11] 'Nie możemy lepiej ocenić tej przewzniosłej funkcji Matematyki, jeno wyznawając, że absolutna jej cecha, *przewidoczność*, jest rodzajem boskiego objawu. I, w tym pojęciu, dobrodziejstwo takowe podarek twórczy staje tuż obok Boskiego objawienia prawd religijnych.'

[12] It is worth saying that Hoene-Wroński's ideas aroused the interests of Samuel Dickstein, a mathematician, historian of mathematics and educator. It was Dickstein that catalogued and described the collection of Hoene-Wroński's writings from the Kórnik Library. He made it in his book *Katalog dzieł i rękopisów Hoene-Wrońskiego* (1896b). He also wrote *Hoene-Wroński. Jego życie i prace* (1896a).

[13] See Murawski (1998, 2002, 2005).

[14] It is worth adding that the exception was Samuel Dickstein who published posthumously Struve's handwritten sketch dedicated to Hoene-Wroński.

[15] Cf. Trzcieniecka-Schneider, *Logika Henryka Struvego. U progu nowego paradygmatu* [The Logic of Henryk Struve. On the Threshold of a New paradigm] (2010).

> this thinker, writer, who saved from the past what was of lasting value, and he showed the workers of today's Polish philosophy the direction through his prudent, and devoid of all prejudices, opinion (1912, p. 101).[16]

Struve presented his views on logic in his fundamental work *Historya logiki jako teoryi poznania w Polsce* [History of Logic as the Theory of Knowledge in Poland] (1911) and in the textbook *Logika elementarna* [Elementary Logic] (1907) as well as in various papers. Certain difficulties in reconstructing his views result from the fact that he wanted to create a coherent system of philosophy that would embrace all the traditional branches of philosophy. It caused that the limits between particular branches were flexible and imprecise. The principles of one division influence the foundations of the other and conversely. He balanced between materialism and idealism aiming at the golden mean, which among other things was reflected in his understanding of the object of logic. At first he thought that the object of logic was principles and rules of thinking. In his talk given in 1863, inaugurating his lectures at the Main School, he said:

> Gentlemen! Logic is most generally the *science of rational thinking*, having thinking, its principles and rules as its object (1863, Lecture 1).[17]

However, he added that thinking is one of the powers of the soul; it is 'an objective, neutral consideration of this world by the soul' (1863, p. 55).[18] Thus he introduces a psychological element, and indirectly—an ontological one. Since the soul is the ideal embryo of the human being and 'the limits of our being are the limits of our correct thinking' (1863, p. 35).[19]

In Struve's opinion logic concerns objective reality; nonetheless, it does not concern it directly—the mediator between logic and the world is the thought. However, this does not lead to the thesis that the thought reflects the logical structure of the world or to the thesis that the world has some logical structure at all.

Struve's earlier views were even more inclined towards psychologism. Initially, he claimed that thinking is 'an objective, neutral consideration of this world by the soul.' He upholds this thesis in *Logika elementarna* (1907), but here he separates logic from psychology, writing that logic deals with thinking as 'an auxiliary mean to get to know the truth' whereas psychology is interested in emotional and volitional motives of cognition. Logic has both a descriptive and normative character and is to oversee the application of the established norms and thus to evaluate the degree of the truth of cognition.

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[16] 'Był tedy Henryk Struve jakby ogniwem, łączącym ten okres nowy z poprzednim. Między pokoleniem Cieszkowskich, Gołuchowskich, Kremerów, Libeltów, Trentowskich a pokoleniem współczesnem widnieje czcigodna postać myśliciela, nauczyciela, pisarza, który z przeszłości ocalił to, co miało w niej wartość trwałą, a dzisiejszym na polu filozofii polskiej pracownikom wskazał drogę rozważnym i dalekim od wszelkiego uprzedzenia sądem.'

[17] 'Panowie! Logika jest to w najogólniejszem pojęciu *nauka myślenia* mająca myślenie, jego zasady i prawidła za przedmiot.'

[18] 'obiektywne, neutralne rozpatrywanie tego świata przez duszę.'

[19] 'granice naszego bytu są granicami naszego myślenia prawidłowego.'

The foundation of logic is philosophy, but also conversely: philosophy can be developed only on the foundation of logical laws. The title of the main analysed work of Struve *Historya logiki jako teoryi poznania w Polsce* may suggest that he identified logic with the theory of knowledge. In the first editions of *Logika elementarna* in Russian[20] he made no clear distinction between these two disciplines, but in the Polish version of the textbook (1907) he wrote:

> While it is true that *thinking* is the main co-factor of cognitive activity but not the only one; it unites directly and constantly with the suitable expressions of *emotion* and *will* (1907, p. 3).[21]

The examination of emotions and will as well as their relationships with thinking belongs to the sphere of psychology whereas logic deals with thinking merely in one aspect, namely:

> As *an auxiliary mean of getting to know the truth* [. . .]. Simultaneously, logic is not satisfied with the real course of mental activity but seeks principles, i.e. laws and rules which one *should* follow as norms if one wants to get to know the truth as exactly as possible. This separate view on thinking gives logic the character of an independent science, which is strictly different from *psychology*, namely this part of logic that investigates *thinking* as well (1907, p. 5).[22]

In *Historya logiki* we find the following words:

> [. . .] many separate the theory and criticism of *cognition* from *logic* as science dealing only with *thinking*. Despite that, the connection between the development of correct thinking as well as arriving at and getting to know the truth is so close from the psychological perspective that these mental activities cannot be separated (1911, p. 1).[23]

One can see here some traces of his discussions conducted with Kazimierz Twardowski and the Lvov-Warsaw School (see Chap. 3). On the one hand, one can notice a certain readiness to recognise the new understanding of logic and on the other hand, a desire to abide by his current understanding of logic.

Speaking of getting to know the truth Struve differentiates between objective and subjective truth. The former is an ideal being that is independent of the human cognition and the latter is the reconstruction of the content of being, of what exists

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[20] 'Елементарная логика' [Elemientarnaja logika] was first published in 1874; there were altogether 14 Russian editions. It was the obligatory manual of logic in classical junior high school from the year 1874. Its Polish version appeared in 1907.

[21] '*Myślenie* jest wprawdzie głównym, ale nie jedynym współczynnikiem czynności poznawczej; jednoczy się ono zarazem bezpośrednio i stale z odpowiednimi objawami *uczucia* i *woli*.'

[22] 'Jako *środek pomocniczy poznania prawdy* [. . .]. Przytem nie zadowala się logika danym faktycznym przebiegiem czynności myślowej, lecz odszukuje zasady, t.j. prawa i prawidła, któremi w myśleniu jako normami kierować się *należy*, chcąc dojść do możliwie ścisłego poznania prawdy. Ten odrębny pogląd na myślenie nadaje logice charakter samodzielnej nauki, ściśle różniącej się od *psychologii*, a mianowicie tej części jej, która bada również *myślenie*.'

[23] '[. . .] wielu odróżnia zarówno teorią, jak i krytykę *poznania* od l*ogiki* jako nauki samego tylko *myślenia*. Pomimo to łączność pomiędzy rozwojem prawidłowego myślenia a dochodzeniem i poznaniem prawdy tak jest ścisła ze stanowiska psychologicznego, że tych czynności umysłowych rozerwać nie podobna.'

in reality, in the mind—done through correct thinking. Logic is to control this reconstruction and thus through the formal means it is to reach the real being. Thus thinking has a reconstructive and not creative character. In Struve's opinion there are three forms of logic: (1) formal, (2) metaphysical and (3) logic treated as the theory of knowledge. Formal logic considers the principles and laws of thinking regardless of its object. Metaphysical logic (developed by Plato, Neo-Platonists, Spinoza or the German idealists: Fichte, Schelling and Hegel) states that since thinking contains its object directly in itself we get to know the very objective reality knowing the principles and laws of thinking. Struve accepts neither the first nor the second conception of logic. He opts for the third solution treating it as the golden mean. Thus he understands logic as the method of investigation and cognition of truth. Its task is to discover the principles according to which man reconstructs the structure of the real world in his mind. Naturally, Struve sees the difficulties connected with this view. In *Logika elementarna* he wrote:

> The difficulties of examining the relation [. . .] between thinking and the objective world are obvious and can be reduced mainly to the fact that we are not able to compare directly our images and concepts of objects and our views on them with the objects themselves. The question concerning the objective knowledge of truth could be solved in favour of thinking only when it turned out that the laws of our mind, and thus thinking, were fundamentally consistent with the laws of the objective being which is independent of us. [. . .] Nonetheless, showing the accordance between the laws of the mind and the laws of the objective being requires a series of critical investigations concerning the results of scientific studies (1907, pp. 6–7).[24]

Struve calls this logic 'logic of ideal realism.' It is to constitute the framework of a coherent system of philosophy giving a general outlook on the world. We should add that Struve is far from ascribing Messianic tendencies to logic (cf. the concepts of Hoene-Wroński). He opts for a balance between the knower and the known, seeing the role of emotions and will in cognition. He was interested in Leibniz's view to which he many a time referred, the view that logic is abstracted from reality.

Struve begins his lecture on logic by giving images and concepts. Then he introduces judgements. By 'image' he means a kind of representation of the object through its characteristics, and 'concept' is a set of essential features. Moreover, images result from certain mental processes. It is the object that makes the mind create images.

Struve attached great importance to the teaching of logic. He thought that teaching how to think correctly is much more important that giving students

[24] 'Trudności zbadania stosunku [. . .] myślenia do świata przedmiotowego są oczywiste i sprowadzają się głównie do tego, że nie jesteśmy w stanie porównywać bezpośrednio naszych wyobrażeń i pojęć o przedmiotach ani poglądów na nie z samymi przedmiotami. Kwestya przedmiotowego poznania prawdy mogłaby być rozwiązaną na jego korzyść dopiero wtedy, gdyby się okazało, że prawa naszego umysłu, a więc i myślenia, są zasadniczo zgodne z prawami niezależnego od nas bytu przedmiotowego. [. . .] Wykazanie atoli tej zgodności praw umysłu z prawami bytu przedmiotowego wymaga szeregu badań krytycznych nad wynikami dociekań naukowych.'

concrete contents. Consequently, he placed a strong emphasis on the teaching of logical culture.[25]

We have already shown that Struve's conception of logic places him between the old and new paradigm or rather even in the old paradigm. We have mentioned that he did not value the role and significance of symbolic and mathematised formal logic but he stressed psychological questions. Consequently, one should ask what made him not see the advantages of the new attitude. It seems that one of the reasons was the fact that Struve saw no cognitive value in pure form devoid of content (cf. Trzcieniecka-Schneider 2010). According to his conception it is the object, i.e. external world that stimulates our thinking that realises its existence and the characteristics of objects, and then using logical methods it creates notions which in turn it uses, applying logical methods, to formulate judgements. Thus there can be no cognition without content. Another reason may be that he set a low valuation on the role and importance of mathematics. Trzcieniecka-Schneider even claims that Struve 'did not understand mathematics, reducing it only to the techniques of operations on numbers' (2010, p. 93). In 'Filozofia i wykształcenie filozoficzne' [Philosophy and Philosophical Education] he wrote:

> Mathematics considers only quantitative factors: [. . .] But in its activities the human mind is not limited only to quantitative factors but everywhere supplements quantity with quality. [. . .] One cannot say that truth contains more or fewer thoughts than falsity: [. . .] These are all qualitative differences and they cannot be defined quantitatively; they cannot be understood rightly and characterised closely from the mathematical point of view but they can only be understood from a more general standpoint, going much beyond the scope of the quantitative factors alone (1885, pp. 156–157).[26]

Struve also opposed the introduction of quantifiers. He permitted quantitative elements in logic only in the case of the conversion of judgements and in the logical square. However, he was not consistent with his views since analysing the relations between the scopes of concepts using Euler diagrams he actually used the arithmetic notation.

In his opinion formal logic 'leads [. . .] only to the development of one-sided formalism without elevating the essential cognitive value of the relevant forms' (1907, p. IX) and 'mathematical logic depends on the completely dogmatic transfer of the quantitative and formal principles to the mental area where quality and

[25] The further activities of Kotarbiński and Ajdukiewicz (cf. Chap. 3) promoting logical culture fit with this tendency in an excellent way and can be treated as the continuation of Struve's activities.

[26] 'Matematyka rozpatruje wyłącznie czynniki ilościowe: [. . .] Tymczasem umysł ludzki w działalności swej nie jest bynajmniej ograniczonym samemi tylko pojęciami ilościowymi, lecz uzupełnia wszędzie ilość jakością. [. . .] Nie można powiedzieć, że prawda zawiera w sobie więcej lub mniej myśli niż fałsz: [. . .] To wszystko są różnice jakościowe, których ilościowo określić nie można, które należycie zrozumiane i bliżej scharakteryzowane być nie mogą z punktu widzenia matematycznego, lecz pojęte być mogą tylko ze stanowiska ogólniejszego, wynoszącego się wysoko ponad zakres samych tylko czynników ilościowych.'

content are of primary importance (*ibid*.).[27] He also claims that ‘reducing judgement to *equation* and basing conclusion on *substance*, i.e. *substituting equivalents*, does not correspond to the real variety of judgements and conclusions’[28] (1907, p. X).

Struve’s aversion towards mathematics and mathematical methods in logic was connected with his views on the function of language in logic and cognition as well as with his conception of truth. Since if—in accordance with Aristotle—truth is the conformity of thought to the content of proposition, conducting operations on propositions as symbols means losing sight of this property to some extent. In fact, the characteristic of being true does not refer to propositions but to their contents. Symbolically identical propositions can differ with respect to their contents. Thus the operations conducted on the symbols of propositions do not have much in common with establishing the truth. And yet logic is to lead to the truth about the real world.

1.4 Władysław Biegański

By profession Władysław Biegański was a general practitioner, held a medical doctorate and had his own medical practice. He worked in the hospital and was physician for a factory and the railways. His scientific interests included many medical disciplines as well as the philosophy of medicine, and in particular the methodological and ethical issues connected with it. Biegański represented the Polish school of the philosophy of medicine. However, his true passion was logic. As a student he listened to Henryk Struve’s lectures (cf. the previous section). Besides his medical practice he taught logic in local secondary schools for some time. In 1914 there was even an initiative to appoint Biegański as the professor of the Jagiellonian University Chair of Logic.[29] The idea was not fulfilled because of his bad health condition and the outbreak of the First World War.

Before discussing Biegański’s philosophical views on logic we should mention his works on epistemology and the methodology of medicine. As far as the theory of cognition is concerned Biegański promoted a view which he called ‘previsionism’ (from the Latin *praevidere*—predict, foresee). Thus he referred to A. Comte’s motto ‘savoir c’est prévoir’ (to know is to foresee). In Biegański’s opinion the main task of science is to foresee phenomena (cf. Biegański 1910a, 1915). He understood the concept of prevision in a broad sense. He claimed that cognition

[27] ‘doprowadza [...] tylko do rozwoju formalizmu jednostronnego bez podniesienia istotnej wartości poznawczej odnośnych form’; ‘logika matematyczna polega na zupełnie dogmatycznym przeniesieniu zasad ilościowych i formalnych na pole umysłowe, gdzie jakość i treść mają znaczenie pierwszorzędne.’

[28] ‘sprowadzanie sądu do *równania* oraz oparcie wniosku na *substytucyi*, czyli *podstawianiu równoważników*, nie odpowiada rzeczywistej rozmaitości ani sądów, ani wniosków.’

[29] Cf. Borzym (1998), p. 13.

consists in the prevision of events, their causes and effects as well as properties. The aim of cognition is not to reproduce reality—thus he opposed the so-called reproductionism. At the same time he stressed the importance of the teleological point of view, which in his opinion brought better effects than causal explanation in many disciplines of science, especially in medicine and biology. Laws formulated in science are not identical with reality—as he wrote 'there is no identity of laws with the real order of phenomena' (1915). However, in our constructions there are always 'real elements.' The aim of cognition is not to reproduce reality but to obtain proper orientation in the environment.

In the field of the methodology of medicine Biegański formulated the first systematic general theory of diagnosis. He dealt with scientific observation, worked on the theory of experiment and was interested in the theory of induction. Of importance are his considerations concerning analogy. He presented them mainly in his work *Wnioskowanie z analogii* [Deduction by Analogy] (1909). He understood analogy in the traditional way as reasoning from details to details. However, he did not agree to reduce analogy to deduction or induction. In his opinion the foundations of analogy always include the suitability of some relations. Analogy is both something different from identity, which is the conformity of all features, and similarity in which we are to deal with the consistency of only some features.

Let us proceed to discuss Biegański's philosophical views on logic. Firstly, we should notice that he was neither a formal nor mathematical logician, but—as Woleński mentions (1998)—he was a philosophical logician in the standpoint formulated by Łukasiewicz. The latter characterised philosophical logic in the following way:

> If we use here the term 'philosophical logic' we mean the complex of problems included in books written by philosophers, and the logic we were taught in secondary school. Philosophical logic is not a homogenous science; it contains various issues; in particular, it enters the field of psychology when it speaks not only about a proposition in a logical sense but also this psychological phenomenon, which corresponds with a proposition and which is called 'judgement' or 'conviction.' [...] Philosophical logic also embraces some issues from the theory of knowledge, for example, the problem of what truth is or whether any criterion of truth exists (1929a, pp. 12–13).[30]

At this point it is immediately worth adding that Łukasiewicz himself did not value philosophical logic, thinking that the scope of problems it considers is not homogenous, and philosophical logic mixes logic with psychology. Moreover, both fields are different and use different research methods (cf. Chap. 3, Sect. 3.2).

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[30] 'Jeżeli używamy terminu logika filozoficzna, to chodzi nam o ten kompleks zagadnień, które znajdują się w książkach pisanych przez filozofów, o tę logikę, której uczyliśmy się w szkole średniej. Logika filozoficzna nie jest jednolitą nauką, zawiera w sobie zagadnienia rozmaitej treści; w szczególności wkracza w dziedzinę psychologii, gdy mówi nie tylko o zdaniu w sensie logicznym, ale także o tym zjawisku psychicznym, które odpowiada zdaniu, a które nazywa się "sądem" albo "przekonaniem". [...] W logice filozoficznej zawierają się również niektóre zagadnienia z teorii poznania, np. zagadnienie, co to jest prawda lub czy istnieje jakieś kryterium prawdy.'

How did Biegański understand logic? In *Zasady logiki ogólnej* [Principles of General Logic] he wrote:

> Logic is the science of the ways or norms of true cognition (1903, p. 1).[31]

In *Podręcznik logiki i metodologii ogólnej* [Manual of Logic and General Methodology] we find the following definition:

> We call logic the science of the norms and rules of true cognition (1907, p. 3).[32]

Therefore, the laws of logic concern the relationships of mental phenomena because of its aim, which is true cognition. Consequently, logic aims at investigating cognitive activities of the mind. At the same time Biegański claims that one must separate and distinguish between logic and the theory of knowledge on the one hand and psychology on the other since logic is a normative and applied science whereas both the theory of knowledge and psychology are theoretical. However, in practice Biegański—like other authors of those days—did not distinguish strictly between logical and genetic questions, investigating logical constructions both from the precisely logical and psychological points of view. Yet, it should be noted that in *Zasady* (1903) Biegański suggests that his conception of logic makes him reject the division into formal and material truth whereas in *Podręcznik* (1907) he regards this distinction as correct. He also adds that logic embraces the formal side of cognition.

In his large (638 pages) monograph entitled *Teoria logiki* [Theory of Logic] (1912a)—an attempt to consider the foundations of logic comprehensively [33]—Biegański writes:

> The main aim of logic is to control argumentation (1912a, p. 34).[34]

He continues:

> Logic, as the science and art of argumentation, is an *a priori* science, i.e. science that draws its content not from experience and not from the facts given in experience, but from certain *a priori* presumptions and constructions (1912a, p. 35).[35]

Thus logic appears as a normative science—accordingly, Biegański proposes to use the name 'pragmatic logic.' He does separate logic from psychology, ontology and epistemology. The basis of logic is axioms: 'the most general laws which are

[31] 'Logika jest to nauka o sposobach albo normach poznania prawdziwego.'

[32] 'Logiką nazywamy naukę o normach i prawidłach poznania prawdziwego.'

[33] This work presents general problems concerning logic, the study of concepts, the study of judgments, the study of argumentation and the study of induction. Every problem is considered in historical and comparative perspectives on the one hand and a systematic perspective on the other hand. Although Biegański focuses on the views of the representatives of traditional logic, he also analyses the algebra of logic.

[34] 'Logika ma na celu głównie kontrolę dowodzenia.'

[35] 'Logika, jako nauka i sztuka dowodzenia jest nauką aprioryczną, tj. taką, która swoją treść czerpie nie z doświadczenia, nie z faktów w doświadczeniu nam danych, lecz z pewnych naprzód powziętych założeń i konstrukcji.'

directly obvious, i.e. requiring no proof' (1912a, p. 41). The axioms are the laws of identity, contradiction, excluded middle and sufficient reason. It should be added that Biegański distinguished between the context of discovery and the context of justification.

This understanding of logic as the art of argumentation is found in his earlier treatise 'Czem jest logika? [What is Logic?]' (1910b) where he wrote:

> [. . .] logic does not reproduce the processes of thought and it does not aim at doing it at all. Therefore, the definition of logic as the science or art of thinking is actually devoid of any basis. [. . .] But the origin of logic shows that this ability [i.e. logic—remark is mine] is neither a science nor art of thinking, but was created by Plato and Aristotle as the art of argument. Such differences in views cause serious consequences. If logic is a science or even an art of thinking, it is or should be a branch of psychology; on the contrary, if it is only the art of argument, it becomes a separate science that is independent from psychology. Logic as the art of argument does not describe the ordinary course of thoughts, used in argumentation; it does not reproduce it; it does not find laws for it, laws expressing the mutual causal relationship of thoughts, but uses ideal constructions which serve to control the ways of argumentation and in this respect it is explicitly separated from psychology (1910b, p. 144).[36]

Consequently, logic 'must be of normative character' (1910b, p. 145), which Biegański explains:

> The essence of argumentation consists in valuing. Looking for a proof of any proposition we always follow the question about its cognitive value (1910b, p. 145).[37]

What is meant here is not the meaning of the proposition and its content but its veracity. 'Every proof consists in stating the consistency between the content of the proposition and the principles, which we recognise as true, and it is in this consistency that the essence of truth lies' (1910b, p. 145).[38]

What is then the relation between logic and psychology? Biegański stresses their autonomy:

> Any direct [. . .] dependence here is out of the question. Nonetheless, psychological investigations are not completely meaningless to logic since they constitute an important

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[36] '[. . .] logika nie odtwarza procesów myśli i nie ma wcale na celu tego zadania. To też określenie logiki jako nauki lub sztuki myślenia jest pozbawione właściwie wszelkiej podstawy. [. . .] Tymczasem geneza logiki wykazuje, że umiejętność ta [tzn. logika—uwaga moja R.M.] nie jest ani nauką, ani sztuką myślenia, lecz utworzona została przez Platona i Arystotelesa jako sztuka dowodzenia. Takie różnice w zapatrywaniach prowadzą za sobą poważne konsekwencje. Jeżeli logika jest nauką lub nawet sztuką myślenia, to w każdym razie jest lub powinna być działem psychologii, przeciwnie, jeżeli jest tylko sztuką dowodzenia, to staje się nauką odrębną, niezależną od psychologii. Logika jako sztuka dowodzenia nie opisuje zwykłego biegu myśli, stosowanego przy dowodzeniu, nie odtwarza go, nie wynajduje dla niego praw, wyrażających wzajemny związek przyczynowy myśli, lecz posługuje się konstrukcyami idealnymi, które służą dla kontroli sposobów dowodzenia i pod tym względem odgranicza się wyraźnie od psychologii.'

[37] 'Istota dowodzenia polega na wartościowaniu. Poszukując dowodu dla jakiegokolwiek zdania, kierujemy się zawsze pytaniem o jego wartości poznawczej.'

[38] 'Każdy dowód polega na stwierdzeniu zgodności treści zdania z zasadami, które uznajemy za prawdziwe i w tej właśnie zgodności tkwi istota prawdy.'

> control for logical constructions [. . .]. An ideal logical construction would be one that is the closest to the real course of thoughts, that completely guarantees to distinguish truth and is easy to apply. [. . .] Thus psychological investigations are undoubtedly of great importance for the development of logic because they can contribute to formulating new constructions which are the closest to the natural course of thoughts (1910b, pp. 147–148).[39]

What did Biegański mean by argumentation? In fact, he did not give any clear idea of inference. He neither used the concept of logical deduction nor distinguished between deductive and inductive reasoning. He says that inference is based on the idea of necessity, that the principles of logic refer to the form and not the content of cognition, but these ideas are not fully clear and additionally, they are mixed. Biegański's misconception concerning deduction and its role is confirmed, for example by the fact that in his work 'Sposobność logiczna w świetle algebry logiki' [Logical Modality in the Light of the Algebra of Logic] (1912b) he speaks about reliable and possible deduction, which is a misunderstanding.

Finally, discussing Biegański's conception of logic we should add that his departure from psychologism was not definitive since in *Podręcznik logiki ogólnej* [Manual of General Logic] (1916) one can see his return to psychologism. He writes:

> We call logic the science about the ways of controlling the truth of our cognitive thoughts (1916, p. 1).[40]

The presented analyses show that Biegański should be rather included among the traditional 'pre-mathematical' approaches to logic. Although the first signs of interest in mathematical logic appeared in Poland in the 1880s (suffice it to mention the treatise of Stanisław Piątkiewicz *Algebra w logice* [Algebra in Logic] published in 1888), the logical culture was decisively 'pre-mathematical' in Poland at the turn of the nineteenth and the twentieth centuries. This opinion is supported, for example, by the first edition of *Poradnik dla samouków* [A Guide for Autodidacts] (1902), which includes Adam Marburg's paper 'Logika i teoria poznania' [Logic and the Theory of Knowledge] written in a rather old-fashioned manner. Struve's and Biegański's views on logic were shared by their contemporaries: Władysław Kozłowski (1832−1899) and Władysław Mieczysław Kozłowski (1858−1935). The former wrote in *Logika elementarna* [Elementary Logic] that 'logic is the science about mental activities with the aid of which we reach truth and prove it' (1891, p. 1).[41] In turn Władysław M. Kozłowski wrote in *Podstawy logiki*

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[39] 'O bezpośredniej [. . .] zależności nie może tu być mowy. Pomimo to badania psychologiczne nie są zupełnie bez znaczenia dla logiki, stanowią bowiem bardzo ważną kontrolę dla konstrukcji logicznych. [. . .] Ideałem konstrukcyi logicznej byłaby taka, któraby się najbardziej zbliżała do rzeczywistego biegu myśli, dawała zupełną gwarancyę w odróżnianiu prawdy i była łatwa do stosowania. [. . .] To też badania psychologiczne mają niewątpliwie duże znaczenie w rozwoju logiki, gdyż mogą się przyczynić do wynalezienia konstrukcyi nowych, najbardziej zbliżonych do naturalnego biegu myśli.'

[40] 'Logiką nazywamy naukę o sposobach kontrolowania prawdy naszych myśli poznawczych.'

[41] 'Logika jest nauką o czynnościach umysłowych, za pomocą których dochodzimy prawdy i jej dowodzimy.'

[The Foundations of Logic] that 'Logic is the science about the activities of the mind which seeks truth' (1916, p. 8).[42] He calls the first chapter of his work 'Thinking as object of logic' (1916, p. 22). He repeats this thought in *Krótki zarys logiki* [A Brief Outline of Logic], claiming that logic is a normative science and its task is 'to examine the ways leading the mind to truth' (1918, p. 1). However, he stresses that logic:

> analyses mental operations conducted to reach the truth in a form that is so general that could be apply to any content. It investigates its form, separating it completely from the content. Logic shares this property with mathematics [. . .]. [. . .] This formal character, common to logic and mathematics, made these sciences close in their attempts, which were less or more developed, and led to the creation of mathematical logic (1918, pp. 8–9).[43]

Finally, he states that logic can be defined 'as the science about the forms of every ordered field of real or imaginary objects' (1918, p. 9).[44]

However, let us come back to Biegański. His conceptions concerning the foundations and philosophy of logic did not evoke much interest but rather criticism. In *Ruch Filozoficzny* Łukasiewicz. published a review of Biegański's work 'Czem jest logika?' (1910b), stressing his departure from psychologism but noticing that it was not completely consistent. He also emphasised the fact that Biegański's conception of logic was too narrow. Since he limits it to inference while in Łukasiewicz's opinion the object of logic should be reasoning in general, which should include non-deductive reasoning (we will present it more widely in the chapter dedicated to Łukasiewicz.).

Being stuck in the traditional paradigm of logic Biegański could, however, see the advantages of the new approach, in particular the values and advantages of the algebra of logic. In the introduction to his work 'Sposobność logiczna w świetle algebry logiki' (1912b), in which he attempted to (admittedly, with a miserable result) apply the algebra of logic to the theory of modal categories, he wrote:

> Although logical calculus, called the algebra of logic or logistics, has not and cannot have a large practical application, considering the logical evaluation of our judgements and conclusions, it has undoubtedly important theoretical significance. [. . .] Yet, algebraic symbols, which we use in logical calculus, separate clearly the object of investigation from psychological factors and objective relations, and bring to light all the properties of pure logical relations. Therefore, the main value of the algebra of logic consists in the fact that using it we can explain more thoroughly and mark strictly the relations that are explained variously in school logic (1912b, p. 67).[45]

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[42] 'Logika jest nauka o czynnościach umysłu poszukującego prawdy.'

[43] 'bada operacye umysłowe, wykonywane w celu osiągnięcia prawdy w formie tak ogólnej, iżby mogły zastosować się do jakiejkolwiekbądź treści. Bada je ze stanowiska ich formy, odrywając się zupełnie od treści. Własność tę podziela z logiką matematyka [. . .]. [. . .] Ten formalny charakter, wspólny logice z matematyką, spowodował zbliżenie do siebie obu nauk w próbach mniej lub dalej posuniętych i znalazł wyraz w utworzeniu logiki matematycznej.'

[44] 'jako naukę o formach każdej uporządkowanej dziedziny przedmiotów rzeczywistych lub urojonych.'

[45] 'Rachunek logiczny, zwany algebrą logiki lub inaczej jeszcze logistyką, jakkolwiek nie ma i nie może mieć rozległego zastosowania praktycznego przy ocenie wartości logicznej naszych sądów i

1.5 Samuel Dickstein

Before discussing Samuel Dickstein's views concerning the philosophy of mathematics let us focus on his great activities in the field of the organisation of scientific life and his publishing efforts. In the year 1884, together with Aleksander Czajewicz, he founded 'Biblioteka Matematyczno-Fizyczna' [Mathematical-Physical Library] and in the year 1888, together with Edward and Władysław Natanson as well as Władysław Gosiewski, he began editing *Prace Matematyczno-Fizyczne* [Mathematical-Physical Works]. It was the first periodical dedicated entirely to mathematics and physics in Poland. In the year 1897, he initiated *Wiadomości Matematyczne* [Mathematical News].[46] Both *Prace* and *Wiadomości* were financed from Dickstein's private fund from their beginnings until 1939. Moreover, one should mention his merits in translation efforts. He translated into Polish various classical works (published in *Prace Matematyczno-Fizyczne* and *Wiadomości Matematyczne*), and thus familiarised Polish readers with important scientific achievements. Consequently, he contributed to forming Polish mathematical terminology.[47] Undoubtedly, all his activities led to creating conditions for the development of a Polish school of mathematics. His scientific interests focused on algebra and the history of mathematics. The latter led him to write the monograph *Hoene-Wroński. Jego życie i prace* [Hoene-Wroński. His Life and Work] (1896a; cf. also 1896b) and publish the correspondence between Adam Kochański and Gottfried Wilhelm Leibniz in *Prace Matematyczno-Fizyczne*

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wniosków, posiada jednak niewątpliwie ważne teoretyczne znaczenie. [. . .] Tymczasem symbole algebraiczne, jakimi się w rachunku logicznym posługujemy, odrywają wyraźnie przedmiot badania zarówno od czynników psychicznych jako też od stosunków objektywnych i wydobywają na jaw wszystkie właściwości czystych stosunków logicznych. To też główna wartość algebry logiki polega na tem, że przy jej pośrednictwie możemy dokładniej wyjaśnić i ściślej wyznaczyć stosunki, które w logice szkolnej rozmaicie bywają tłomaczone.'

[46] Their continuation is *Roczniki Polskiego Towarzystwa Matematycznego*. Seria II: *Wiadomości Matematyczne*, which have been published until today.

[47] At this point, it is worth mentioning the translations from foreign languages, initiated by Dickstein as well as the mathematical and philosophical environments (e.g. within the framework of *Biblioteka Przeglądu Filozoficznego*) and many others (e.g. the physicist Ludwik Silberstein was extremely active as the editor of *Biblioteka Naukowa Wendego* and as a translator). His Polish translations include Bernhard Riemann's famous work about the fundamental hypotheses of geometry (1877), Felix Klein's *Odczyty o matematyce* (1899), Hermann Helmholtz's *O liczeniu w matematyce z punktu widzenia teorii poznania* (1908), Henri Poincaré's three books *Nauka i hipoteza* (1908), *Wartość nauki* (1908) and *Nauka i metoda* (1911), Richard Dedekind's *Ciągłość i liczby niewymierne* (1914), Federigo Enriques' edition of the collection of works *Zagadnienia dotyczące geometrii elementarnej* (1914), Mario Pieri's *Geometria elementarna oparta na pojęciu kuli i punktu* (1915), Alfred North Whitehead's *Wstęp do matematyki* (without any date, but certainly before the year 1918) and finally, John Wesley Young's *Dwanaście wykładów o podstawowych pojęciach algebry i geometrii* (bearing no date but not later than 1918). The publication of these translations points to the growth of the interests of Polish mathematicians, logicians and philosophers in the problems of the foundations of mathematics. It is also a wonderful testimony and a sign of positivistic work in social education.

(vol. XII in the year 1901 and vol. XIII in 1902). Moreover, he wrote numerous textbooks.

Dickstein's works include two publications that directly concern the subject of this book—the philosophy of mathematics. These are the monograph *Pojęcia i metody matematyki* [Concepts and Methods of Mathematics], vol. I, part 1: *Teoria działań* [Theory of Operations] (1891) and the paper 'Matematyka i rzeczywistość' [Mathematics and Reality] (1893).

The first work presents the methodology of mathematics or—using modern terminology—mathematical foundations of mathematics. The author discusses the terms and methods of mathematics. At the same time—which is worth noticing—he stresses the role of formalism in mathematics. Dickstein was in some sense a forerunner of the mathematical foundations of mathematics, i.e. using mathematical methods to examine mathematics. Furthermore, *Pojęcia i metody matematyki* deserves our attention because it includes the first Polish references to the works of Bolzano, Cantor, Dedekind, Frege and Peano. Dickstein quotes and reviews the ideas and theories of many authors, both mathematicians and philosophers, in particular Grassmann, Hankel, Helmholtz, Riemann, Weierstrass and Wundt. He also quotes Hoene-Wroński whose philosophy of mathematics he was going to develop.[48] Additionally, he refers to Stanisław Piątkiewicz's dissertation *Algebra w logice* [Algebra in Logic] (1888), which must have been the first Polish work on mathematical logic.[49] Dickstein defines it as 'a short paper on the Algebra of logic' (1891, p. 39).

It is worth asking what the reception of Dickstein's work was. It could have been read because of its author who was respected and honoured by the Polish mathematical environment. On the other hand, it was very rarely referred to, which might have been influenced by the fact that Dickstein, like Piątkiewicz or Stamm (presented in Sect. 1.6 of this chapter), was not part of the academic environment of those days.[50] This situation did not allow him to exert any influence. Nevertheless, Dickstein's monograph is a clear sign of the growth of interest in the foundations of mathematics in Poland.

In the other work 'Matematyka i rzeczywistość' (1893) Dickstein deals with the fundamental problem of the philosophy of mathematics, which is interesting both from the ontological and epistemological points of view, namely the question of the relation between mathematical objects and the empirical reality, especially the physical reality. Dickstein states that mathematical objects are reflections of reality in the mind. However, these are not passive reflections since in the process of the

[48] Cf. Dickstein's monograph *Hoene-Wroński. Jego życie i prace*, published in 1896.

[49] Although this work was noticed, it did not arouse much interest. Concerning Piątkiewicz, his dissertation and its meaning see Batóg (1971, 1973) as well as Batóg and Murawski (1996). Cf. Woleński (1995a) who quotes a fragment of the speech delivered by Kazimierz Twardowski, welcoming Heinrich Scholz in Lvov in 1932. In his speech Twardowski mentioned Piątkiewicz's work, calling it 'the first Polish work dedicated to logistics, i.e. algebraic or mathematical logic, as it was called those days' (Woleński 1995a, p. 195).

[50] Notice that Dickstein became professor of Warsaw University only in 1915.

creation of mathematical objects the mind is active and the creative imagination works. There is a mutual interaction as well as collaboration between the mind and the external world. In creating new objects (‘forms’—according to Dickstein) mathematicians go beyond reality, an example being the extension of the term of number to embrace negative, imaginary, irrational, infinitesimal, infinitely large numbers. But as he writes in ‘Matematyka i rzeczywistość’ one should remember that:

> Among other things progress in mathematics consists in the fact that mathematics bears the impossibilities it encounters on the way of development (if they are not logical or absolute impossibilities) by going beyond the field of research, in a way extending the horizon, creating a new world of forms which embrace the primary world. Such investigations of more general forms inspire the mind to consider new interesting issues, usually abounding in consequences (1893, p. 6).[51]

It is geometry that throws much light on the attitude of mathematics towards reality. Dickstein explicitly distinguishes geometry as a formal science and the application of geometry to describe experiments. In the first case it is senseless to ask questions concerning the truth of assumptions (axioms). Such questions become meaningful only when we want to apply geometrical theorems to describe the world. Then we ask ‘whether and to what extent reality is ideally reflected in axioms; whether the application of theorems does not lead to experimental discordance’ (1893, p. 13). However, this problem belongs to philosophy, more precisely to the theory of cognition and not to mathematics as such. For example, mathematicians can never claim that Euclidean geometry reflects reality in a perfect way. They can only say that ‘Euclidean geometry is good enough to describe reality within the limits of experiment’ (1893, p. 13). Nevertheless, it does not close off further search to them. They can still look for other systems that will allow them to define experimental data; *a fortiori*, they can look for systems that are interesting from the formal point of view. It already happened in the history of geometry when the systems of non-Euclidean geometry appeared.

Dickstein stresses firmly that the assumptions and axioms of geometry are not experimental truths since ‘they refer to ideal forms as elements of considered spaces’ (1893, p. 17). Mathematics cannot solve Kant’s problem concerning space as an *a priori* and necessary form of all sensible intuitions. In fact, mathematics does not need any solution of this question.

What then directs the development of mathematical theories, and in particular, what causes the creation of new systems and introduction of new mathematical objects? Dickstein sees the sources of this development in the principle of generalising and extending mathematical forms. He calls it (following Peacock and Hankel) the principle of permanence of equivalent forms or formal laws.

[51] ‘Postęp matematyki polega właśnie między innymi i na tym, że niemożliwości, jakie napotyka na drodze rozwoju (jeżeli nie są niemożliwościami logicznymi lub bezwzględnymi) znosi przez to, że przekracza dziedzinę badania, że rozszerza niejako widnokrąg, stwarzając nowy świat form, obejmujący w sobie świat pierwotny. Badanie takich form ogólniejszych nasuwa umysłowi nowe interesujące zagadnienia, zazwyczaj płodne w następstwa.’

The principle consists in that when we extend the concept of number we do it in such a way that new objects and activities embrace the previous objects and activities as special cases. This principle can be seen in the whole development of mathematics, '[. . .] under the leadership of the principle of permanence the development of mathematical knowledge takes place; since it leads to generalisations, which are a superior characteristic of this knowledge' (1893, p. 31).[52] However, the very formulation of this principle is not sufficient to make discoveries! The principle only allows describing the development of science—in no way can it replace creativity and be sufficient to give strict reasons for mathematical truths. Besides this principle we need another regulating principle, i.e. demand that the generalisations of concepts and activities do not lead to logical contradiction between them or towards the already accepted theorems.

Mathematics plays a very important role in natural science research, in exploring reality. Yet, it is only the role of a tool. Besides mathematics experiment and observation are needed. Mathematics is allowed to be characterised by certain universality; 'with its relations it can embrace various possibilities, the special case of which is reality' (1893, p. 34).

The fact that mathematics is good enough to describe reality can lead to—as Dickstein writes—mathematical mysticism when the objects of mathematics are regarded as reality itself. The Pythagoreans, the Neoplatonists, astrologists and others fell into this trap. It is:

> a departure from the principles of using and applying mathematics. A true scholar does not take directly mathematical forms for reality itself since he is aware of the way through which these forms originated, and he understands under what conditions he is allowed to return from the results of speculations to the real world. Then mathematical mysticism results from the misunderstanding of the origin of mathematical concepts and conditions of their applicability to examine nature (cf. 1893, p. 35).[53]

Mathematics does not solve any metaphysical questions. It only gives tools to examine phenomena. Therefore, one must clearly separate mathematics from philosophy. One cannot introduce metaphysics to mathematics and one cannot draw metaphysical conclusions from mathematical theorems (for example, conclusions about the infinity of the universe). On the other hand, the significance of philosophical investigations of mathematics should be appreciated.

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[52] 'pod przewodnictwem zasady zachowania odbywa się rozwój wiedzy matematycznej; ponato bowiem prowadzi do uogólnień, stanowiących wybitną cechę tej wiedzy.'

[53] 'zboczeniem od zasad stosowania matematyki. Badacz prawdziwy nie bierze wprost form matematycznych za samą rzeczywistość, bo świadomy jest drogi, na jakiej pojęcia tych form powstały, i rozumie, pod jakimi warunkami wolno mu od wyników spekulacji powrócić do świata rzeczywistego. Mistycyzm matematyczny jest wtedy wynikiem niezrozumienia genezy pojęć matematycznych i warunków ich stosowalności do badania przyrody.'

1.6 Edward Stamm

Although Stamm was cut off from the main scientific centres and was involved in didactic activities, he conducted research on logic, philosophy, mathematics, the history of science and ethics. One should mention first of all his publications concerning the algebra of logic (cf. 1911c, 1912, 1927–1928). He was fascinated with the new field and promoted it in the Polish scientific environment, especially among mathematicians. He was also a pioneer of its application to the theory of codes. He referred to it many a time while reflecting on the philosophy of mathematics. He discussed its advantages and significance for mathematics (cf. below). From 1927 he focused on the history of science and technology. In 1935 he wrote his main work *Historia matematyki XVII wieku w Polsce* [History of Mathematics in Poland in the 17th Century].[54] Stamm collaborated with Samuel Dickstein who followed his scientific development and supported him (most of Stamm's works were published in *Wiadomości Matematyczne*). He was also involved in numerous scientific societies in Poland and abroad, including the Polish Philosophical Society, the Polish Mathematical Society and Academia pro Interlingua. In fact, he enthusiastically supported the international language *latino sine flexione* created by Giuseppe Peano (in 1926, being invited by Peano he used this language to write the treatise *Praesente et futuro de Matematica*, which was published in the periodical *Academia pro Interlingua* in Turin).

The following papers written by Stamm can be included in the circle of issues that this book presents, i.e. the philosophy of mathematics and logic:

– *O aprjoryczności matematyki* [On the Apriority of Mathematics] (1909),
– *Czem jest i czem będzie Matematyka?* [What is Mathematics and What Will It Be?] (1910),
– *Logiczne podstawy nauk matematycznych* [Logical Foundations of Mathematical Sciences] (1911a),
– *O przedmiotach urojonych* [About Imaginary Objects] (1913a),
– '*Characteristica geometrica*' *Leibniza i jej znaczenie w Matematyce* [Leibniz's 'Characteristica geometrica' and Its Significance in Mathematics] (1913b).

In his youthful (written at the age of 23, before his graduation) work *O aprjoryczności matematyki* Stamm aims at showing the relations between Kant's philosophy and logicism as well as reflecting on the problem of the necessity and commonness of mathematics (referring to Couturat he calls them 'apriority' like in the title). According to Russell and logicism pure mathematics is a collection of

[54] It is worth adding that this work includes a presentation of the works of Stanisław Pudłowski (1597–1645), a professor of the Cracow Academy. Stamm focused on Pudłowski's ideas related to the formation of symbolic languages of logic and mathematics. It must have been connected with Stamm's conviction pertaining to the role of formal methods and symbolic languages for mathematics and logic, cf. below.

judgements in the forms of conditionals, i.e. a system of hypothetical-deductive relations. Couturat stated that these relations were independent from the truthfulness or fulfilment of axioms; thus they are absolutely true. Here is the source of their necessity and commonness, i.e. apriority. However, there is the problem of consistency of axioms accepted in mathematics. It is a very serious problem since we have no absolute proofs of consistency. In logicism one cannot refer to experiment to determine consistency, and consequently, one can merely speak about the relative necessity and commonness of mathematics.

The problem of necessity and commonness is not a special problem of mathematics. On the contrary, it can be formulated with regard to any discipline. Stamm discusses this issue in general, from the point of view of psychology and the theory of cognition (considering the genetic aspect of cognition). He concludes that:

> [...] among mathematical judgements (and also axioms) one can differentiate degrees of necessity, and on the other hand, we can easily find non-mathematical (and illogical) judgements that are also necessary, like the former.
>
> As a result, one cannot speak about the absolute necessity and commonness of mathematical judgements, and that from this perspective the problem of the apriority of mathematics is not of a special character at all; it is then a question concerning (basic) judgements of sciences in general (1909, p. 511).[55]

Taking into account the genetic point of view, the commonness and certainty of axioms are essential for mathematics and other sciences. Stamm regards it as the problem of the natural system of axioms and adds that only such a system 'can satisfy the aspects: formal, logical and material, psychological' (1909, p. 514).

Stamm's most important—with respect to his theses—dissertation concerning the philosophy of mathematics is probably the paper 'Czem jest i czem będzie Matematyka?' (1910). He asks what kind of science mathematics is and what role it plays towards other sciences. His conclusions become references to his other reflections on mathematics. As his research scheme Stamm chooses to compare mathematics to other sciences with regard to their contents and applied methods.

In his opinion one can see a clear regularity in the development of mathematics, namely a transition from the investigation of quantitative relationships to the research that 'have almost no quantitative character' and in which 'the concept of quantity plays almost no role' (1910, p. 183). Thus the definition of mathematics as a science on quantities becomes invalid. A concept of order, as Russell showed, became more important in pure mathematics.

If one considers the algebra of logic, a new discipline which mathematics embraces, one can discern another characteristic. The algebra of logic has both

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[55] '[...] między sądami (także pewnikami) matematycznymi rozróżnić można stopnie konieczności, a z drugiej strony znajdziemy łatwo sądy niematematyczne (i nielogiczne), które są również konieczne, jak i tamte.

Z tego wynika, że nie można mówić o absolutnej konieczności i powszechności sądów matematycznych, i że z tego stanowiska problem aprioryczności matematyki nie ma wcale specjalnego charakteru; jest to więc pytanie odnoszące się do sądów (podstawowych) nauk w ogóle.'

logical and ontological aspects (cf. the calculus of concepts or the calculus of classes). Thus a part of philosophy has entered mathematics.

Mathematics embraces more and more objects within its investigations, which leads—according to Stamm—to the thesis about the indefinitiveness of the scope of mathematical investigations. Stamm concludes that:

> [. . .] *the content of mathematics is not separated from the content of other sciences*. In the historical development we can see the opposite: mathematics *absorbs subjects of other sciences slowly* (1910, p. 186).[56]

Moving to the scientific method, in particular mathematical methods, Stamm distinguishes three stages in every science (carefully separating science from knowledge, ascribing to the former the feature of inner order and ability to predict, which is in turn possible thanks to classification): (1) inductive stage, formulating principles and axioms; (2) stage of deduction from axioms, and finally (3) stage of induction related to applications. The second stage is especially developed in mathematics. The first stage is regarded as a private matter of the scientists. Thus we have the false conviction about the purely deductive method of mathematics. Additionally, within the deductive stage symbolism plays an important role, particularly in mathematics. Stamm regards it as 'the crown of deduction' and 'the most perfect degree of development' (1910, p. 190). Symbolism:

> allows [. . .] us to predict more surely; it frees us from unnecessary thinking and in general, it is the economy of the method; the symbolically presented theories become much stricter than those presented verbally do. Words have no permanent meanings whereas the *stability* of symbols is almost ideal (1910, p. 190).[57]

However, one must notice that other sciences also aim at deducting and applying symbolism. Consequently, the method does not differentiate mathematics from other sciences. Moreover, in its development mathematics had a stage during which it resembled natural sciences, for example in ancient Egypt or Babylon.

Thus Stamm formulates his main thesis:

> *Mathematics is not a science at all, but it is the method, that ideal, deductive-symbolic state of science in general. We give the name of mathematics to these sciences that have achieved such a state*, namely arithmetic, analysis, geometry, the algebra of logic, etc. Yet one should not think that the deductive-symbolic state, i.e. the mathematical one, is the *arithmetic or geometric state*, that the mathematisation of science consists in applying counting and measuring (1910, p. 192).[58]

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[56] '[. . .] *treść matematyki nie jest odgraniczona od treści innych nauk*. W rozwoju historycznym możemy przeciwnie zauważyć, że matematyka *absorbuje powoli przedmioty innych nauk*.'

[57] 'pozwala [. . .] pewniej przepowiadać, uwalnia nas od zbytecznego myślenia, jest w ogóle ekonomią metody; teorie symbolicznie przedstawione stają się o wiele ściślejsze, aniżeli słownie przedstawione. Podczas gdy słowa nie posiadają stałych znaczeń, jest stałość symboli prawie idealna.'

[58] '*Matematyka nie jest wcale nauką, lecz metodą, owym idealnym, dedukcyjno-symbolicznym stanem nauki w ogóle. Matematyką nazywamy te nauki, które stan taki osiągnęły*, a więc arytmetykę, analizę, geometrię, algebrę logiki itd. Ale nie należy sądzić, że stan dedukcyjno-

This thesis is similar to the thesis of Benjamin Peirce[59] that ‘Mathematics is the science, which draws necessary conclusions,’ but the word ‘science’ should be replaced by ‘method.’

The discussed paper also includes a polemic with Russell, who thinks that pure mathematics is the class of propositions asserting formal implications and consequently, mathematics has a fully deductive-hypothetical character and depends only and exclusively on logic. Stamm is right stating that according to Russell logic in fact means *logica magna*, and thus also embraces set theory, i.e. part of ontology. Russell’s definition does not refer to applied mathematics, either.

Stamm tries to support his thesis that mathematics is the method in ‘Characteristica geometrica Leibniza i jej znaczenie w Matematyce’ (1913b). He describes Leibniz’s symbolic system related to geometry (and based on the concept of congruence). He shows that Leibniz’s *characteristica universalis* is a generalisation of *characteristica geometrica*, noticing that it consists largely of symbolic logic and partly of ontology, which is strictly connected with logic, i.e. science about concepts called the calculus of classes. The systems of Hermann Grassmann and Giuseppe Peano are, according to Stamm, the continuations of Leibniz’s *characteristica geometrica*, showing the effectiveness of this attitude. In the future the common method of mathematics will be the theory of relations, which Stamm calls the theory of relativity and describes at the end of his work in question.

He also writes about the theory of relations and its significance for the foundations of mathematics as well as for the philosophy of mathematics in his review of Paul N. Natorp’s book *Die logischen Grundlagen der exakten Wissenschaften* (1910), published as ‘Logiczne podstawy nauk matematycznych’ [Logical Foundations of Mathematical Sciences] (1911a). Discussing the work of the German philosopher, written in the spirit of Neo-Kantianism, he formulates his own philosophical views on mathematics. Analysing Natorp’s problem of the attitude of logic towards mathematics he criticises Russell’s logicistic concept, according to which pure mathematics is ‘the continuation of logic’ (1911a, p. 255). Stamm thinks that logic itself is not sufficient to build mathematics. What is needed is ontology or at least its fragment, in particular the theory of relations, which Russell (wrongly, in Stamm’s opinion) includes to logic.

What is worth noticing in Stamm’s works on philosophy is his good understanding of the contemporary trends and tendencies, in particular his positive attitude towards logicism.[60] However, he also sees its weaknesses, for which he gives accurate arguments and with which he argues. One can also see his sympathy

symboliczny, a więc matematyczny, jest stanem *arytmetycznym albo geometrycznym*, że matematyzowanie się nauki polega na zastosowaniu liczenia i mierzenia.’

[59] Benjamin Peirce (1809–1880), an American mathematician, astronomer and lecturer, professor at Harvard University and perhaps the first serious research mathematician in America. He was the father of Charles Sanders Peirce (1839–1914), an American philosopher, logician, mathematician and scientist, sometimes known as ‘the father of pragmatism.’

[60] It should be added that in those days the trend related to formalism and intuitionalism either did not exist or was developed to a small extent.

towards and interest in the algebra of logic, which was then the prevailing approach to the system of logic.

Certain elements of the philosophy of mathematics can be found in Stamm's large publication 'O przedmiotach urojonych' (1913a). It concerns epistemology and partly ontology. Stamm discusses real objects (which he divides into synthetic and analytic ones) and imaginary objects. Then he reflects on the role and significance of the latter in science, especially in physics and mathematics as well as in religion and art. He mentions examples of imaginary objects in mathematics: limit, differentials, infinity, point, line and surfaces, and in philosophy: the Kantian thing-in-itself (*Ding an sich*), alien self, and in religion: God as the cause of everything. He also tries to explain their status and origin with the help of (unfortunately, slightly vague) psychological reflections supported by the use of the language of the theory of relations. He claims that in science we must use imaginary objects if we desire its development. He writes:

> Since imaginary objects are also tools of predicting and anticipating natural real predictions. [. . .] Without imaginary objects we would look at the world only from our own level; imaginary objects allow us to look from the highlands. That is why we can command a view of far wider domains (1913a, p. 464).[61]

[61] 'Przedmioty urojone są bowiem także narzędziem przepowiadania i narzędziem wyprzedzającym naturalne przepowiedniki rzeczywiste. [. . .] Bez przedmiotów urojonych patrzylibyśmy na świat tylko z własnego poziomu; przedmioty urojone pozwalają spoglądać z wyżyn. Dlatego jesteśmy w stanie ogarnąć wzrokiem daleko szersze dziedziny.'

Chapter 2
The Polish School of Mathematics

This chapter presents the philosophical views on mathematics and logic that appeared in the papers (and research practice) of the representatives of two main mathematical centres in interwar Poland, namely the Warsaw one and the Lvov one.

2.1 Warsaw School of Mathematics: Sierpiński, Janiszewski, Mazurkiewicz

Speaking about the philosophy of mathematics in the Warsaw School of Mathematics one must recall three figures: Wacław Sierpiński, Zygmunt Janiszewski and Stefan Mazurkiewicz. Their philosophical views on mathematics were expressed primarily in set theory.

However, let us begin with Sierpiński's habilitation procedure, which took place in 1908. His habilitation lecture, delivered during the meeting of the Council of the Faculty of Philosophy at the Jan Kazimierz University in Lvov, concerned a certain issue of the philosophy of mathematics. Its title was 'Pojęcie odpowiedniości w matematyce' [The Concept of Correspondence in Mathematics]. Then the lecture was published as a paper (bearing the same title) in *Przegląd Filozoficzny* [Philosophical Review] in the year 1909.

Sierpiński aimed at reflecting on the role and significance of the concept of correspondence in mathematics. He examined various disciplines that embraced this concept, paying special attention to the concept of equipollency of sets and cardinal numbers, operations, analytic geometry, complex numbers, geometry (in particular cartography, projective geometry and descriptive geometry), analysis and the concept of function. He concluded that the concept of correspondence was one of the most important mathematical concepts, writing:

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6_2

> It penetrates all areas of mathematical thought; it is the basis on which we build other fundamental concepts; it is the source of all the most wonderful ideas (1919, p. 8).[1]

Sierpiński justifies this fact by quoting Poincaré's statement from *La Science et l'hypothèse*:

> Mathematicians do not study objects, but the relations between objects; to them it is a matter of indifference if these objects are replaced by others, provided that the relations do not change (1905, p. 25).[2]

Finally, Sierpiński postulates:

> [. . .] the fact that the science—thus separated—which is mathematics, finds so many real applications can be explained by the existence of perfect correspondence between the domain of abstraction and the domain of true reality (1909, p. 19).[3]

It is a strong thesis regarding one of the fundamental issues of the philosophy of mathematics, namely the problem of relations between pure mathematics and applied mathematics as well as the problem of the mathematisation of the physical world. In fact, Sierpiński neither solves these problems nor justifies his thesis, but at this point, it is not the most important thing. What is essential is the fact that he chose a problem of the philosophy of mathematics as the theme of his habilitation lecture.

Several years later it was Zygmunt Janiszewski that made a similar choice. Although his *Habilitationsschrift* concerned topology, he decided to lecture on the problem of the dispute between realists and idealists in the philosophy of mathematics during the session of the Council of the Faculty of Philosophy of the Jan Kazimierz University in Lvov, held on 11 July 1913. The title of his lecture was 'O realizmie i idealizmie w matematyce' [On Realism and Idealism in Mathematics]. It was published, bearing the same title, in *Przegląd Filozoficzny* in 1916 (as was Sierpiński's lecture).

The debate between realism and idealism had been held in the philosophy of mathematics since the very beginning (cf. ontological concepts concerning mathematical objects, which were put forward by Plato, recognised as the father of idealism, and by Aristotle, seen as the father of realism). Its apogee fell at the turn of the twentieth century because of Cantor's set theory, especially after 1904 when Zermelo proved the well-ordering theorem, which turned mathematicians' attention

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[1] 'Przenika ono wszystkie dziedziny myśli matematycznej; jest podstawą, na której budujemy inne zasadnicze pojęcia; jest źródłem wszystkich najwspanialszych pomysłów.'

[2] 'Les mathématiciens n'étudient pas des objets, mais des relations entre les objets; il leur est donc indifférent de remplacer ces objets par d'autres, pourvu que les relations ne changent pas' (1902, p. 32).

[3] '[. . .] fakt, że nauka, tak oderwana, jaką jest matematyka, znajduje tyle zastosowań realnych, wytłumaczyć daje się istnieniem doskonałej odpowiedniości między dziedziną abstrakcji a dziedziną realnej rzeczywistości.'

to the (controversial) axiom of choice.[4] The debate comes down to the question, 'What does it mean "to exist" (in mathematics)?' Let us notice that both the axiom of choice and Zermelo's well-ordering theorem announce the existence of certain objects (the axiom of choice—the set of selectors; Zermelo—the relation of well-ordering) in a non-constructive way, i.e. they do not give any information about the postulated objects: how to construct them.

In his paper Janiszewski analyses the statements of realists and points to the difficulties they faced. He also discusses the necessary and sufficient conditions for existence in mathematics. Naturally, non-contradiction is a necessary condition. But is it sufficient? Idealists claim that it is. In their opinion, 'being' means 'being non-contradictory.' Realists say that the answer is negative, i.e. in mathematics what has 'a (good) definition' exists (1916, p. 163). Obviously, this leads to another problem: what does '*good* definition' mean?

Consequently, according to realists a set is defined when—if one cannot define all its elements individually—at least the law of construction of any element of the set is given (cf. 1916, p. 168). Whereas idealists claim that one can define a set without defining its individual elements. A set is defined when we have a membership criterion (Cantor accepted this principle).

Janiszewski concludes that in the philosophy of mathematics the debate between idealists and realists shows that:

> [. . .] contrary to the spread belief about the complete obviousness and certainty of mathematical argumentations here we can also encounter controversial problems (1916, p. 169).[5]

Such cases were numerous in mathematics. However, solutions were always found. Will this apply to the philosophy of mathematics? Janiszewski gives a pessimistic answer, stating:

> One should doubt it. Since the diversity of philosophical views, which is revealed in this dispute and which is its source, is this eternal difference that caused controversies between nominalists and Platonists throughout the Middle Ages, this dispute has lasted until today as the conflict between positivism and idealism (1916, p. 170).[6]

It is worth noting that Janiszewski does not support any party of the debate. He only presents various standpoints and arguments, which is—as will be seen—a typical attitude of the Warsaw mathematicians' environment.

Yet, let us return to the question posed while presenting the theme of Sierpiński's habilitation lecture. Let us ask why Sierpiński and Janiszewski chose

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[4] The well-ordering theorem is equivalent to—on the basis of a proper axiomatic system of set theory—the axiom of choice. On the theme of these relationships as well as the history, status and meaning of the axiom of choice in mathematics see Murawski (1995), Appendix I.

[5] '[. . .] w przeciwieństwie do rozpowszechnionego mniemania o bezwzględnej oczywistości i pewności rozumowań matematycznych i tu spotykamy kwestie sporne.'

[6] 'O tym należy wątpić. Różnica bowiem filozoficznych poglądów, która się objawia w tym sporze, która jest jego źródłem—jest tą odwieczną różnicą, która powodowała przez średniowiecze ciągnący się spór między nominalistami a platończykami, który ciągnie się i dziś między pozytywizmem a idealizmem.'

issues concerning the philosophy of mathematics although they were 'purebred' mathematicians. Could the fact that their habilitation procedures were conducted before the Council of the Faculty of Philosophy, embracing mostly humanists and not mathematicians, have been a decisive factor? Could the scholars not have been interested in a strictly mathematical (more technical) theme? After all, Sierpiński and Janiszewski might have chosen some popular mathematical issue. The fact that they chose themes pertaining to the philosophy of mathematics shows that at the Lvov University the intellectual atmosphere was good as far as the foundations and philosophy of mathematics were concerned, and both mathematicians were interested in mathematics as such as well as its philosophical problems. Moreover, both were convinced that in Poland there was a need for some definite conception of growth in mathematics so that this discipline could be practised and developed. They wanted to define its methodological foundation, which should be set theory (as will be seen later).

Janiszewski showed interest in the philosophy of mathematics and was convinced about its significance much earlier, on the occasion of the publication of *Poradnik dla samouków* [A Guide for Autodidacts] in 1915. He was the soul of the whole undertaking and author of the biggest number of papers published in the guide. Besides the introduction and conclusion as well as the information chapter he wrote papers on differential, functional, difference and integral equations as well as on series, the foundations of geometry, logic and the philosophy of mathematics.[7]

The last two papers, i.e. 'Logistyka' [Logistics] (Janiszewski 1915a) and 'Zagadnienia filozoficzne matematyki' [Philosophical Problems of Mathematics] (Janiszewski 1915b), are the most important ones from our perspective.

The first paper, 'Logistyka', presents mathematical logic (called symbolic logic or—the special term used then—logistics). Janiszewski begins by explaining the reasons why this book, dedicated to mathematics, speaks about logic. He mentions four:

a) logistics is formulated as calculus (*algebra of logic*) whereas mathematics is regarded as the science concerning all calculuses;
b) it is the only science that can be applied to mathematics;
c) in some branches (e.g. the theory of relations) it examines the same objects as mathematics, but they are considered on a large scale;
d) logistic *calculus* has both logical and mathematical interpretations and thus it undeniably belongs to mathematics (namely, to set theory) (1915a, p. 449).[8]

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[7] Besides Janiszewski the authors of the papers in *Poradnik* were: Stefan Kwietniewski, writing about analytic, synthetic, descriptive and differential geometry as well as the history of mathematics, Wacław Sierpiński, writing about arithmetic, number theory, higher algebra, set theory, real variable theory, differential and integral calculus, Stanisław Zaremba, writing about analytic function theory, partial differential equations, group theory and calculus of variations, and Stefan Mazurkiewicz who wrote about probability calculus. The introductory chapter 'O nauce' [About Science] was written by Jan Łukasiewicz.

[8] '(a) logistyka ujęta jest w postaci rachunku (*algiebra logiki*), matematykę zaś uważamy za naukę o wszelkich rachunkach;

(b) jest ona jedyną nauką, mogącą mieć w matematyce zastosowanie;

In a footnote Janiszewski adds that interpretation is also possible in number theory.

Furthermore, he characterises logistics, saying that it is '*formal logic* (i.e. the science of forms of pure thought) *using the mathematical method*; speaking more strictly: method, which so far only mathematics has applied on a large scale' (1915a, p. 449).

He regards the use of symbols in logistics as one of the characteristics that distinguish and differentiate it from other (also earlier) forms and branches of logic.[9]

In the discussed paper Janiszewski focuses on facts from the history of logistics and its most important achievements. He stresses that attacks on mathematical logic and undermining its significance are not supported by any serious arguments, and 'the funny, full of deeper thoughts, but mischievous chapters of the book *Science et méthode* by Poincaré, concerning discussing logistics, are rather satire than criticism' (1915a, p. 456).[10]

It is of interest to note his commentaries on the relations between logic and mathematics as well as his standpoint concerning the status of logic. Janiszewski is

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(c) w niektórych działach (np. teorii stosunków) traktuje o tych samych przedmiotach co i matematyka, tylko szerzej ujętych;

(d) *rachunek* logistyczny ma interpretację nie tylko logiczną, lecz i matematyczną, należy więc bezsprzecznie i do matematyki (mianowicie do teorii mnogości).'

[9] He even adds that 'it became the cause of unpopularity of logistics among philosophers' (1915a, p. 450).

[10] Poincaré wrote in *Science and Method* (1914, Book II, Chapter III: Mathematics and Logic, Paragraph VII; Pasigraphy): 'The essential element of this language consists in certain algebraical signs which represent the conjuctions: if, and, or, therefore. That these signs may be convenient is very possible, but that they should be destined to change the face of the whole of philosophy is quite another matter. It is difficult to admit that the word *if* acquires when written ɔ, a virtue it did not possess when written if.

This invention of Peano was first called *pasigraphy*, that it to say the art of writing a treatise on mathematics without using a single word of the ordinary language. This name defined its scope most exactly. Since then it has been elevated to a more exalted dignity, by having conferred upon it the title of *logistic*. The same word is used, it appears, in the *École de Guerre* to designate the art of the quartermaster, the art of moving and quartering troops. But no confusion need be feared, and we see at once that the new name implies the design of revolutionizing logic.'

Science et méthode (1908, Livre II, Chapitre III: Les Mathématiques et la Logique, VII. La pasigraphie): 'L'élément essentiel de ce langage, ce sont certains signes algébriques qui représentent les différentes conjonctions: si, et, ou, donc. Que ces signes soient commodes, c'est possible; mais qu'ils soient destinés à renouveler toute la philosophie, c'est une autre affaire. Il est difficile d'admettre que le mot *si* acquiert, quand on l'ecrit $\supset$, une vertu qu'il n'avait pas quand on l'écrivait si.

Cette invention de M. Peano s'est appelée d'abord la *pasigraphie*, c'est-à-dire l'art d'écrire un traité de mathématiques sans employer un seul mot de la langue usuelle. Ce nom en définissait très exactement la portée. Depuis, on l'a élevée à une dignité plus éminente, en lui conférant le titre de *logistique*. Ce mot est, paraît-il, employé à l'École de Guerre, pour désigner l'art du maréchal des logis, l'art de fair marcher et de cantonner les troupes; mais ici aucune confusion n'est à craindre et on voit tout de suite que ce nom nouveau implique le dessein de révolutionner la logique.'

aware of the fact that mathematical logic can be a convenient and useful tool for analysing language and arguments, that ‘sometimes logistic calculus can have the significance of a method and can facilitate making conclusions’ (1915a, n. 1, p. 456). He clearly declares:

> We recommend everyone to get to know some aspects of logistics; those who want to have an idea of the present day condition of logic, especially professional philosophers and in a way, mathematicians, too [. . .]. Particularly, it becomes indispensible for them if they want to work on the philosophy of mathematics (1915a, p. 455).[11]

Knaster writes that Janiszewski himself did his best ‘to gain profound knowledge of mathematical logic, then known as logistics, and began applying it’ (1960, p. 2). He used mathematical logic ‘first of all to solve methodically mathematical problems using widely the specific symbolism of set theory’ as well as ‘to reveal deficiency and ambiguity in the structure of mathematical concepts, even such basic ones as line and surface’ (1960, p. 2). However, in his paper Janiszewski clearly states that (mathematical) logic is an independent and autonomous mathematical discipline and not only a mathematical method or tool (cf. 1915a, p. 456); that ‘in fact, it does not aim at (at least direct) practical benefits’ (1915a, n. 1, p. 454). This should be emphasised, considering that Janiszewski studied in France, which was influenced by Poincaré (cf. above). This ‘pro-logic’ attitude and the emphasis on the significance of mathematical logic for mathematics itself, together with the decisive acceptance of its autonomy and independence, are very important and they characterise the Warsaw School (and undoubtedly, contributed to the development of the Warsaw School of Logic).

The other paper written by Janiszewski concerns philosophical problems of mathematics. The author discusses particular questions of a philosophical nature related to mathematics, especially the problem of deductive or inductive character of mathematics, the character of mathematical induction, the correctness of definition, the nature of objects of mathematics as well as the mode of their existence. He presents the debate between idealists and realists, discusses the role and importance of antinomies, philosophical issues concerning space and the related problem of the nature and character of geometrical theories as well as the sense of the question about their validity. Each of these problems contains references. At the end of the paper, there is a list (with commentaries) of general publications concerning the philosophy of mathematics, which proves Janiszewski’s excellent knowledge of the current philosophical literature on the subject of mathematics. It is worth noting the subtle distinctions he drew when formulating problems. Another characteristic is that—like in other publications—he never formulates his own views but only presents (in a very competent way) other people’s opinions. Thus he shows the complexity of problems. On the one hand, he stresses the independence of mathematicians’ work from certain philosophical issues and on the other hand, he thinks

[11] ‘Pewne zaznajomienie się z logistyką należy polecić każdemu, kto chce mieć pojęcie o dzisiejszym stanie logiki, szczególniej więc fachowym filozofom, a poniekąd i matematykom [. . .]. Staje się zaś ona dla nich niezbędna, jeśli zechcą się zająć filozofią matematyki.’

that there are controversial philosophical questions that do influence mathematicians' work. He writes:

> The problems discussed in the previous paragraphs are, so to say, outside the scope of mathematicians' work: whatever views concerning these problems they have, or if they have no views, that will not influence—at least directly—their work within mathematics and will not hinder them from reaching an agreement with other mathematicians in this sphere. Regardless of their definitions of natural numbers or mathematical induction all mathematicians will use them in the same way. However, there are disputable questions exerting a direct influence on current mathematical work. They concern the *importance* of certain mathematical argumentations and *objectivity* of certain mathematical concepts (1915b, p. 470).[12]

Among the latter he mentions the dispute concerning imaginary quantities, infinitesimal calculi, the summation of series or Poncelet's continuity principle, which are now of a historical character. He also analyses the questions of the accuracy of definitions, which he regards as valid (for instance, whether mathematics should allow impredicative definitions) or certain issues related to set theory.

Indeed, set theory played an important role in the Warsaw School. It all began with Sierpiński's discovery. In 1907, he stated that plane and line were composed of the same number of points. Soon he learnt[13] that 30 years earlier this fact had been discovered by Georg Cantor and that it had been the basic result of a new discipline, namely set theory. From that moment on, this theory became Sierpiński's main interest. As a professor of the Jan Kazimierz University in Lvov from 1910[14] he lectured on this subject there,[15] and he wrote the textbook entitled *Zarys teorii mnogości* [An Outline of Set Theory] (1912).

Interned at the beginning of World War I by the Russian authorities[16] (in Wiatka), he finally found himself in Moscow—thanks to his Russian colleagues'

[12] 'Zagadnienia, poruszone w poprzednich paragrafach, znajdują się, że tak powiemy, poza obrębem działalności matematyka: jakiekolwiek będzie on miał poglądy na nie, czy też nie będzie ich mieć wcale, to nie wywrze—przynajmniej bezpośrednio—wpływu na jego pracę w obrębie matematyki i w tym obrębie nie utrudni porozumienia z innymi matematykami. Bez względu na to, za co uważają liczby naturalne albo indukcję matematyczną, wszyscy matematycy będą się nimi posługiwać w jednakowy sposób. Istnieją jednak i takie kwestie sporne, które mają wpływ bezpośredni na aktualną pracę matematyczną. Dotyczą one *ważności* pewnych rozumowań matematycznych i *przedmiotowości* niektórych pojęć matematycznych.'

[13] Mostowski writes (1975, p. 9) that when Sierpiński made this discovery he asked his colleague Tadeusz Banachiewicz, who had studied in Göttingen and then became a professor of astronomy at the Jagiellonian University, whether he knew that conclusion. Banachiewicz answered by sending him a telegram containing only one word, 'Cantor.' Thus he turned Sierpiński's attention to Cantor's works. The former began studying them.

[14] He was the head of one of the two chairs of mathematics; the other one was directed by Józef Puzyna.

[15] The opinion, which is sometimes spread, that these were the first lectures on this new discipline conducted in the world is wrong. Earlier lectures on set theory were given by Ernst Zermelo (Göttingen in 1900–1901), Felix Hausdorff (Leipzig in 1901) and Edmund Landau (Berlin in 1902–1903, 1904–1905).

[16] When the war broke out Sierpiński was on holiday in Russia.

efforts—where he collaborated with Nikolai N. Luzin and where he got to know the theory of analytic sets, then being formulated and developed. In the future Sierpiński would be one of the most important figures who developed this part of set theory, i.e. descriptive set theory.

In Lvov Sierpiński made the young mathematicians Zygmunt Janiszewski, Stefan Mazurkiewicz and Stanisław Ruziewicz interested in set theory.

Several months after the Russian authorities had evacuated their university from Warsaw to Rostov-on-Don in 1915, Poles opened their university in Warsaw. Its first professors included Zygmunt Janiszewski and Stefan Mazurkiewicz. Towards the end of 1918 Wacław Sierpiński joined them, taking over the chair of mathematics. This place therefore gathered people who had the same research interests: they were dedicated to set theory.[17]

In 1917, responding to the appeal of the Mianowski Fund, Janiszewski wrote a paper entitled 'O potrzebach matematyki w Polsce' [About the Needs of Mathematics in Poland] (1917). This small (only six pages) paper became the programme of the whole generation of Polish mathematicians. Janiszewski postulated focusing on one branch of mathematics[18] and creating a new mathematical periodical. He wrote:

> According to the above-mentioned project one should create a strictly scientific periodical, entirely dedicated to one of these branches of mathematics in which we have outstanding, truly creative and numerous workers. This paper [. . .] would accept papers in any of the four languages that mathematics recognises as international [. . .]. The periodical would contain, besides original papers, bibliographies of this branch, summaries, and even reprints of important papers published somewhere else, in particular translations of valuable papers, published in 'non-international' languages, i.e. mainly Polish works that are wasted as unknown; finally, correspondence: answers to questions [. . .].
>
> [. . .] let us return to mathematical creativity. Here dealing with common themes can create a suitable atmosphere. A researcher just needs co-workers. When he is alone he usually stops creating. The reasons are not only psychological, the lack of stimulus: being alone he *knows* much less than others who work jointly. What he gets is only results of research, fully developed and complete ideas, they are often published several years after they were formulated. Secluded, he did not see how and from what they originated; he did not experience this process with their creators. 'We are far from these forges or pots in which mathematics is created; we come late and there is no help, we must be behind,' I heard from some Russian mathematician in Göttingen, speaking about his fellow countrymen. How much more it applies to us!
>
> Well, if we do not want to always 'fall behind' we must resort to radical means, reach the foundations of evil. We must create such a 'forge' at home! We can succeed only by gathering most of our mathematicians and making them work on one branch of mathematics. At present it is being done by itself; one must only help this movement. In fact, creating

[17] Ruziewicz was a professor of the University of Technology and at the Jan Kazimierz University. He was also Rector of the Academy of Foreign Trade in Lvov.

[18] The importance of this postulate can be testified by a story described by Marczewski (1948, pp. 17–18): 'When [. . .] in 1911 Puzyna, Sierpiński, Zaremba and Żorawski met as a group during the Congress of Natural Scientists and Medical Doctors in Cracow they could not find a common subject: their interests were so much divergent.'

> a special periodical for one branch of mathematics at our place will draw many to work in this domain.
>
> But the periodical would help create this 'forge' in another way: then we would be a technical centre of mathematical publications concerning this branch. New works would be sent to us and relationships would be maintained with us (1917, pp. 15 and 18). [19]

Naturally, the field that was to draw the research efforts of Polish mathematicians was set theory and related disciplines: topology, the theory of real functions, etc.[20] It was the area of research of the Warsaw mathematicians, who had come from Lvov, and some Lvov mathematicians. In order to fulfil Janiszewski's second postulate the new periodical *Fundamenta Mathematicae* was called into being. The cover of its first volume[21] said that the periodical was dedicated to 'set theory and related issues (direct applications of set theory), Analysis Situs,[22] mathematical logic, axiomatic investigations.' The first volume appeared in 1920.[23]

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[19] 'W myśl powyższego projektu należałoby założyć u nas czasopismo ściśle naukowe, poświęcone wyłącznie jednej z tych gałęzi matematyki, w których mamy pracowników wybitnych, prawdziwie twórczych i licznych. Czasopismo to [. . .] przyjmowałoby artykuły w każdym z czterech języków uznanych w matematyce za międzynarodowe [. . .]. Pismo to zawierałoby, obok artykułów oryginalnych, bibliografie tej gałęzi, streszczenia, a nawet przedruki ważniejszych artykułów, drukowanych gdzie indziej, szczególnie zaś tłumaczenia artykułów wartościowych, drukowanych w językach nie "międzynarodowych", a więc przede wszystkim prac polskich, które marnują się nieznane; wreszcie korespondencje: odpowiedzi na zapytania [. . .].

[. . .] powróćmy do sprawy twórczości matematycznej. Tu atmosferę odpowiednią może wytworzyć dopiero zajmowanie się wspólnymi tematami. Konieczni prawie dla badacza są współpracownicy. Odosobniony najczęściej zamiera. Przyczyny tego są nie tylko psychiczne, brak pobudki: odosobniony *wie* o wiele mniej od tych, co pracują wspólnie. Do niego dochodzą tylko wyniki badań, idee już dojrzałe, wykończone, często w kilka lat po swym powstaniu, gdy ukażą się w druku. Odosobniony nie widział, jak i z czego one powstawały, nie przeżywał tego procesu razem z ich twórcami. "Jesteśmy z daleka od tych kuźni czy kotłów, w których wytwarza się matematyka, przychodzimy spóźnieni i, nie ma rady, musimy pozostać w tyle" mówił mi w Getyndze o swoich rodakach pewien uczony matematyk rosyjski. O ileż bardziej stosuje się to do nas!

Otóż, jeśli nie chcemy zawsze "pozostawać w tyle", musimy chwycić się środków radykalnych, sięgnąć do podstaw złego. Musimy stworzyć taką "kuźnię" u siebie! Osiągnąć zaś to możemy tylko przez skupienie większości naszych matematyków w pracy nad jedną gałęzią matematyki. Dokonywa się to obecnie samo przez się, trzeba tylko temu prądowi dopomóc. Otóż niewątpliwie utworzenie u nas specjalnego pisma dla jednej gałęzi matematyki pociągnie wielu do pracy w tej gałęzi.

Lecz jeszcze w inny sposób pismo dopomogłoby do wytworzenia się u nas tej "kuźni": bylibyśmy wtedy ośrodkiem technicznym publikacji matematycznych w tej gałęzi. Do nas przysyłano by rękopisy nowych prac i utrzymywano by z nami stosunki.'

[20] Let us note that in his paper Janiszewski does not speak clearly about any concrete discipline. It cannot be excluded that in those days the conflict with Zaremba was about to start—cf. Chap. 3. In fact, Zaremba wrote a paper about the needs of mathematics, which was published in the same volume of *Nauka Polska* [Polish Science] as Janiszewski's work.

[21] This phrase was repeated in every volume.

[22] Today called typology.

[23] Unfortunately, Janiszewski did not see the publication of this volume—he died on 3 January 1920 when the Spanish influenza struck again.

Janiszewski and other scientists saw and stressed the connection between set theory and other branches of mathematics (both the classical ones and the ones being developed). They did not see it a separate and single theory. In his paper 'À propos d'une nouvelle revue mathématique: *Fundamenta Mathematicae*' (1922), written on the occasion of the second volume of *Fundamenta*, Henri Lebesgue stated that 'set theory was removed beyond the sphere of mathematics by the great priests of the theory of analytic functions,' and if 'now this ostracism against set theory vanishes' it is thanks to the fact that 'set theory, which developed from the theory of analytic functions, could turn out to be useful for its elder sister and could show people of good will its values and riches.'

This conviction of the place and role of set theory in mathematics, shared by the creators of the Polish School of Mathematics, found its decisive expression in the above-mentioned *Poradnik dla samouków*. Stefan Mazurkiewicz wrote in the paper 'Teoria mnogości w stosunku do innych działów matematyki' [Set Theory vs. Other Branches of Mathematics], published in the third volume of *Poradnik* (as a supplement to the first volume):

> Reflecting on the table of 'the division of mathematics' made by Janiszewski (*Poradnik*, vol. 1, pp. 22/23) we can see that the position of set theory in the table was determined in a very special way. The table has two wings, which is in accordance with the traditional division of mathematics into two branches: on the left we have analysis (including arithmetic and algebra) and on the right—geometry. On the central line we have only two theories: set theory and group theory. Moreover, let us see that moving downwards the table we go from generally simpler branches, more primary and self-sufficient, to more complex ones, requiring external supporting means; thus we have a kind of pyramid of mathematical skills, pyramid obviously based on top. This top is set theory, which occupied the highest place in the table, having the foundations of arithmetic, the foundations of geometry and topology directly under it. Finally, we can see numerous 'lines of relation,' diverging (mostly centrifugally) from set theory in all directions.—Recapitulating, one can say that the table gives set theory the place that is almost prevailing in mathematics (being both basic and central); furthermore, it highlights its influence on other fields (1923, pp. 89–90).[24]

[24] 'Rozważając ułożoną przez Janiszewskiego tablicę "podziału matematyki" (*Poradnik*, t. 1, str. 22/23), dostrzegamy, że stanowisko teorii mnogości zostało w tablicy tej wyznaczone w sposób bardzo szczególny. Tablica jest dwuskrzydłowa, co jest zgodne z tradycyjnym podziałem matematyki na dwie gałęzie: po lewej stronie mamy analizę (łącznie z arytmetyką i algebrą), po prawej geometrię. Na linii środkowej znajdujemy dwie tylko teorie: teorię mnogości i teorię grup.—Zauważmy nadto, że przesuwając się w tablicy omawianej od góry ku dołowi, przechodzimy na ogół od działów prostszych, bardziej pierwotnych samowystarczalnych—do bardziej złożonych i wymagających z zewnątrz czerpanych środków pomocniczych, tym sposobem mamy tu rodzaj piramidy umiejętności matematycznych, opartej oczywiście na wierzchołku. Otóż tym wierzchołkiem jest teoria mnogości, która zajmuje w tablicy miejsce szczytowe, mając pod sobą bezpośrednio podstawy arytmetyki, podstawy geometrii i topologię.—Wreszcie widzimy liczne "linie związku", rozchodzące się (przeważnie odśrodkowo) od teorii mnogości we wszystkich kierunkach.—Reasumując, powiedzieć można, że tablica nadaje teorii mnogości stanowisko niemal dominujące w matematyce (gdyż zarazem podstawowe i centralne), ponadto zaś uwydatnia jej oddziaływanie na inne działy.'

Mazurkiewicz then discusses the significance and role of set theory within the theory of real functions, analysis, geometry and the foundations of mathematics. He stresses that the theory of functions of a real variable 'gave the first impulse to the creation of set theory, and today is predominantly a direct application of the latter' (1923, p. 90). He adds that 'in the theory of functions of real variable, set theory leads first of all to the systematisation of problems and provides a certain structure to the formless mass of details' (1923, p. 92). He also shows that investigations within functional calculus are essentially dependent on set theory, in particular the generalisations of the very concept of function are reliant on it. In geometry, set theory did not find—according to Mazurkiewicz—'a wider application and it is not going to find it' (1923, p. 97). Yet, we owe 'extreme enrichment of our knowledge of spatial forms' (1923, p. 97) to set theory.

This shows that the Warsaw School treated set theory as the basis of mathematics in the methodological sense and not the philosophical one (i.e. ontological and epistemological). Janiszewski's programme 'generated' the set-theoretic foundations of mathematics as a non-philosophical but mathematical direction. In the Warsaw School set theory was treated as a kind of auxiliary theory (though of fundamental significance) for mathematics. The team realised that set theory (like topology) was only developing and—as Mazurkiewicz put it in the quoted paper in *Poradnik*—was 'at the embryonic stage' (1923, p. 98). This fact 'very firmly counteracts the possibility of a wider application of set theory and topology to mathematics [...]' (1923, p. 98). However, 'as set theory moves forward its importance will undoubtedly increase' (1923, p. 98). Janiszewski expressed this belief in his conclusion in *Poradnik dla samouków*, focusing on the role of set theory as the new and universal language of mathematics, on the role of the axiomatic method as well as its affinity with logic. Whereas in his paper concerning set theory written for *Poradnik*, Sierpiński remarked:

> Despite the relatively short period (merely 40 years) set theory has managed to develop to an extraordinary extent and has occupied a first-rate position in mathematics. Today even a lecture on the foundations of higher mathematics cannot omit certain information from set theory (1915, p. 222).[25]

The treatment of set theory as the basis of mathematics in the methodological sense was expressed in the emphasis on its application in other branches of mathematics. For example, consider the fact that in *Fundamenta Mathematicae* there were relatively few papers dedicated to the 'internal' problems of set theory. Most papers showed the application of this theory to topology, the theory of functions or analysis.

Of special interest to our present discussion is the problem of the awareness of the relationships between set theory and logic and the foundations of mathematics

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[25] 'Pomimo stosunkowo krótkiego okresu czasu (zaledwie 40-letniego) teoria mnogości zdążyła już nadzwyczajnie się rozwinąć i zająć pierwszorzędne stanowisko w matematyce. Dzisiaj już nawet wykład podstaw matematyki wyższej nie może się obyć bez pewnych wiadomości z teorii mnogości.'

as well as the philosophy of mathematics in the Warsaw School. In the paper 'Teoria mnogości w stosunku do innych działów matematyki' (1923), volume 3 of *Poradnik dla samouków*, Mazurkiewicz refers to Janiszewski's paper 'Zagadnienia filozoficzne matematyki' from volume 1 of *Poradnik* (cf. Janiszewski 1915b) and stresses:

> [. . .] revealing certain contradictions, i.e. antinomies, within set theory has become one of the motifs to review the principles of formal logic [. . .] [and that] on the basis of the concept of a set there was an attempt (made by Peano's school and then by Russell and Whitehead) to pack the whole mathematics within the framework of one uniform hypothetical-deductive system; although the attempt was defective it was extremely interesting because of the tendencies to synthesis which it contained (1915b, p. 98).[26]

In the paper which Mazurkiewicz quoted, Janiszewski discusses the philosophical problems of set theory from the standpoint of the debate between realists and idealists, and formulates the conclusion that set theory is necessary for reflections on the philosophy of mathematics. He writes:

> In order to study the philosophy of mathematics one should know *well* set theory, arithmetic, the foundations of geometry and the basic concepts of infinitesimal analysis; then the knowledge of logistics is necessary; finally, general education in philosophy is needed (1915b, p. 486).[27]

Finally, a word must be said about another important characteristic of the Warsaw School of Mathematics, which has been already mentioned and will be

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[26] '[. . .] ujawnienie w łonie teorii mnogości pewnych sprzeczności, tj. antynomij, stało się jednym z motywów rewizji zasad logiki formalnej [. . .] [oraz że] na gruncie pojęcia zbioru podjęta została (przez szkołę Peany, a następnie przez Russella i Whiteheada) próba wtłoczenia całej matematyki w ramy jednolitego systemu hipotetyczno-dedukcyjnego, próba wprawdzie ułomna, jednak niezwykle interesująca z uwagi na tkwiące w niej tendencje do syntezy.'

[27] 'Do studiowania filozofii matematyki należy znać *dobrze* teorię mnogości, arytmetykę, podstawy geometrii i podstawowe pojęcia analizy nieskończonościowej; następnie konieczna jest znajomość logistyki; wreszcie potrzebne jest ogólne wykształcenie filozoficzne.'

It is worth quoting Janiszewski's further words concerning the necessary competences to exercise the philosophy of mathematics. He writes: 'It is not sufficient to be active in this field [philosophy of mathematics]; it is necessary to understand mathematics more profoundly, which can be expected only of those who have been creative in this field themselves. May the example of so many philosophers who, even having thorough mathematics education, have made mathematical mistakes in their works concerning the philosophy of mathematics and showed misunderstanding (though not ignorance!) of mathematics, be repellent here. Whereas the lack of philosophical education often makes mathematicians, dealing with these problems, misunderstand the philosophical aspects of these problems; they simply overlook numerous issues' (1915b, p. 486). ('Do czynnej jednak pracy na tym polu to nie wystarczy; koniecznym jest głębsze zrozumienie matematyki, czego można oczekiwać tylko od tych, którzy sami w tej dziedzinie pracowali w sposób twórczy. Niech przykład tylu filozofów, którzy, mając duże nawet wykształcenie matematyczne, popełnili w swych pracach nad filozofią matematyki błędy matematyczne i wykazali niezrozumienie (choć nie nieznajomość!) matematyki, działa tu odstraszająco. Brak znowu filozoficznego wykształcenia powoduje często u matematyków, zajmujących się temi zagadnieniami, niezrozumienie filozoficznej ich strony, przeoczenie po prostu całej masy zagadnień.')

referred further. The school did not favour any concrete philosophical doctrine within the philosophy of mathematics although the current concepts of the philosophy of mathematics were well-known there.[28] The only important thing was the correctness and fruitfulness of the methods applied. What was important was results and not concrete methods. It particular, this was expressed in research concerning the axiom of choice, which evoked numerous controversies. Some rejected it whereas others accepted it, recognising it as indispensible in mathematics. The Warsaw School took the stand that the mathematical implications of this axiom should be investigated and thus strict mathematical reflections should replace philosophical reflections. This attitude was clearly supported by Sierpiński, writing:

> Regardless of the fact whether we tend to accept Zermelo's axiom or not, we must take into account, in any case, its role in set theory and analysis. On the other hand, if Zermelo's axiom has been questioned by some mathematicians [. . .], it is important to know which theorems this axiom proves. (After all, if nobody questioned Zermelo's axiom it would be beneficial to analyse which proofs are based on this axiom—this is done, as one knows, for other axioms, too) (1923, p. 78).[29]

He repeated this opinion in his monograph, which was published a few decades later:

> Still, apart from our personal inclination to accept the axiom of choice, we must take into consideration, in any case, its role in set theory and in calculus. On the other hand, since the axiom of choice has been questioned by some mathematicians, it is important to know which theorems are proved with its aid and to realize the exact point at which the proof has been based on the axiom of choice; for it has frequently happened that various authors have made use of the axiom of choice in their proofs without being aware of it. And after all, even if no-one questioned the axiom of choice, it would not be without interest to investigate which proofs are based on it and which theorems are proved without its aid—this, as we know, is also done with regards to other axioms (1965, p. 95).

Therefore, the examination of the role of the axiom of choice in mathematics leads to solving the (philosophical) question of its validity. The axiom should not be assumed or rejected *a priori*; neither should its application be limited. But, putting aside personal philosophical convictions one should investigate (in a way impartially) which theorems, and how, depend on this controversial axiom. The same applies—*per analogiam*—to other axioms or hypotheses having a similar status (for example, the continuum hypothesis).

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[28] Let us add that Janiszewski himself said that he was rather a philosopher than a mathematician, and '[. . .] he deals with mathematics in order to become convinced how far the human mind can go by its logical thinking' (Steinhaus 1921).

[29] 'Niezależnie od tego, czy jesteśmy osobiście skłonni przyjąć pewnik Zermelo, czy też nie, musimy w każdym razie liczyć się z jego rolą w teorii mnogości i analizie. Z drugiej zaś strony, skoro pewnik Zermelo był kwestionowany przez niektórych matematyków [. . .], jest ważną rzeczą wiedzieć, jakie twierdzenia są dowodzone przy pomocy tego pewnika. (Zresztą nawet, gdyby nikt nie kwestionował pewnika Zermelo, nie byłoby rzeczą pozbawioną interesu badanie, jakie dowody opierają się na tym pewniku—co też robi się, jak wiadomo, i dla innych pewników).'

2.2 Lvov School of Mathematics: Steinhaus, Banach, Żyliński, Chwistek

This section presents Hugo Steinhaus, Stefan Banach, Eustachy Żyliński and Leon Chwistek—representatives of the Lvov School of Mathematics. Adopting the fundamental ideas of the programme formulated by Janiszewski the school specialised in other mathematical branches than the Warsaw School. While the Warsaw mathematicians dealt with set theory, topology and mathematical logic, the prevailing domain in Lvov was functional analysis initiated by Stefan Banach (whom Steinhaus discovered for mathematics), and developed by such figures as Steinhaus, Stanisław Mazur, Władysław Orlicz, Juliusz Schauder, Stefan Kaczmarz, Stanisław Ulam and Władysław Nikliborc. This field did not require such profound studies of logic and the foundations of mathematics as the fields pursued in Warsaw. Hence it is comparatively difficult to find remarks on mathematics as such—which this book focuses on—in the works of the Lvov mathematicians. This may have been caused by the fact that logic was not developed in Lvov, although its atmosphere favoured this field as well as the foundations of mathematics. It was only in the year 1928 that a chair of logic was created there. The chair was given to Leon Chwistek. Earlier Eustachy Żyliński had been the only Lvov mathematician who dealt with mathematical logic. However, it should be added that the other mathematicians of this environment did not disqualify the foundations of mathematics and logic. In fact, they 'casually' dealt with it. Let us mention Banach and his joint work with Tarski concerning the paradoxical decomposition of the sphere (1924) or the Banach-Mazur results on constructive methods in mathematics and computable analysis (cf. Mazur 1963).

Speaking of Banach, it is worth saying that he did not stand aloof from the philosophical environment in Lvov. In particular, in his *Dziennik* [Diary] (1997) Kazimierz Twardowski wrote that Banach had participated (on 7 March 1921) in the inaugural session of the Section of Epistemology of the Polish Philosophical Society (cf. 1997, vol. 1, p. 201) as well as in the session of the Society, held on 26 March 1927, during which Zygmunt Zawirski had lectured on the relation between logic and mathematics. Banach also took the floor in the discussion after the lecture (cf. 1997, vol. 1, p. 300). At the First Congress of Polish Mathematicians, held in Lvov in 1927, Banach gave a talk 'O pojęciu granicy' [On the concept of limit] (on 7 September 1927) at the meeting of the section of mathematical logic (cf. 1997, vol. 1, p. 323). In January 1923, Banach delivered a paper concerning paradoxes in mathematics during the session of the Polish Philosophical Society in Lvov. He spoke about the paradoxes related to the concept of the equipollency of certain sets (e.g. the set of whole numbers and the set of even numbers) as well as the problems of the Banach-Tarski paradox. He showed that the cause of these paradoxes were infinite sets and the axiom of choice, which were not formally inconsistent with set theory. In Banach's opinion, solving these apparent paradoxes required constructing a logical system that 'evokes no objections.' This remark characterises to some extent the Lvov mathematicians' attitude towards logic.

Banach did not see anything wrong in the fact that mathematical practice lacked a good logical system. In the Lvov School the cultivation of mathematics did not have to be completed with additional research on logic and the foundations of mathematics.

The picture of mathematics adopted in Lvov can best be reconstructed on the basis of certain remarks included in the works aiming at popularising mathematics, especially in Steinhaus's popular publications. We will also pay attention to several (unfortunately, separate) remarks of another representative of the Lvov environment, namely Eustachy Żyliński, regarding mathematics as a science. These remarks (when we have no systematic and complete testimonies) can give us a certain image of his views.

Reflecting on Steinhaus's philosophical views on mathematics we must first of all mention his popular book *Czem jest a czem nie jest matematyka* [What Is and What Is Not Mathematics] (1923). He presents numerous issues, especially the definition of mathematics, its historical development, practical applications, the method of mathematics, differential calculus and integral calculus, computational mathematics, errors in mathematics as well as the relations between mathematics and life. From our perspective his reflections on the definition of mathematics as a science and on mathematical methods are most interesting.

Trying to define mathematics as a science, Steinhaus stresses that it grew from certain practical needs of man but, in fact, it is a theoretical science. He writes:

> We can see that here we are dealing with an old, developing science, growing out of the background of practice and connected with the world of real applications, but a *theoretical* science, which does not avoid the biggest efforts even when dealing with some issues devoid of any utilitarian character, e.g. the quadrature of the circle (1923, p. 25).[30]

Mathematics is characterised by the use of the deductive method, but 'its axioms and definitions have the feature of randomness to a large extent' (1923, p. 25). Another characteristic, which differentiates it at face value, is the use of symbols, which on the one hand is necessary but on the other hand can lead to the so-called symbolmania (cf. Twardowski's work 'Symbolomania i pragmatofobia' [Symbolmania and Pragmatophobia], 1927), i.e. 'the mania of the mechanical use of symbols,' which 'contradicts mathematical psychology' (1923, p. 27).

Although Steinhaus had sympathy for logic, he did not see it as an independent discipline with its own research problems and methods, but as a tool of deduction. He gave this picture of logic in the booklet in question. Moreover, he describes it relatively late—only in the second part of the booklet, reflecting on the method of mathematics. This is how Steinhaus characterises it:

> Mathematics aims at discovering absolutely true theorems. In order to do that it uses the method called *deductive*. In other words, it formulates new theorems from those that it has

[30] 'Widzimy, że mamy tu do czynienia z nauka starą, rozwijającą się, wyrosłą na podłożu praktyki i związaną ze światem zastosowań realnych, ale nauką *teoretyczną*, nie uchylającą się przed największymi wysiłkami nawet wtedy, gdy chodzi o zagadnienia zupełnie pozbawione utylitarnego charakteru, jak np. kwadratura koła.'

> made sure to be sufficient, using the *logical* way, i.e. correct deduction without references to observation, experiment, the testimony of the senses or spatial outlook as well as to vision, revelations or authority (1923, p. 74).[31]

The deductive method in some sense determines the object of mathematics. Steinhaus writes:

> Therefore, we can see that mathematics has its object determined only by the method and that every deductive theory is mathematics; that after all, this description of mathematics is just a framework that will be filled only after mathematical axioms are introduced, and they are—to some extent—arbitrary (1923, p. 78).[32]

And he adds:

> A characteristic feature of mathematics is its method. The mathematical method is deductive, synthetic and formal (1923, p. 80).[33]

The deductivity of the method of mathematics consists in the fact that 'the only means which the mathematical reasoning uses is deduction' (1923, p. 80). In Steinhaus's opinion the regularity of the method of mathematics is revealed in the choice of axioms, assuming that axioms can be both mathematical and logical. Choosing the latter 'is not done on the logical way but by virtue of the verdict of another instance, which some call "intuition" and others "feeling of certainty"' (1923, p. 81).

In mathematics definitions serve to shorten statements. However, 'the choice of definition determines the direction of our development of mathematics, i.e. which combinations of symbols we will recognise as important and worth separate shortening' (1923, p. 81).

The feature of formality is that in mathematical reasoning one can consider only such content of concepts that has been included in definitions. Steinhaus writes:

> The formalism of the mathematical method consists in that any content of the considered concepts is excluded in case someone wanted to assign them some nondefinitional content, and all that is contained in the very sound of words and is not clearly visible in the definitional agreement is as far as possible rejected from the definition (1923, p. 81).[34]

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[31] 'Matematyka stawia sobie za cel wykrywanie teorematów absolutnie prawdziwych. Do tego celu używa metody zwanej *dedukcyjną*. Innymi słowy wysuwa ona z teorematów, co do których już upewniła się dostatecznie, nowe, drogą *logiczną*, tj. drogą poprawnego wnioskowania bez odwoływania się do obserwacji, do eksperymentu, do świadectwa zmysłów lub też oglądu przestrzennego, czy też do wizji, objawień albo autorytetu.'

[32] 'Widzimy więc, że matematyka ma swój przedmiot określony tylko przez metodę i że jest matematyką każda teoria dedukcyjna, że jednak to określenie matematyki jest tylko ramą, która zostaje wypełniona dopiero po wprowadzeniu pewników matematycznych, a one są—do pewnego stopnia—dowolne.'

[33] 'Charakterystyczną cechą matematyki jest jej metoda. Metoda matematyczna jest dedukcyjna, syntetyczna i formalna.'

[34] 'Formalizm metody matematycznej polega na tym, że wyklucza się z rozumowań matematyki wszelką treść pojęć rozważanych, o ile by ktoś chciał im przypisać jakąś treść pozadefinicyjną, a z definicji odrzuca się o ile możności wszystko, co mieści się w samym dźwięku wyrazów a nie jest wyraźnie uwidocznione w umowie definicyjnej.'

The only utilitarian and 'instrumental' character of logic towards mathematics is stressed in the following statement of Steinhaus:

> The teaching of formal logic finds in mathematics the most beautiful field for exercises and examples (1923, p. 169).[35]

Apart from the above-mentioned characteristics the aesthetic element plays an important role in the development of mathematics.[36] In Steinhaus's opinion beautiful is 'what is understandable, what is sufficiently general to be applied to the known, and not *ad hoc* formulated examples, and at the same time not so general to be trivial' (1958, p. 43). In fact, there are no absolute criteria of beauty but the sense of beauty and drive for beauty 'influence the direction of mathematical investigations more strongly than the principle of perfect strictness' (1958, p. 44). In the paper 'Drogi matematyki stosowanej' [Ways of applied mathematics] he wrote:

> In the mathematician's soul, like in any other man's soul, there are various beliefs and passions, aversions and cults, superstitions and inclinations. The strongest of these feelings and the most respectable one is sensitivity to the beauty of mathematics. Not everyone can see the beauty of the mountains. Not everyone has been moved by the view of the sea, and the stars do not appeal to all people; it cannot be explained but it is even more difficult to explain what the beauty of a function of complex variable or of synthetic geometry is (1949, p. 11).[37]

Steinhaus valued applied mathematics and the applications of mathematics very much—in fact, he was fairly successful in this domain. He thought that the Platonic approach to mathematics interfered with the interest and involvement in its applications. In 'Drogi matematyki stosowanej' he wrote that this attitude 'is not only hostile to applied mathematics but also destroys all natural sciences' (1949, p. 11). Since he never defined clearly the connection of mathematical concepts and objects to reality experienced through the senses, we must content ourselves with his short aphoristic but beautiful and apt remark:

> Mathematics mediates between spirit and matter[38] (1980, p. 54).

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[35] 'Nauka logiki formalnej znajduje w matematyce najpiękniejsze pole do ćwiczeń i przykładów.'

[36] Many authors writing about mathematics paid attention to this problem. Suffice it to mention Aristotle, Proclus or Poincaré. In particular, in his *Metaphysics* (book 3, 1078a52–1078b4) Aristotle writes that mathematics speaks, though not necessarily *explicite*, about beauty and reveals elements of beauty; moreover: beauty is one of the motive powers of this science. In the fifth century the Neo-Platonic philosopher Proclus Diadochus used similar words in *A Commentary on the First Book of Euclid's Elements*. And so did Henri Poincaré, living in the nineteenth century, in his work *Science et méthode*.

[37] 'W duszy matematyka, jak każdego człowieka, tkwią różne wierzenia i zamiłowania, awersje i kulty, przesądy i upodobania. Najsilniejszym z tych uczuć i najgodniejszym szacunku jest czułość na piękno matematyki. Nie każdy widzi piękno gór, nie każdy doznał wzruszenia na widok morza i nie do każdego przemawiają gwiazdy w nocy; tłumaczyć tego nie można, a jeszcze trudniej jest wyjaśnić, w czym tkwi piękno funkcji zmiennej zespolonej lub geometrii syntetycznej.'

[38] 'Między duchem i materią pośredniczy matematyka.' These words of Steinhaus were inscribed on his tombstone.

The importance which Steinhaus attributed to mathematics is also testified by the following remark made at the end of his booklet *Czem jest a czem nie jest matematyka*:

> No other science than mathematics strengthens so much our faith in the power of the human mind. The possibility to prove every theorem excludes all phraseology. In this autonomy from platitude, authority, in this independence of results from researchers' wishes and 'points of view' one can see both the scientific and pedagogical value of this science. If one can use the term 'mental health' mathematics can boast of playing the most positive role in 'mental hygiene' (1923, p. 169).[39]

Speaking of the philosophy of mathematics and logic in the context of the Lvov School of Mathematics it is worth mentioning—apart from Steinhaus—the figure of Eustachy Żyliński. He dealt mainly with the theory of numbers, but after 1919 his interests included algebra, logic and the foundations of mathematics. In particular, he proved (cf. 1925 and 1927) that in classical bivalent logic the only two-argument logical functions which are sufficient to define all the one and two-argument logical functions are binegation and disjunction (the Sheffer stroke). As for the problems related to the philosophy of logic and mathematics there are no separate works written by Żyliński. Although he wrote the large work *Formalizm Hilberta* [Hilbert's Formalism] (1935), he did not include any philosophical remarks regarding Hilbert's programme—contrary to what the title might suggest—but aimed at 'elaborating and giving a detailed presentation of certain formalism, on which the works of Hilbert and his school concerning the foundations of mathematics are based' (1935, Introduction, p. 1). Żyliński focuses on technical issues, especially set theory and the logic of sentences. He also announced that he would 'work on the extension of formalism H_1 embracing the foundations of arithmetic and mathematical concept of function in its applications' (1935, p. 2). However, this work has never been published. It is worth noting that the author understood the discussed set theory and the logic of propositions 'as separate disciplines' (1935, p. 2).

Żyliński's other works include several brief statements of a philosophical character. As they are just a few it is worth analysing them. In his own abstract of the lecture entitled 'O przedmiocie i metodach matematyki współczesnej' [On the Subject and Methods of Modern Mathematics], delivered on 21 May 1921 (1921–1922), he explained what mathematical theories were. He claimed that they could be identified with a set of consequences of the accepted axioms. We can read:

> A particular 'mathematical theory' can be recognised as a set of conclusions that 'can' be obtained through basic thoughts connected with the sense of certainty, applied to basic variety on the sub-varieties of which certain initial properties (axioms) are projected (1921–1922, p. 71a–71b).[40]

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[39] 'Żadna nauka nie wzmacnia tak wiary w potęgę umysłu ludzkiego jak matematyka. Możność udowodnienia każdego teorematu wyklucza wszelką frazeologię. W tej niezależności od frazesu, od autorytetu, w tej niezawisłości rezultatu od życzenia badacza i od "punktu widzenia", upatruję nie tylko naukową, ale i pedagogiczną wartość tej nauki. Jeśli wolno użyć pojęcia "zdrowie umysłowe", to matematyce przypada najdodatniejsza rola w "umysłowej higienie".'

[40] 'Poszczególna "teoria matematyczna" uważana być może za zbiór wniosków, które "mogą" być otrzymane za pomocą podstawowych pomyśleń mnogościowych połączonych z uczuciem

One should note his rather imprecise understanding of logic referring to some subjective sense of obviousness and certainty rather than to the formally *a priori* rules of inference. Żyliński also allows an infinite set of consequences of accepted axioms while speaking of conclusions that *can* be drawn.

Referring to the mutual relation between logic and mathematics he states:

> From this point of view the relation between mathematics and extensional logic would present itself to some extent as a relation between special set theories and the general set theory (1921–1922, p. 71b).[41]

Assuming that the concept of object is 'the simplest natural concept' (1921–1922, p. 71b), he claims that mathematics is a natural science of objects. He strengthens his thesis, stressing that '[in] investigations of particular mathematical theories (e.g. number theory) we use observation and even experiment' (1921–1922, p. 71b).

In his work 'Z zagadnień matematyki. II. O podstawach matematyki' [Mathematical Problems. II. About the Foundations of Mathematics] (1928), Żyliński discusses the role of intuition in mathematics. He emphasises that intuition can help construct a proof but in no way can the proof itself refer to intuition:

> In mathematics intuition can direct a proof successfully but in no case can it be its ingredient (1928, p. 51).[42]

Consequently, we are dealing with a clear differentiation between the context of discovery and the context of justification. The former allows intuition and the latter does not.

Żyliński's works also embrace several statements on the role and significance of mathematics for other sciences and, broadly, the world of culture. In his abstract 'O przedmiocie i metodach matematyki współczesnej' he claims that 'a strictly synthetic exposition of every science consists in a certain mathematical theory, the theorems of which are binding in this science' (1921–1922, p. 71b). In the quoted publication 'Z zagadnień matematyki. II. O podstawach matematyki' he writes:

> The birth of mathematics is at the same time the birth of mankind's culture. [...] Together with the development of intellectual culture, geometry and arithmetic, apart from a purely practical life meaning, begin drawing minds thanks to the simple and distinct laws occurring in their area (1928, p. 42).[43]

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pewności, stosowanych do mnogości podstawowej, na której podmnogości nałożone są pewne własności początkowe (aksjomaty).'

[41] 'Z tego punktu widzenia stosunek matematyki do logiki zakresowej przedstawiałby się w pewnym stopniu jako stosunek specjalnych teorii mnogości do ogólnej.'

[42] 'Intuicja w matematyce może z pożytkiem kierować dowodem, lecz w żadnym razie nie może być jego częścią składową.'

[43] 'Narodziny matematyki są jednoczesne z narodzinami kultury ludzkości. [...] Wraz z rozwojem kultury intelektualnej geometria i arytmetyka, poza swym czysto praktycznym życiowym znaczeniem, zaczynają pociągać umysły dzięki wyjątkowo prostym i wyraźnym prawom występującym na ich terenie.'

On the other hand, in the memorial signed by Żyliński, Steinhaus, Ruziewicz and Banach on 14 April 1924 we can read:

> Today's mathematics is nothing else but a general theory of exact thinking connected with the feeling of certainty. [...] However, being the most general science of relations existing between objects mathematics is applied to every scientific and practical field, exceeding sufficiently enough the framework of descriptiveness, simple inductions or literary-artistic methods (Żyliński et al. 1924, p. 1).[44]

Therefore, it is an explicit statement concerning the object of mathematics and—being its consequence—explanation of the applicability of mathematics in other fields.

This chapter also presents Leon Chwistek's views on the philosophy of mathematics and logic. As explained in the introduction, he began his scientific career in Cracow but from 1930 he was a professor of mathematical logic at the Jan Kazimierz University in Lvov. There he developed his concepts and tried to create a school. No wonder, then, that his philosophical views on mathematics and logic are worth discussing in this chapter.

Although Chwistek's first works concerned experimental psychology, he is predominantly known for his treatises on logic. Chwistek, like some Polish logicians (e.g. Leśniewski, cf. Sect. 3.4 in Chap. 3), expressed his philosophical views while building and interpreting logical theories. Moreover, his research on logic was motivated to a large extent by his philosophical views. By creating semantics he wanted to overcome philosophical idealism and opposed the conception of absolute truth. He was not satisfied with solving concrete fragmentary problems (neither was Leśniewski) but he strove to formulate a system embracing mathematics as a whole.

Chwistek's interest in logic began during his studies in Göttingen, especially after he had listened to Poincaré's lecture delivered in the spring of 1909. Chwistek decided to combine the ideas of Russell and Poincaré and to reform the theory of logical types by omitting impredicative definitions. He began by criticising the system of the ramified theory of types formulated by Whitehead and Russell in *Principia Mathematica* (1910–1913). It mainly concerned the principle of reducibility: every sentential function has an equivalent sentential function of the same type and the first order (the so-called quantifier-free), which allowed the elimination of impredicative definitions. However, this principle is of a non-constructive character and thus it introduces—according to Chwistek—ideal objects. It constitutes a typical axiom of existence and, in Chwistek's opinion, in the deductive system one should not make any other presumptions than the principle of sense and deductive rules.

[44] 'Matematyka dzisiejsza jest niczym innym jak ogólną teorią ścisłego myślenia połączonego z uczuciem pewności. [. . .] Będąc jednak najogólniejszą nauką o relacjach zachodzących między przedmiotami, matematyka znajduje zastosowania w każdej dziedzinie naukowej i praktycznej, wychodzącej w dostatecznej mierze poza ramy opisowości, prostych indukcji lub metod literacko-artystycznych.'

Therefore, Chwistek attempted to reconstruct the system of Whitehead-Russell, and he did so in the nominalistic spirit. He formulated a certain version of the simple (simplified) theory of types.[45] He presented its foundations in the works: 'Antynomie logiki formalnej' [The Antinomies of Formal Logic] (1921a), 'Zasady czystej teorii typów' [The Principles of the Pure Theory of Types] (1922a) and 'Über die Antinomien der Prinzipien der Mathematik' (1922b). In the simple theory of types one can distinguish types of functions but not their orders. This theory allows one to eliminate only logical antinomies (the set-theoretic ones)—and so did the ramified theory of types formulated by Whitehead-Russell—but it does not remove semantic antimonies in the style of Richard's antinomy. Then Chwistek formulated a pure theory of logical types—theory of constructive types (cf. 1924 and 1925). Among other things it rejects the axiom of reducibility. Yet, it leads to certain big formal complications of logical systems (especially the theory of classes and the theory of cardinal numbers) resulting from the necessity of considering not only the types but also the orders of sentential functions (which now cannot be reduced to the lowest order). Thus it removes non-constructible objects at the cost of increasing the degree of the formal complication of the system.

The described investigations led Chwistek to create a complete theory of expressions and to rational metamathematics that was based on it. It was to be a more basic system than logic and it would make it possible to reconstruct the classical logical calculus and the whole of Cantor's set theory. It would also meet the nominalist assumptions, and therefore it would be free from existential axioms, mainly the axiom of reducibility and the axiom of choice. Chwistek's system was based on the assumption that its theorems and consequently, the theorems of classical logic and set theory reconstructed in it, refer only to the inscriptions which can be obtained in a finite number of steps with the help of a pre-established rule of construction, and not to what these inscriptions mean. At the same time, these inscriptions are understood as physical objects. Realising his programme, Chwistek approached the version of nominalism, which can be seen in the early works of Willard Van Orman Quine and Nelson Goodman.

We will return to Chwistek's nominalism further in the book. Now let us just state that his conceptions neither won widespread recognition nor played a big role in the development of logic. One can see the reasons for that in his symbolism, which was complicated, unclear and difficult to decipher, as well as in his rather

[45] This theory did not reach international logicians, and independently from Chwistek it was again formulated by F.P. Ramsey in 1925. In his introduction to the second edition of *Principia Mathematica* Russell paid tribute to Chwistek's conceptions. At the same time he paid attention to the costs they involved—they lead to the necessity of abandoning many important parts of mathematics. He wrote (cf. Whitehead, Russell 1925–1927, vol. 1, p. XIV): 'Dr Leon Chwistek [in his *Theory of Constructive Types*—Russell and Whitehead's footnote] took the heroic course of dispensing with the axiom without adopting any substitute; from his work, it is clear that this course compels us to sacrifice a great deal of ordinary mathematics.' Cf. the correspondence between Chwistek and Russell in Jadacki (1986). The reasons for the poor reception of Chwistek's works and results will be discussed later.

illegible and careless way of presenting results, in particular the lack of proper examples, especially at places that can raise the most serious doubts, which Chwistek replaced with such phrases as 'it is easy to see that'—all of that made it difficult to understand his proposals and evaluate their worth. In his dissertations he often referred to other works spread in various periodicals and thus difficult to access. Another obstacle could have been the fact that Chwistek treated his results concerning the foundations of mathematics as an argument in favour of his diverse philosophical questions.

One can see a certain interest in the philosopher's logical works after the year 1945, when there was greater curiosity in nominalism in the philosophy of mathematics.[46] His system of rational metamathematics was not sufficiently elaborated. Moreover, his collaborators (Jan Herzberg, Władysław Hetper and Jan Skarżyński) and disciples (Wolf Ascherdorf, Celina Gildner, Kamila Kopelman, Abraham Melamid, Józef Pepis and Kamila Waltuch) could not do it because all of them lost their lives during World War II. Chwistek went his own way and his research on logic did not follow the main trend of the historical development of logic. Like Leśniewski, he worked with just a few people and, for example, he did not collaborate with mathematicians. He did not have any close scientific contacts with the Lvov philosophers, either.

Let us present in detail Chwistek's philosophical views related to logic and mathematics. We will focus on his judgements regarding the methodology of deductive sciences, which he put forward in *Granice nauki* [The Limits of Science] (1935), bearing the subtitle *Zarys logiki i metodologii nauk ścisłych* [Outline of Logic and Methodology of Exact Sciences].

According to Chwistek, human knowledge is neither complete nor absolute. It cannot be complete because the theorems concerning all objects lead to contradiction. It cannot be absolute since there is no one absolute reality. He wrote in *Granice nauki*:

> It follows from these considerations that the principle of contradiction does not permit complete knowledge, i.e. knowledge which includes the answer to all questions. The attempt to secure such knowledge will sooner or later conflict with sound reason (1948, p. 42).[47]

In his opinion, sound reason—besides acknowledging experience as a fundamental source of knowledge and besides the necessity to schematise cognised objects or phenomena—is a common factor of the whole correct process of cognition. It lies in the rejection of all assumptions which are not experimentally

[46] In the years 1950–1951 J.R. Myhill published a series of papers dedicated to the search for possibilities for using Chwistek's systems of rational metamathematics in the proof of the consistency of set theory as presented by Bourbaki. Cf. Myhill (1950, 1951a, b). Let us add that 'The Theory of Constructive Types' by Chwistek was reprinted by the University of Michigan in the series *The Michigan Historical Reprints Series*—cf. Chwistek (1988).

[47] 'Z rozważań tych wynika, że zasada sprzeczności wyklucza wiedzę pełną, dającą odpowiedź na wszystkie pytania. Dążenie do takiej wiedzy musi—czy prędzej, czy później—doprowadzić do kolizji ze zdrowym rozsądkiem' (1935, p. 20; see also 1963, p. 17).

verifiable or are inconsistent with experience or are not based on reliable theorems concerning simple facts or cannot be reduced logically to such theorems. Both empirical and deductive knowledge is relative. The former is relative because there are various types of experience, matching various realities, and the latter—because it depends on the accepted system of concepts. Here Chwistek speaks about rational relativism.

He assumed the principle of rationalism of cognition and firmly opposed irrationalism. Rationalism lies in there being only two sources of knowledge, namely experience and exact reasoning. This concerns mathematics and exact sciences as well as empirical sciences and philosophy. He wrote in *Granice nauki*:

> [. . .] the point of departure in constructing our world view should not be a confused metaphysics, but simple and clear truths based on experience and exact reasoning (1948, p. 3).[48]

Therefore, he opposes irrationalism, metaphysics and idealism in philosophy and mathematics.[49] He roundly criticises Plato, Hegel, Husserl and Bergson. Seeing the errors of positivism he values its epistemological concepts. Let us add that Chwistek very much appreciated dialectical materialism, ignoring the fundamental contrasts between dialectical materialism and positivism. He describes his views concerning cognition as critical rationalism and contrasts it with dogmatic rationalism.[50]

Formal logic, and in particular Chwistek's rational metamathematics, is to be both a solution to the difficulties caused by irrationalism and a weapon to struggle with it. Chwistek begins his introduction to *Granice nauki* with the phrase 'Having experienced a period of unparalleled growth of irrationalism' (1935, p. III), and ends with the words, 'History teaches that the final victory has always been shared by nations that followed the principles of exact reasoning.' He also writes:

> When this new system [i.e. the system of rational metamathematics—remark is mine] is completely worked out, we will be able to say, that we have at our disposal an infallible apparatus which sets off exact thought from other forms of thought (1948, p. 22).[51]

Chwistek's epistemological views are close to neo-positivism. He claims that the object of scientific cognition can be only what is or can be given in experience, i.e. what we can see through our senses, possibly supported by tools:

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[48] '[. . .] punktem wyjścia budowy naszego poglądu na świat nie powinny być męty metafizyczne, ale prawdy proste i jasne, oparte na doświadczeniu i ścisłym rozumowaniu' (1935, p. V).

[49] Chwistek rejects irrationalism and idealism not only as false philosophical theories but also because they are, in his opinion, the sources of human sufferings, social injustice, cruelty and wars.

[50] It is of interest to note that a certain difficulty in interpreting Chwistek's views is the fact that he often uses classical philosophical terms, giving them specific meanings, which he does not explain at all or explains insufficiently.

[51] 'Z chwilą, kiedy system ten zostanie wykończony, będzie nam wolno twierdzić, że rozporządzamy niezawodnym aparatem, oddzielającym myślenie ścisłe od innych form myślenia' (1935, p. XXIV).

> [...] in speaking about reality we have in mind not some ideal object but the patterns which must be employed in dealing with a given case (1948, p. 261).[52]

Chwistek recommended using the constructive method in both science and philosophy. He formulated it in 'Zastosowanie metody konstrukcyjnej do teorii poznania' [The Application of the Constructive Method to Epistemology] (1923). Although one can refer this way mainly to deductive sciences it is also applied in empirical sciences and philosophy. The analysis of intuitional concepts in a given discipline lies in the foundations of the constructive method. It allows the separation of primitive concepts, the meaning of which is characterised in axioms. Then using the laws of logic, (formal) theorems are formulated in the axioms. Later Chwistek concluded that constructing deductive systems on the basis of philosophy is pointless—such a system cannot be built due to the degree of the complexity of philosophical investigations.

As aforementioned, in Chwistek's opinion the object of knowledge can be only what is given in experience. However, we are dealing with various kinds of experience. Thus we reach Chwistek's most known and original philosophical concept, namely his theory of plurality of realities.[53] He first formulated it in the paper 'Trzy odczyty odnoszące się do pojęcia istnienia' [Three Lectures Concerning the Concept of Existence] (1917), stating that 'intuitive faith in one reality seems a prejudice' (1917, p. 145) and seeing the concept of plurality of realities in Pascal and Mach (cf. 1917, pp. 149–150). He developed his theory in the book *Wielość rzeczywistości* [The Plurality of Realities] (1921b), its final version being in *Granice nauki* (1935). He framed its foundations again in the English edition—*The Limits of Science*—published in 1948, i.e. after his death, but this version does not include anything new.

In his first period (before 1925), Chwistek differentiates between the meaning of the term 'reality' and the meaning of 'existence.' In his opinion the latter is of a more general character since it can concern both the objects of reality and abstract objects, such as the objects of mathematics:

> Assuming that all that exists is real, we had to regard mathematical relations and elements of experience as real (1917, p. 145).[54]

In 'Trzy odczyty odnoszące się do pojęcia istnieni' (1917), Chwistek identified three standpoints relating to existence: nominalism, realism and hyperrealism. According to him, nominalists 'demand verbal definitions, excluding contradiction' whereas realists 'go without verbal definitions but exclude contradictory objects' and hyperrealists 'go without verbal definitions and do not exclude contradictory objects' (1917, p. 126).

[52] '[...] jeśli mówimy o rzeczywistości, to nie mamy na myśli jakiegoś idealnego obiektu, tylko te schematy, z jakimi w danym wypadku mamy do czynienia' (1935, p. 229; see also 1963, p. 205).

[53] This theory is sometimes compared and juxtaposed with Popper's conception of three worlds.

[54] 'Gdybyśmy założyli, że wszystko, co istnieje, jest rzeczywiste, to musielibyśmy uznać za rzeczywiste stosunki matematyczne wraz z elementami doświadczenia.'

At first, Chwistek accepted only two realities and tried to formalise his own theory. In *Granice nauki* he gives up his attempts to formalise it and accepts four kinds of reality respectively to possible kinds of experience. Thus we have the reality of sensations, the reality of images, the reality of things (reality of everyday life) and physical reality (constructed in exact sciences). Simultaneously, he attributes separate existence and full theoretical equality to the particular realities.

Having briefly discussed Chwistek's general methodological and ontological conceptions we can proceed to his views concerning the philosophy of mathematics (although we have already mentioned some of his views on mathematics, which lie at the source of his logical concepts). His firm nominalistic standpoint came to prominence here.

Therefore, in Chwistek's opinion the object of deductive sciences, including mathematics, is expressions constructed in these sciences in accordance with their rules of construction. Consequently, the object of mathematics is not ideal objects, such as points, straight lines, numbers or sets. Here the expressions being the object of mathematics are physical objects that we are given in experience. They can be transformed according to the adopted rules. Any system approves rules and certain expressions that play the role of axioms from which theorems are deduced out. The rules of transformation and axioms are chosen in such a way that the expressions could be interpreted as descriptions of the analysed state of affairs. In order to be able to apply deductive theories to specific sciences and more generally, to perceive concrete areas of reality, the elements of the latter should be schematised.

According to Chwistek, geometry is an experimental science. In Chap. VIII of *Granice nauki* he writes:

> Geometry is an experimental science. It depends upon the measurement of segments, angles, and areas. The Egyptians conceived it in this way and it has remained essentially the same up to this very day. Today what is generally regarded as geometry, i.e. what is included in textbooks, is the peculiar mixture of experimental geometry and the geometrical metaphysics which was inherited from the Greeks as Euclid's *Elements* (1948, p. 170).[55]

The rise of the systems of non-Euclidean geometry of Bolyai, Gauss and Lobachevsky in the nineteenth century, which Chwistek regards as the most important achievement in exact sciences, abolished—in his opinion—Kant's idealism.[56] These geometries showed that, for example, the concept of a straight line is

[55] 'Geometria jest nauką doświadczalną. Polega ona na mierzeniu odcinków, kątów i powierzchni. Tak pojmowali ją Egipcjanie i taką pozostała w istocie swojej do dzisiaj. To, co uważa się powszechnie za geometrię za naszych czasów, tj. to, o czym pisze się w podręcznikach, jest osobliwą mieszaniną geometrii doświadczalnej i metafizyki geometrycznej, którą pozostawili nam w spadku Grecy pod postacią *Elementów* Euklidesa' (1935, p. 190; see also 1963, p. 170).

[56] The thesis that non-Euclidean geometries refuted Kant's philosophy of geometry seems not to be fully justified. Now, if we take into consideration that Kant distinguished between postulating the existence of an object and its construction this thesis is not valid, since postulating existence requires only the inner consistency of a given concept, and construction assumes a certain structure of perceptual space. So one can postulate the existence of a five-dimensional sphere since the

not of an objective character, but depends on the accepted axioms. It may suggest that conventionalism is the proper philosophy for geometry. Indeed, in his first works, e.g. the quoted paper 'Trzy odczyty odnoszące się do pojęcia istnienia' (1917), he states that the existence of systems of non-Euclidean geometry, which are consistent, refutes the thesis of the *a priori* character of geometry. It seems that he would tend to accept conventionalism, although he does not state this explicitly:

> Both systems [of Euclidean geometry and non-Euclidean geometry—remark is mine] are free from contradiction since they can be reduced to analytic geometry; thus they do not show any fundamental differences from the theoretical standpoint. Intuition reconciles easily with Lobachevsky's theorems, which seem paradoxical only at first sight [. . .]. Therefore, we reach the conclusion that both geometries are equally true since each refers to different straight lines; only the differences between both kinds of these lines cannot be formulated with the help of experimental and intuitional means so that a segment of a straight line, which we will draw or think of, can serve as an illustration of the first or the second kind, depending on our will (1917, pp. 144–145).[57]

However, in *Granice nauki* Chwistek categorically rejected conventionalism, stating that geometry—like all other fundamental experimental sciences –should be based on the theory of expressions. This is because conventionalism introduces hypothetical entities, as was the case in John Stuart Mill's works or later Poincaré's, a promoter of this direction.[58] Chwistek wrote:

> It seems that it is impossible to attain a general concept of geometry without using formulae. It is therefore clear that the conception of geometry as the science of ideal spatial constructions must be nullified. [. . .] To speak of different four-dimensional space-times it is necessary to employ five-dimensional spacetime. It is clear that all this has only as much meaning as do mathematical formulae (1948, pp. 186–187).[59]

concept is consistent but it cannot be constructed since the perceptual space is three-dimensional. Kant stated nothing to contradict the possibility of constructing consistent systems of geometries other than the Euclidean one.

[57] 'Obydwa systemy [tzn. system geometrii euklidesowej i systemy geometrii nieeuklidesowej] są wolne od sprzeczności, można je bowiem sprowadzić do geometrii analitycznej, nie wykazują więc zasadniczych różnic z punktu widzenia teoretycznego. Intuicja godzi się z łatwością z twierdzeniami Łobaczewskiego, które tylko na pierwszy rzut oka wydają się paradoksalne [. . .]. Dochodzimy więc do wniosku, że obydwie geometrie są w równym stopniu prawdziwe, każda z nich bowiem odnosi się do innych linii prostych; tylko różnice pomiędzy obydwoma gatunkami tych linii prostych nie dadzą się uchwycić przy pomocy środków doświadczalnych ani intuicyjnych, tak że kawałek linii prostej, który narysujemy lub pomyślimy sobie, może służyć za ilustrację jednego lub drugiego gatunku zależnie od naszej woli.'

[58] Let us add that in Chwistek's opinion conventionalism also became a source of reactionary social views, reducing truth and truthfulness to effectiveness, thus leading to the strengthening of the ruling classes: 'It is good to see that idealism dressed in the feathers of conventionalism has become a tool in the hands of reactionary elements that is even more dangerous than the old dogmatic idealism' (1935, p. 186).

[59] 'Okazuje się, że dotarcie do ogólnego pojęcia geometrii bez formuł jest niemożliwe. Jasne jest, że idąc tą drogą, musimy dojść do unicestwienia geometrii jako nauki o idealnych utworach przestrzennych. [. . .] Żeby mówić o różnych czterowymiarowych czasoprzestrzeniach, musimy się odwołać do czasoprzestrzeni pięciowymiarowej. Jest jasne, że wszystko to ma tyle sensu, ile zawierają go formuły matematyczne' (1935, pp. 186–187).

According to Chwistek, arithmetic, mathematical analysis and other mathematical theories should be treated like geometry, thus consequently obtaining their nominalistic interpretations.

Chwistek's philosophical conceptions shared the fate of his logical theories (as mentioned earlier). Chwistek was alone in his search. His ideas were often criticised bitterly, as he himself wrote in *Zagadnienia kultury duchowej w Polsce* [Issues of Spiritual Culture in Poland]:

> [. . .] the spheres of professional philosophers reacted to the idea of the plurality of realities either with disrespect or with unparalleled indignation, verging on fierce rage (1933; cf. 1961, p. 203).[60]

What were the reasons for these reactions? Chwistek's philosophical investigations were not of a systematic character and he seemed not to treat them with full responsibility (as Pasenkiewicz wrote in *Przedmowa* [Foreword] to Chwistek's selected works, cf. 1961, p. VII). He did not explain many of the terms he used, and his conceptions 'had been announced before they were verified' (1961, p. VII).

Finally, despite the described circumstances there have been references to Chwistek and citations from his works. For example, the Australian philosopher Richard Sylvan refers to Chwistek's pluralism in his book *Transcendental Metaphysics* (1997).

[60] '[. . .] sfery zawodowych filozofów zareagowały na ideę wielości rzeczywistości już to jej lekceważeniem, już to bezprzykładnym oburzeniem, graniczącym z dziką wściekłością.'

Chapter 3
Lvov-Warsaw School of Philosophy

This chapter presents and analyses the philosophical views on mathematics and logic formulated by representatives of the Lvov-Warsaw School of Philosophy, in particular the scientists belonging to the so-called Warsaw School of Logic. The discussed figures include: Kazimierz Twardowski, Jan Łukasiewicz, Stanisław Leśniewski, Kazimierz Ajdukiewicz and Tadeusz Kotarbiński as well as Alfred Tarski and Maria Kokoszyńska. We have also added Zygmunt Zawirski, who worked in Lvov, Poznań and Cracow but all the time he was connected with the Lvov-Warsaw School (cf. Introduction). We will also consider the ideas of two other scientists, who have usually been recognised as the second generation of the Lvov-Warsaw School, namely Andrzej Mostowski and Henryk Mehlberg. Moreover, we will present the views on logic and mathematics held by the members of the so-called Cracow Circle: Fr Józef (Innocenty) Bocheński, Jan Drewnowski and Rev. Jan Salamucha.

3.1 Kazimierz Twardowski

Kazimierz Twardowski is primarily known as an organiser of science and the creator of the Lvov-Warsaw School of Philosophy. However, he was the author of numerous important and original scientific views, too. He also made better formulations of the existing views, contributing to their fuller understanding. He belonged to the School of Brentano; he was Brentano's disciple and outstanding continuator. One should add that he created modern Polish philosophical terminology. As Woleński writes:

> In the elegant language of Tatarkiewicz and in Łukasiewicz's comments on logical formulas, in the dry and economic style of Ajdukiewicz and Czeżowski, and in the flowing archaic metaphors of Kotarbiński, in Witwicki's translations from Plato, we can see a common source: the language of Kazimierz Twardowski (1989, p. 53).

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6_3

Of our interest are Twardowski's ideas and concepts concerning the methodology and theory of science, in particular his view on the distinction between *a priori* sciences and *a posteriori* sciences as well as his defence of the absolute understanding of the concept of truth.

The first problem was presented in his work 'O naukach apriorycznych, czyli racjonalnych (dedukcyjnych), i naukach aposterioryczynych, czyli empirycznych (indukcyjnych)' [*A Priori*, or Rational (Deductive) Sciences and *A Posteriori*, or Empirical (Inductive) Sciences] (1923). Twardowski begins his considerations with the traditional differentiation of the sciences into *a priori* (rational) and *a posteriori* (empirical) as well as their identification with, respectively, deductive and inductive sciences. The basis of this division is the statement that *a priori* sciences are based on reason and argumentation whereas *a posteriori* sciences—on experience and acts of perception. However, in Twardowski's opinion the very term 'to be based' is imprecise and in fact, it is a figurative expression. Since 'a science [. . .] is indeed no simple matter; it is made up of concepts and propositions (assertions), in addition to other factors of a formal nature' (1999, p. 172).[1]

The distinction of concepts into those that are independent of experience and concepts based on it has been known—it was Descartes or Kant that perceived this difference. However, Twardowski (referring to Hume) thinks that this distinction has no meaning for the discussed classification of sciences. The essential thing is the second constituent of every science, namely its propositions and assertions. Thus he writes:

> It is therefore suggestive to define *a priori* sciences as those sciences that discover their assertions, or that arrive at true propositions, in a matter independent of experience, by means of reasoning alone. And in contrast to these, the *a posteriori* sciences would be those that discover their assertions, or arrive at true propositions, by way of experience (1999, p. 173).[2]

The history of science provides, however, examples proving that this definition is not fully justified. Now, in both types of the sciences, neither reasoning nor experience can help discover the truths but one can do it rather thanks to intuition. Moreover, in *a posteriori* sciences assertions are often discovered by reasoning and in *a priori* sciences—using experience (suffice it to quote the example of Archimedes and his experiments, not only by means of reasoning but frequently by physical experiments, allowing him to discover certain relations and dependences, which he then—in general by means of the method of exhaustion—proved in an exact way). Therefore, the consideration of the question of discovering theses does

[1] 'nauka [. . .] to nie rzecz prosta; w jej skład wchodzą—by pominąć czynniki natury formalnej—pojęcia i sądy (twierdzenia)' (1923; 1965, p. 365). The page numbering of the Polish text is according to Twardowski, *Wybrane pisma filozoficzne* [Selected Philosophical Writings], 1965.

[2] 'Nasuwa się tedy takie określenie nauk apriorycznych, według którego byłyby to nauki wykrywające swe twierdzenia, czyli dochodzące do sądów prawdziwych, w sposób niezależny od doświadczenia, samym tylko rozumowaniem; w przeciwieństwie zaś do nich naukami aposteriorycznymi byłyby nauki wykrywające swe twierdzenia, czyli dochodzące do sądów prawdziwych, drogą doświadczenia' (1923; 1965, p. 366).

not provide a proper method that allows distinguishing the types of the sciences, either. The important thing is the manner of grounding theses in a given science. Twardowski writes:

> There is therefore a distinct difference between the sciences. It consists of the fact that one type of science makes the acknowledgement of new propositions that seek admission into its domain—however they might be obtained—depend on reasoning that proceeds without the participation of experience, whereas dependence on the latter is precisely what is required by the other type. In the mathematical sciences, the supreme court for deciding the acceptability of any particular proposition is a complex of definitions, axioms, and postulates, which serve as the springboard for the reasoning process. For the empirical sciences this court is experience, which ascertains the facts.
>
> Hence it is not the manner of *discovering* or finding out the truths, not the path by which the sciences arrive at new assertions, that constitutes the basis for dividing the sciences into *a priori* (rational) and *a posteriori* (empirical), but rather the manner of grounding them (1999, p. 176).[3]

Yet, this is not the end of the difficulties since the very concept of experience is ambiguous. When the *a posteriori* sciences are said to refer to experience while grounding their theses, what is meant is experience understood as single perceptual judgements. Whereas in mathematics, when we follow Mill claiming that axioms are based on experience, we mean generalisations from experience. Since mathematics, as all the *a priori* sciences, never solves doubts and justifies the truth of axioms—if at all it considers such questions—referring to perceptual judgements. Twardowski writes:

> In the mathematical sciences, and those akin to them, [. . .] ungrounded propositions are precisely the axioms (along with the definitions and postulates), whereas in the empirical sciences such propositions are the perceptual judgements, judgements that affirm individual facts (1999, p. 176).[4]

The *a priori* sciences are sometimes called deductive whereas the *a posteriori* sciences—inductive. Twardowski stresses the fact that it is not true that the former use only deduction and the latter—induction. Since in both types one can encounter these two methods. Nonetheless, 'deduction is a method that is characteristic of the *a priori* sciences and induction, of the *a posteriori* sciences' (1999, p. 177). That

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[3] '[. . .] istnieje zupełnie wyraźna różnica między naukami, polegająca na tym, że jedne z nich czynią uznanie mających wejść w ich skład sądów nowych—uzyskanych jakąkolwiek bądź drogą—zależnym od rozumowania, dokonującego się bez doświadczenia, drugie zaś czynią je zależnym od doświadczenia; dla jednych ostatnią instancją bezapelacyjną w rozstrzyganiu kwestii, czy sąd jakiś należy przyjąć, czy odrzucić, jest kompleks definicji, aksjomatów, postulatów, służących za punkt wyjścia w rozumowaniu, dla drugich taką instancją jest doświadczenie stwierdzające fakty.

Więc nie sposób *wykrywania*, wynajdywania prawd, nie droga, po której nauki do nowych twierdzeń dochodzą, lecz sposób ich uzasadniania jest podstawą podziału nauk na aprioryczne, czyli racjonalne, i aposterioryczne, czyli empiryczne' (1923; 1965, pp. 367–368).

[4] 'W naukach matematycznych i im podobnych [. . .] nieuzasadnionymi sądami są właśnie aksjomaty (wraz z definicjami i postulatami), w naukach empirycznych zaś takimi sądami są sądy spostrzeżeniowe, sądy stwierdzające indywidualne fakty' (1923; 1965, p. 369).

means that 'deduction may serve as the method for the ultimate grounding of propositions only in the *a priori* sciences, whereas induction may do so only in the *a posteriori* sciences' and 'deduction is the exclusive method of grounding propositions in the *a priori* sciences, whereas in the *a posteriori* sciences, in addition to induction, other methods can also be applied for the ultimate grounding of propositions—but never will deduction be found among these other methods' (1999, p. 177). [5]

Twardowski summarises his considerations, writing:

> We may therefore say that *a priori* (rational) sciences are also aptly referred to as 'deductive' because they must employ deduction when they wish to give a definitive *grounding* of their assertions, even though they may *arrive* at these by means of both deduction and induction. And the *a posteriori* (empirical) sciences are also correctly termed 'inductive' because they are the only sciences in which induction may serve as the final *grounding* of assertions, even though these sciences may arrive at their assertions along the path of both induction and deduction, and even though they may also make use of deduction as an auxiliary device for verifying some of their hypotheses (1999, p. 178).[6]

And he adds that 'an inductive science can never become deductive. On the other hand, it is possible: to display an inductive science in a deductive garb, provided it has reached an appropriate stage of evolution; to explicate it in a deductive fashion, to systematize it with a deductive method' (1999, p. 178).[7]

One more moment is still important. The *a priori* sciences, particularly mathematics and logic 'can boast of assertions that are certain' (1999, p. 179)[8] whereas the *a posteriori* sciences do not go beyond probable assertions. The former do not concern—according to Twardowski—facts whereas the latter operate with facts. 'If someone claims to have endowed the assertions of an inductive science with certainty by conferring on that science the garb of a deductive system, that person

[5] 'dedukcja i indukcja są metodami charakterystycznymi, pierwsza dla nauk apriorycznych, druga dla nauk aposteriorycznych'; 'dedukcja może służyć za metodę ostatecznego uzasadniania sądów tylko w naukach apriorycznych, indukcja zaś tylko w naukach aposteriorycznych'; 'dedukcja jest w naukach apriorycznych zarazem wyłączną metodą uzasadniania sądów, gdy tymczasem w naukach aposteriorycznych mogą obok indukcji być stosowane jeszcze inne metody ostatecznego uzasadniania sądów—nigdy jednak wśród tych metod nie znajdzie się dedukcja' (1923; 1965, p. 370).

[6] 'Można więc powiedzieć, że nauki aprioryczne, czyli racjonalne, dlatego zwą się słusznie także naukami dedukcyjnymi, że muszą się posługiwać dedukcją, gdy chcą swe twierdzenia ostatecznie uzasadnić, chociaż dochodzić do swych twierdzeń mogą zarówno drogą dedukcji jak indukcji. A nauki aposterioryczne, czyli empiryczne, dlatego zwą się słusznie także naukami indukcyjnymi, że są jedynymi naukami, w których indukcja może służyć do ostatecznego uzasadniania twierdzeń, chociaż dochodzić do swych twierdzeń mogą nauki indukcyjne zarówno drogą indukcji jak i dedukcji i chociaż także poza tym posługują się dedukcją jako środkiem pomocniczym przy sprawdzaniu niektórych swoich przypuszczeń' (1923; 1965, p. 371).

[7] 'nauka indukcyjna nie może stać się nauką dedukcyjną; można natomiast naukę indukcyjną, skoro osiągnęła odpowiedni stopień rozwoju, przedstawić w szacie dedukcyjnej, wyłożyć ją sposobem dedukcyjnym, usystemizować ją metodą dedukcyjną' (1923; 1965, p. 371).

[8] 'mogą się szczycić twierdzeniami pewnymi' (1923; 1965, p. 372).

has thereby fallen pray to a deception—having mistaken the form of a thing for its essence' (1999, p. 179).[9]

Let us notice that in his considerations Twardowski distinguishes clearly what is nowadays called 'context of discovery' and 'context of justification,' stressing the role of the latter for the theory of science. Additionally, his ideas are also characteristics of the whole Lvov-Warsaw School. The distinction of these two contexts can already be found in the works of John Stuart Mill. This distinction became popular when Karl Popper mentioned it in his work *Logik der Forschung* [The Logic of Scientific Discovery] (1934). Twardowski—as seen above—adds the context of explication of a given scientific discipline to these two contexts.

It is worth mentioning that the discussed views of Twardowski were reinterpreted by M. Kokoszyńska (cf. Sect. 3.8 of this chapter). In her work (1957) she formulated the criterion of distinguishing between 'good' and 'bad' induction, using the concept of probability. Whereas in her works (1962) and (1967) she analysed the concept of deductive justification and considered its role in empirical sciences. At that point she made a distinction between absolute and relative justification. In her opinion this distinction makes it possible to formulate and solve the problem of justification of propositions because of their analytic–synthetic distinction. Only analytic propositions can be justified deductively in the absolute sense whereas synthetic propositions can be justified deductively in the relative sense. At the same time, a synthetic proposition that is justified deductively in the relative sense does not become analytic, and the empirical knowledge is not transformed into the *a priori* knowledge.

Let us proceed to the other question indicated in the beginning, namely Twardowski's defence of the absolute understanding of the concept of truth. He dedicated the whole work 'O tzw. prawdach względnych' [On So-Called Relative Truths] (1900) to this issue. He begins by explaining the terminology:

> Those judgements that are unconditionally true, without any reservations, irrespective of any circumstances, are called 'absolute truths,'—judgements, therefore, that are true always and everywhere. On the other hand, those judgements that are true only under certain conditions, with some measure of reservation, owing to particular circumstances, are called 'relative truths'; such judgements are therefore not true always and everywhere (1999, p. 147).[10]

Twardowski aims at showing that there are no relative truths. He proves his statement by saying that the examples given by the advocates of relative truths, i.e. the so-called *relativists*, do not justify the existence of such truths. His main tool of analysis is the distinction between a judgement and statement. He writes:

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[9] 'Kto zaś sądzi, że nadając nauce indukcyjnej szatę systemu dedukcyjnego tym samym wyposaża jej twierdzenia w pewność, ulega złudzeniu, biorąc formę za istotę rzeczy' (1923; 1965, p. 372).

[10] 'Bezwzględnymi prawdami nazywają się te sądy, które są prawdziwe bezwarunkowo, bez jakichkolwiek zastrzeżeń, bez względu na jakiekolwiek okoliczności, które więc są prawdziwe zawsze i wszędzie. Względnymi zaś prawdami nazywają się te sądy, które są prawdziwe tylko pod pewnymi warunkami, z pewnym zastrzeżeniem, dzięki pewnym okolicznościom; sądy takie nie są więc prawdziwe zawsze i wszędzie' (1900; 1965, p. 315).

> For although there is a very intimate connection between a judgement [. . .] and a statement, which is the external expression of the judgement, the statement is nonetheless not identical with the judgement, just as the noun that ordinarily serves as the external sign of an image or concept is not identical with the image or concept. The relativists, however, do not take this distinction into account, and only because of this lack of rigour are they in a position to adduce examples of judgements which apparently support their theory concerning the existence of relative truths (1999, p. 149).[11]

The identity of statements does not guarantee the identity of the judgements themselves. The same statements may express different judgments, which can be caused by the use of occasional and ambiguous expressions. Deleting or making such expressions precise removes the relativity of truth. Another error of the relativists, which Twardowski analyses, is the confusion between probability and certainty in scientific hypotheses and theories extracted from experience by way of induction. Hypotheses and theories can be more or less probable because of our knowledge but the logical value of a judgement, i.e. its truth or falsity, does not depend on our knowledge of its justification. Twardowski also engages in polemics with relativism rooted in subjectivism. His considerations end with an important statement: it is the judgements that are the bearers of truth in the fundamental sense whereas the statements—only in the secondary meaning:

> [. . .] *the differentiation of truth* into *relative and absolute has its raison d'être only in the realm of statements*, to which the characteristic of truth applies only in a figurative, mediated sense. *Insofar as judgements themselves are concerned, on the other hand, it is impossible to speak of relative and absolute truth, since every judgement is either true, or else it is not true, in which case it also is not true anywhere and at any time* (1999, p. 169).[12]

Twardowski's work on relative and absolute truths played an important role in the development of the Lvov-Warsaw School. In some way it marked out the way of further search in the field of the theory of truth. Almost all representatives of this School accepted Twardowski's arguments and opted for absolutism. The exceptions were Edward Poznański and Aleksander Wundheiler who followed operationalism in physics (cf. their work 1967). Twardowski's argumentation and some of his theses were developed by M. Kokoszyńska in her works (1936a, 1936b, 1939–1946, 1949–1950)—cf. Sect. 3.8 of this chapter.

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[11] 'Chociaż [. . .] między sądem a powiedzeniem, które jest zewnętrznym wyrazem sądu, zachodzi związek bardzo ścisły, przecież powiedzenie tak samo nie jest identyczne z sądem, jak nie jest identyczny z wyobrażeniem albo pojęciem rzeczownik służący zwykle jako zewnętrzny znak wyobrażenia albo pojęcia. Relatywiści jednak nie liczą się z tą różnicą i tylko dzięki tej nieścisłości są w stanie przytaczać przykłady sądów popierających pozornie ich teorię o istnieniu prawd względnych' (1900; 1965, p. 317).

[12] '[. . .] *rozróżnienie względnej i bezwzględnej prawdziwości ma rację bytu tylko w dziedzinie powiedzeń*, którym cecha prawdziwości przysługuje jedynie w znaczeniu przenośnym; *o ile zaś chodzi o same sądy, nie można mówić o względnej i bezwzględnej prawdziwości, gdyż sąd każdy albo jest prawdziwy, a wtedy jest zawsze i wszędzie prawdziwy, albo też nie jest prawdziwy, a wtedy też nie jest nigdy i nigdzie prawdziwy*' (1900; 1965, pp. 335–336).

3.2 Jan Łukasiewicz

Łukasiewicz dealt with philosophy (especially at the early stage of his scientific research) and above all, with mathematical logic. Although he received education in philosophy, he had an excellent intuitive understanding of mathematics. He was the head of the Chair of Philosophy at the Faculty of Mathematics and Natural Sciences of Warsaw University where he taught logic to students of mathematics. His lectures were very popular with students. One of them, the outstanding Polish mathematician Kazimierz Kuratowski, recalled these lectures after years:

> Another professor[13] who exerted a great influence on the interests of young mathematicians was Jan Łukasiewicz. Apart from his lectures in logic and the history of philosophy Prof. Łukasiewicz delivered more specialist lectures that threw new light on the methodology of deductive sciences and the foundations of mathematical logic. Although Łukasiewicz was not a mathematician, he had an exceptional understanding of mathematics thanks to which his lectures had especially strong repercussions on mathematicians (1973, p. 32).[14]

Łukasiewicz's change of research interests from philosophy to mathematical logic coincided with the announcement of Janiszewski's programme of development of mathematics in Poland (cf. Sect. 2.1, Chap. 2). Janiszewski attributed a serious role to investigations in mathematical logic and the foundations of mathematics, in particular to set theory. Łukasiewicz joined (together with Leśniewski—cf. Sect. 3.4) the editorial board of the periodical *Fundamenta Mathematicae* created by Janiszewski. The presence of two logicians (with philosophical education) on this board beside three mathematicians (Zygmunt Janiszewski, Stefan Mazurkiewicz and Wacław Sierpiński) constituted an excellent example of collaboration between logicians and mathematicians, which was typical of the Warsaw School, collaboration that yielded wonderful fruits.[15]

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[13] Earlier he mentioned Stefan Mazurkiewicz and Zygmunt Janiszewski [remark is mine].

[14] 'Innym profesorem, który wywarł duży wpływ na zainteresowania młodej kadry matematycznej, był Jan Łukasiewicz. Prócz wykładów z logiki i historii filozofii, prowadził profesor Łukasiewicz bardziej specjalistyczne wykłady, które rzucały nowe światło na metodologię nauk dedukcyjnych i podstawy logiki matematycznej. Aczkolwiek Łukasiewicz nie był matematykiem, miał jednak wyjątkowo dobre wyczucie matematyczne, dzięki czemu wykłady jego znajdowały szczególnie silny oddźwięk u matematyków.'

[15] At this point, it is worth adding that the relationships between logicians and mathematicians were not always ideal in Warsaw. For instance, towards the late 1920s there was a conflict that made Łukasiewicz and Leśniewski leave the editorial board of *Fundamenta Mathematicae* in around 1930. The exact reasons for their decisions were never given. Woleński (1997) writes that the source of the conflict was the difference in Leśniewski's and Sierpiński's views concerning set theory, which they expressed in their publications. Leśniewski had a negative attitude towards standard set theory, which he wanted to replace with his mereology. He thought that set theory contained errors. Sierpiński repaid Leśniewski with malicious remarks regarding his paper to be published in *Fundamenta Mathematicae*—it must have been the second part of the paper entitled 'Grundzüge eines neuen System der Grundlagen der Mathematik,' the first part was published in *Fundamenta* in 1929 (cf. Leśniewski 1929a). Responding to that Leśniewski withdrew his text and gave up his membership in the editorial board. Łukasiewicz—showing his solidarity with

Before discussing Łukasiewicz's philosophical views on mathematics and logic we will say a few words about his logical works. It is especially desirable because Łukasiewicz, a typical representative of the Lvov-Warsaw School, was a follower of scientific philosophy, based on exact methodological foundations. Thus his standpoints on various problems related to the philosophy of mathematics and logic remained in close contact with his logical investigations. Łukasiewicz even postulated to construct philosophy as an axiomatic system confronted with experience. He was convinced that logic provided tools that allowed solving scientific problems (including philosophical problems) in a strict way. He wrote about his fascination in mathematical logic and its methods as well his reasons for changing his interests from philosophy to logic:

> My critical appraisal of philosophy as it has existed so far is the reaction of a man who, having studied philosophy and read various philosophical books to the full, finally came into contact with scientific method not only in theory, but also in the direct practice of his own creative work. This is the reaction of a man who experienced personally that specific joy which is a result of a correct solution of a uniquely formulated scientific problem, a solution which at any moment can be checked by a strictly defined method and about which one simply knows that it must be that and no other and that it will remain in science once and for all as a permanent result of methodical research (1970, pp. 227–228).[16]

Łukasiewicz's achievements in mathematical logic allow us to treat him as one of the most outstanding representatives of this field in the twentieth century. In particular, he might have been one of the most eminent creators of propositional calculi. His achievements include: (1) the elaboration of a special logical notation (called parenthesis-free symbolism, Łukasiewicz symbolism or Polish notation) that was excellent to conduct investigations on logical calculi in the Warsaw School of Logic[17]; (2) the creation of many-valued logics; (3) research—based on many-valued logics—on modal connectives and the construction of the so-called systems of Ł-modal logic; (4) the elaboration of a series of axiomatic systems for classical logic calculus (in particular, axiomatic implication-negation system for propositional calculus); (5) investigations into the metalogical properties of various systems of propositional calculus. He also dealt with the history of logic in which he

Leśniewski—left the board, too. But this conflict did not influence the further development of logic, especially the Warsaw School of Logic.

[16] 'Krytyczna ocena moja dotychczasowej filozofii jest reakcją człowieka, który przestudiowawszy filozofię i naczytawszy się do syta różnych książek filozoficznych, zetknął się nareszcie z metodą naukową nie tylko w teorii, ale w żywej i twórczej praktyce osobistej. Jest to reakcja człowieka, który doznał osobiście tej szczególnej radości, jaką daje poprawne rozwiązanie jednoznacznie sformułowanego zagadnienia naukowego, które w każdej chwili można skontrolować przy pomocy ściśle określonej metody i o którym wie się po prostu, że musi być takie, a nie inne, i że pozostanie w nauce po wieczne czasy jako trwały wynik metodycznego badania.' (1936, p. 123).

[17] Łukasiewicz worked out the principles of his notation in 1924. The very idea on which it is based, i.e. writing the functors before the arguments, comes from L. Chwistek. He spoke about it in a paper delivered in Warsaw in the early 1920s. However, it is worth noting that the term 'symbolism of Łukasiewicz' is justifiable because parenthesis-free symbolism is something more than only writing the functors before the arguments.

formulated a virtually new paradigm of research. What he did was to analyse the original historical texts using the concepts of contemporary mathematical logic, which yielded excellent results. Łukasiewicz showed that the Stoics' logic was different from Aristotle's logic. The former was actually the logic of sentences and the latter—the logic of names. Using the same methods he analysed Aristotle's syllogistic (cf. 1951). He was, which is of importance, a magnificent organizer of scientific life. Together with Leśniewski he created the so-called Warsaw School of Logic. It was in his seminar that the following logicians matured: Alfred Tarski, Stanisław Jaśkowski, Adolf Lindenbaum, Jerzy Słupecki, Bolesław Sobociński and Mordechaj Wajsberg.

Now let us proceed to Łukasiewicz's philosophical views on mathematics and logic. We will begin with the question of understanding logic as a science. In his review of Władysław Biegański's work *Czym jest logika?* [What Is Logic?] we find the following formulation:

> Logic does not only concern *argumentation*[18] but *reasoning* in general, while using the term 'reasoning' as more general than 'argumentation' in accordance with Prof. Twardowski's view (cf. my dissertation *O twórczości w nauce*,[19] [About Creativity in Science] p. 8). Secondly, argumentation or reasoning is also *thinking*, and thus psychologism returns. I would agree to distinguish between logic as 'science' and 'art' but I would use different terms. Namely, I think that logic as a *theoretical* science investigates relations which formal propositions (e.g. S is P) maintain because of their truth or falsity, and makes laws of these relations (e.g. 'if it is true that S is M and M is P, it is also true that S is P'); as a practical science it applies these laws to solve *tasks* in the area of *reasoning* in general, for instance to introduce some conclusion, like in induction, to verify or prove some thesis, etc. I will present this view, which has only been outlined here, in some larger work (1912b).[20]

However, Łukasiewicz did not fulfil his promise and we must satisfy ourselves with the above-quoted words. We have the distinction between theoretical and practical logic, which corresponds to the distinction between *logica docens* and *logica utens*. Łukasiewicz defines both in an anti-psychologistic way. Briefly speaking, he understands logic as argumentation theory, dividing reasoning into deductive and reductive; further dividing deductive reasoning into concluding and verifying, and reductive reasoning into proving and explicating (cf. 1912a). At this

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[18] Biegański related logic to argumentation [remark is mine].

[19] Cf. Łukasiewicz, J. (1912a).

[20] 'Logika tyczy się nie tylko *dowodzenia*, ale w ogóle rozumowania, przy czym zgodnie z prof. Twardowskim używam terminu "rozumowanie" jako ogólniejszego od "dowodzenia" (cf. rozprawę moją *O twórczości w nauce*, str. 8). Po wtóre, dowodzenie czy rozumowanie jest także *myśleniem*, a więc psychologizm powraca. Zgodziłbym się natomiast na odróżnienie logiki jako "nauki" i "sztuki", tylko użyłbym innych terminów. Sądzę mianowicie, że logika jako nauka *teoretyczna* bada stosunki, w jakich zdania formalne (np. S jest P) pozostają do siebie ze względu na swoją prawdziwość lub fałszywość, i ustanawia prawa tych stosunków (np. "jeśli prawdą jest, że S jest M i M jest P, to prawdą jest, że S jest P"); jako nauka praktyczna stosuje te prawa do rozwiązywania *zadań* z zakresu *rozumowania* w ogóle, np. do wyprowadzenia jakiejś konkluzji, jak we wnioskowaniu indukcyjnym, do sprawdzenia lub udowodnienia jakiejś tezy itp. Pogląd ten, tu tylko naszkicowany, przedstawię może w jakiejś pracy obszerniej.'

point, it should be stressed that Łukasiewicz meant all kinds of reasoning that have the relation of reason and result. Woleński (1999) discerns the source of Łukasiewicz's standpoint in the fact that although he did not value induction very much, he was interested in a concept of reasoning that would be wide enough to embrace inductive reasoning, which he treated as a kind of reduction. Moreover, Woleński thinks that another source is the fact that the Lvov-Warsaw School of Logic treated logic as *organon* (in the spirit of Aristotle), which can be used to solve all intellectual tasks. Consequently, the wide understanding of the scope of practical logic forced this understanding of theoretical logic.[21]

In this context one should look at the problem of the attitude of logic towards philosophy and mathematics. First of all, Łukasiewicz did not approve the term 'philosophical logic.' In his textbook *Elementy logiki matematycznej* [Elements of Mathematical Logic] he wrote:

> If we use here the term 'philosophical logic' we mean this complex of problems that are in books written by philosophers, and this logic we were taught in secondary school. Philosophical logic is not a homogenous science; it contains various issues; in particular, it enters the field of psychology when it speaks not only about a proposition in a logical sense but also this psychological phenomenon, which corresponds with a proposition and which is called 'judgement' or 'conviction' (1929a, p. 12).[22]

He thought that connecting logic with psychology resulted from a false understanding of the object of logical investigations:

> Logic is often said to be a science of the laws of thinking and since thinking is a psychological activity logic should be a part of psychology (1929a, p. 12).[23]

But logic is not a part of psychology because issues of psychological character related to thinking should be analysed with the aid of completely different methods than those used in logic. Furthermore, logic does not embrace epistemological problems, which philosophical logic reflects on, for example the problem of the definition of truth or the criterion of truth.

Łukasiewicz did not value philosophical logic. He was neither willing to include formal logic he dealt with into philosophy nor was he inclined to treat it as a servant to philosophy. He regarded formal logic as an independent and autonomous science. In his paper 'O znaczeniu i potrzebach logiki matematycznej' [About the Meaning and Needs of Mathematical Logic] he wrote:

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[21] Cf. Ajdukiewicz's views in this respect (see Sect. 3.6), Zawirski's views (see Sect. 3.3) or Tarski's (Sect. 3.7).

[22] 'Jeśli używamy tu terminu "logika filozoficzna", to chodzi nam o ten kompleks zagadnień, które znajdują się w książkach pisanych przez filozofów, o tę logikę, której uczyliśmy się w szkole średniej. Logika filozoficzna nie jest jednolitą nauką, zawiera w sobie zagadnienia rozmaitej treści; w szczególności wkracza w dziedzinę psychologii, gdy mówi nie tylko o zdaniu w sensie logicznym, ale także o tym zjawisku psychicznym, które odpowiada zdaniu, a które nazywa się "sądem" albo "przekonaniem".'

[23] 'Mówi się często, że logika jest to nauka o prawach myślenia, a ponieważ myślenie jest to czynność psychiczna, więc logika powinna być częścią psychologii.'

> In Poland, especially in Warsaw, mathematical logic is treated as an independent science, having its own goals and tasks. The deductive systems, belonging to logic, are, in our opinion, equally important, or even more important, since they are more basic than the various deductive systems included in mathematics. We understand the specificity of logical problems and do not treat them only from the perspective of the usefulness of their solutions for mathematicians or not (1929b, pp. 606–607).[24]

Accordingly, in his opinion logic is not auxiliary to mathematics. Moreover, it does not face—at least in Poland—the danger of 'going astray towards philosophical speculations' (1929b, p. 605), which is guaranteed by the fact that 'Polish mathematicians, who are collaborating with us, think soberly enough not to yield to non-scientific fantasies' and that 'in Poland almost all the philosophers who deal with mathematical logic are the followers of Prof. Twardowski, and therefore, they belong to the so-called Lvov School of Philosophy, where they have learnt to think clearly, conscientiously and methodically' (1929b, p. 605). Let us add that the conviction of the autonomy of logic was shared by some mathematicians, including Janiszewski (cf. Sect. 2.1, Chap. 2) and that logic was placed centrally in the programme of the development of mathematics that Janiszewski formulated. Łukasiewicz favoured the collaboration between mathematicians and philosophers while creating mathematical logic in Poland. In the quoted paper he wrote:

> Since mathematicians will not allow mathematical logic to be changed into some philosophical speculation whereas philosophers will defend this science against the slavish application of mathematical methods in it and against limiting its role to being an auxiliary mathematical science (1929b, p. 606).[25]

He stressed:

> Today in Warsaw we know that there are no two logics: mathematical and philosophical. There is only one logic, the one initiated by Aristotle, completed by the Stoics, practised many a time with fairly great subtlety by logicians in the Middle Ages, misunderstood and neglected by modern philosophy, and flourishing today again in a more perfect form thanks to the efforts of mathematical logicians (1929b, p. 607).[26]

An essential feature of mathematical logic is its scientific precision. At the same time mathematical logic surpasses mathematics itself in exactness. Mathematicians

[24] 'W Polsce, a zwłaszcza w Warszawie, traktuje się dziś logikę matematyczną jako naukę samodzielną, mającą swe własne cele i zadania. Systemy dedukcyjne, należące do logiki, są zdaniem naszym równie ważne, a może nawet ważniejsze, bo bardziej podstawowe niż różne systemy dedukcyjne zaliczane do matematyki. Rozumiemy swoistość zagadnień logicznych i nie traktujemy ich jedynie pod tym kątem widzenia, czy rozwiązanie ich przyda się na coś matematykom, czy też nie.'

[25] 'Matematycy bowiem nie dopuszczą, by logika matematyczna zmieniła się w jakąś spekulację filozoficzną, filozofowie zaś obronią tę naukę przed niewolniczym stosowaniem w niej metod matematycznych i zacieśnieniem jej do roli pomocniczej nauki matematycznej.'

[26] 'Wiemy dziś w Warszawie, że nie ma dwóch logiki matematycznej i filozoficznej; istnieje jedna tylko logika, zapoczątkowana przez Arystotelesa, uzupełniona przez stoików, uprawiana z niemałą nieraz subtelnością przez logików średniowiecznych, niezrozumiana i zaniedbana przez filozofię nowożytną, a rozkwitająca dziś na nowo w doskonalszej postaci dzięki wysiłkom logików matematycznych.'

should—as Łukasiewicz writes (1929b, p. 607)—pattern themselves on logic while building their own systems and proving their theorems. He adds:

> And this is exactly the prime significance of mathematical logic, both for mathematics and all sciences (1929b, p. 611).[27]

In his paper 'O twórczości w nauce' (published in *Poradnik dla samouków*) he stated:

> Logic, along with mathematics, can be compared to a fine set, which we cast into an immeasurable depth of phenomena to find pearls of scientific syntheses in it. They are powerful research *tools*, yet only tools (1912a, p. 13).[28]

The polemic that arose after the publication of his paper 'O pojęciu wielkości. (Z powodu dzieła Stanisława Zaremby)' [About the Concept of Quantity. (Because of Stanisław Zaremba's Work)] (1916), in which he conducted a methodological analysis of *Arytmetyka teoretyczna* [Theoretical Arithmetic] by Stanisław Zaremba (1912), can tell us a lot about Łukasiewicz's views on the role and place of logic towards mathematics. This discussion clearly shows that Łukasiewicz thought—contrary to Zaremba (cf. Sect. 5.2, Chap. 5)—that mathematical logic belonged to the core of mathematics and should be treated as an autonomous discipline.

As these issues are important it is worth focusing on the polemic itself and the views of those who took part in it.[29]

In the year 1916 Łukasiewicz (head of one of the chairs of philosophy) devoted one of his courses for students of mathematics at Warsaw University to the methodology of deductive sciences. During the course he analysed—as Kazimierz Kuratowski, one of his listeners, recollects—'the principle with which every system of axioms (such as the consistency and independence of axioms) should comply' (1981, p. 64). During those lectures,[30] Łukasiewicz conducted a detailed methodological analysis of Zaremba's work *Arytmetyka teoretyczna* and questioned its complicated principle that was to replace the principle of the independence of axioms.

Łukasiewicz's analysis concerned first of all the definition of quantity given by Zaremba who wrote:

> We attach the name 'quantity' to every thing, which can be regarded as one of the objects constituting together a specified infinitely numerous class of such things out of which every two *A* and *B* are comparable with each other on the basis of certain definitions, especially adjusted to the discussed class, and consistent with the principles [equality and inequality—remark is mine] given in the previous section, assuming that whatever integer we marked as

[27] 'I na tym właśnie polega główne znaczenie logiki matematycznej, zarówno dla matematyki, jak i dla wszystkich nauk.'

[28] 'Logikę wraz z matematyką można by przyrównać do misternej sieci, którą zarzucamy w niezmierną toń zjawisk, by wyławiać z niej perły syntez naukowych. Są to potężne *narzędzia* badania, lecz tylko narzędzia.'

[29] Here we refer to Chap. VIII of Woleński's book (1997).

[30] The basis of these lectures must have been Łukasiewicz's article 'O pojęciu wielkości. (Z powodu dzieła Stanisława Zaremby)' (1916), completed in May 1915.

> n we would always be able to find in the discussed class n such things so that no two things are mutually equal (1912, p. 14).[31]

Łukasiewicz did not like this formulation—and similar ones—because it contained several principles in one sentence, thus making it difficult to understand the logical structure of the definition itself and the proofs within which this definition occurs. Consequently, the proofs given by Zaremba are—according to Łukasiewicz—incomplete. The source of Zaremba's problem is the concept of propositions, which are devoid of content. For example, the proposition '2–5 is smaller than zero' is devoid of content if considered in the arithmetic of natural numbers. In order to avoid difficulties Zaremba assumes that the independence of a given system of axioms can be considered only with the presumption that the considered system has no propositions without content. According to Łukasiewicz, the concept of a proposition devoid of content is vague and psychological and it forces to give up the principle of excluded middle and unnecessarily complicates the principle of the independence of axioms. The very concept of quantity, formulated by Zaremba, is too lengthy and can be simplified in many ways.

Writing this paper (1916) Łukasiewicz began a 3-year polemic in which Kazimierz Kuratowski, Tadeusz Czeżowski and Leon Chwistek were also involved. Zaremba answered Łukasiewicz writing a paper 'O niektórych poglądach p. Łukasiewicza. na metodykę nauk dedukcyjnych' [About Some Views of Mr Łukasiewicz Concerning the Methodology of Deductive Sciences] (1917), trying to specify the concept of proposition devoid of content. He referred to Russell's theory of types, in which one can speak of the scope in which a proposition can be reasonably negated or approved. Then Kuratowski joined the debate (cf. 1917). He connected the problem of propositions devoid of content with the theory of definition, namely he noticed that the existence of such propositions contradicts the postulate of the completeness of definition and must lead to the change of the concept of consequence. Later Kuratowski and Zaremba exchanged their remarks (cf. Zaremba 1918a, b; Kuratowski 1918) but they concerned details and not the essential questions. Czeżowski (1918) questioned the validity of Zaremba's reference to Russell and the theory of types, arguing that in this theory soundness was related to the principle that types should not be mixed. This opinion was supported by Chwistek in his paper (1919), in which he writes that the propositions that Zaremba treated as devoid of content are simply false in the light of the theory of types.

From the standpoint of contemporary mathematical logic the whole problem, which was in focus of the polemic, can be solved by relativising formalism to a

[31] 'Wielkością nazywamy każdą rzecz, która uważana być może za jeden z przedmiotów stanowiących razem oznaczoną, nieskończenie liczną klasę rzeczy takich, z których każde dwie A i B są na podstawie pewnych do rozważanej klasy specjalnie przystosowanych, a z zasadami przytoczonymi w ustępie poprzedzającym zgodnych [chodzi tu o zasady równości i nierówności—uwaga moja, R.M.] definicji, pomiędzy sobą porównywalne, zakładając przy tym, że jakąkolwiek liczbę całkowitą oznaczylibyśmy przez n, będziemy zawsze mogli znaleźć w rozważanej klasie n takich rzeczy, żeby żadne dwie z nich nie były sobie równe.'

certain given interpretation or model. Yet, here we speak about that dispute since it revealed various approaches to the question of the place and role of logic in mathematics. Zaremba was of the opinion that logic should play an auxiliary role in mathematics. It should serve to construct correct mathematical argumentations, and thus it belongs to the propaedeutics of mathematics and is not the subject matter of independent studies. Consequently, there is no whatsoever priority of logic over mathematics. According to Zaremba, the postulates of completeness of proofs and redundancy (resulting from elimination) of definition, which the 'new' mathematical logic demanded and stressed, are only a ballast and rather an interference, making clarity and communicativeness difficult. This view is confirmed by the following quotations from Zaremba's paper 'O niektórych poglądach p. Łukasiewicza na metodykę nauk dedukcyjnych':

> On the basis of the attempt I have made I claim that in this case [if Łukasiewicz was right—remark is mine] it would be right to use more complicated propositions than those I accepted as sufficient, and through that the clarity of the list of the characteristic properties of real numbers would suffer, with no advantage for it. In fact, I meant to introduce simplifications of the same category like those we realise by setting suitable definitions; from the perspective of separated logic definitions are not necessary since all defined expressions can be replaced by their equipollents, which do not contain defined terms. In this case the theory would not be changed despite the fact that all of the definitions alone became redundant. However, applying this procedure the exposition would become so extremely complicated that the whole theory would become almost incomprehensible. [. . .]
>
> Mathematicians, realising how that would be put into practice, hardly develop 'complete proofs' [. . .] which they present themselves, but are satisfied with giving, under the label of 'proofs,' less or more detailed sketches of complete proofs. Such a procedure has been forced on us because facing the contemporary state of scientific symbolism, despite the ideas of Peano, Russell and others, the complete proofs of even very elementary theorems are so extensive that it would be impossible to give them for a considerably bigger number of theorems. Well, a sketch of a complete proof differs from the proof itself in that in a sketch we do not refer to all premises but only to some, assuming that readers themselves will see the role of premises that have not been mentioned in the sketch (1917, pp. 75–76).[32]

[32] 'Na podstawie próby, uczynionej przeze mnie, twierdzę, że w takim razie [tzn. gdyby Łukasiewicz miał rację] wypadałoby używać zdań bardziej skomplikowanych od tych, które mi wystarczyły, a przez to ucierpiałaby zrozumiałość wykazu własności charakterystycznych liczb rzeczywistych bez żadnej korzyści dla niej samej. W rzeczywistości chodziło mi o wprowadzenie uproszczeń tej samej kategorii jak te, które urzeczywistniamy przez ustawienie odpowiednich definicji; ze stanowiska oderwanej logiki definicje nie są konieczne, gdyż wszystkie wyrażenia definiowane można by zastąpić przez równoważne im wyrażenia, nie zawierające terminów definiowanych, a w takim razie teoria nie doznałaby zmiany, pomimo że same wszystkie definicje stałyby się zbędnymi. Jednakowoż, przy takim postępowaniu, wykład stałby się tak niezmiernie skomplikowanym, że cała teoria stałaby się prawie niezrozumiałą. [. . .]

Matematycy z zupełną świadomością tego w czynie nie rozwijają prawie nigdy "zupełnych dowodów" [. . .] przez siebie wygłaszanych, a poprzestają na podawaniu, pod mianem dowodów, mniej lub bardziej szczegółowych szkiców dowodów zupełnych. Takie postępowanie jest nam narzucone przez to, że, przy dzisiejszym stanie symboliki naukowej, pomimo pomysłów Peana, Russella i innych, dowody zupełne nawet bardzo elementarnych twierdzeń, są tak obszerne, że podawanie ich przy znaczniejszej ilości twierdzeń byłoby niepodobieństwem. Otóż szkic dowodu

Łukasiewicz's standpoint was different. He lamented first of all on the weak knowledge or simply ignorance of modern mathematical logic:

> [. . .] a new logic has been created and undoubtedly, it will become a powerful, though subtle, tool of cognition in all domains of knowledge. [. . .] This new logic, which is flourishing now, has been very little known so far. Only some of its concepts, many a time distorted, are penetrating the circles of those scientists who do not practice logic professionally. Much time will pass until these new concepts and logical methods break all prejudices that are obstacles today, and become accepted by all scientists. That is why I was not surprised when I could not read any of the above-mentioned names [Boole, De Morgan, Schröder, Russell, Frege, Peano] in the work of the professor of the Jagiellonian University, but I came across views and methods that are imprecise, and even false, from the perspective of modern logic (1916, p. 2).[33]

Łukasiewicz gives three reasons for the need to build complete proofs having all premises that have been used (he reproached Zaremba with their lack):

1. Incomplete proof is didactically defective because readers are not always able to complete it,
2. Treating certain premises as implicit can easily become a source of errors,
3. Incomplete proofs do not allow stating and checking—neither by the author nor by readers—on what premises a given proof is based.

According to him, the third reason is the most important question, which he justifies as follows:

> Science is not only to gather as many true propositions as possible; science is a construction in which every detail should be connected with the whole. Logical relations are the putty joining true propositions. Therefore, one should examine these relations as thoroughly as possible and following them one should shape the theory. That is why all the details of a proof, even the pettiest ones, are important because they testify to the existence of some logical relation. [. . .] Logical algebra is an instrument that makes it easier, and even many a time makes it possible, to reveal logical relations. [. . .] So far mathematicians have not dealt with logical algebra because they have not cared for the logical relations that may exist among the truths they have discovered. They have not even got to know which truths should be regarded as principles and which as theorems. It has been sufficient for them to prove some theorem. Only recently they have had the need to order logically the materials that have been accumulated in mathematics throughout the ages. This work has been taken

zupełnego różni się tym od samego takiego dowodu, że w szkicu powołujemy się nie na wszystkie przesłanki, a tylko na niektóre, przyjmując, że czytelnik sam już dostrzeże rolę przesłanek w szkicu nie wspomnianych.'

[33] '[. . .] powstała logika nowa, która stanie się bez wątpienia potężnym a subtelnym narzędziem poznania we wszystkich dziedzinach wiedzy. [. . .] Ta nowa logika, znajdująca się obecnie w postaci rozkwitu, jest dotąd bardzo mało znana. Zaledwie niektóre jej pojęcia, częstokroć wypaczone, przenikają do kół tych uczonych, którzy nie uprawiają logiki z zawodu. Potrzeba będzie długiego czasu, zanim te nowe pojęcia i metody logiczne przełamią wszystkie uprzedzenia, jakie kładą im się dziś w poprzek, i staną się własnością ogółu uczonych. Dlatego nie zdziwiłem się wcale, gdy w dziele uczonego profesora Wszechnicy Jagiellońskiej nie wyczytałem żadnego z nazwisk, cytowanych powyżej [Boole, De Morgan, Schröder, Russell, Frege, Peano], a natomiast spotkałem się z poglądami i metodami, które ze stanowiska logiki współczesnej są nieścisłe, a nawet błędne.'

> up first of all by those mathematicians who are at the same time logicians [. . .] (1916, pp. 14–15).[34]

He explains the need of precision in mathematics, strictly speaking in the analysis of mathematical reasoning:

> Many seem to think that logic is filled with subtleties, which find no justification facing sound reason; these subtleties needlessly make it difficult to get to know what is scientifically important.[35] [. . .] Such judgements concerning mathematics and logic are wrong. What amateurs can see as a logical subtlety is merely a postulate of scientific precision. This precision does not hamper the cognition of scientifically valuable truths but on the contrary, it makes it easier (1916, p. 53).[36]

He concludes:

> Mathematics, which has been regarded as the most exact science so far, turns out to be full of faults and errors if we put it against this new measure of precision (1916, p. 68).[37]

So much for the polemic between Łukasiewicz and Zaremba and their views, which the polemic revealed. Łukasiewicz also wrote about the meaning of formal logic in *Dodatek* [Supplement] to the book *O zasadzie sprzeczności u Arystotelesa. Studium krytyczne* [On the Principle of Contradiction in Aristotle. A Critical Study] (1910).[38] He opposes the view that ‘formal logic in general, and symbolic logic in particular, is only a mental toy, devoid of any more serious meaning’ (1910, p. 181). He describes the value of symbolic logic in the following points:

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[34] ‘Nauka nie na tym tylko polega, by gromadzić bezładnie jak najwięcej zdań prawdziwych; nauka to budowa, w której każdy szczegół powinien być związany z całością. Kitem, spajającym zdania prawdziwe, są związki logiczne. Należy zatem te związki badać jak najstaranniej i według nich kształtować teorię. Dlatego każdy i najdrobniejszy szczegół dowodu jest ważny, bo świadczy o istnieniu jakiegoś związku logicznego. [. . .] Instrumentem, który ułatwia, a nawet umożliwia niekiedy wykrywanie związków logicznych jest algebra logiczna. [. . .] Matematycy nie bardzo zajmowali się dotąd algebrą logiczną, bo nie dbali o związki logiczne, jakie mogą zachodzić wśród prawd przez nich wykrytych. Nie wiedzieli nawet, które z tych prawd należy uważać za zasady, a które za twierdzenia. Wystarczyło im, że jakieś twierdzenie udowodnili. Dopiero od niedawna odczuwają potrzebę uporządkowania logicznego materiałów, jakie nagromadziły się w matematyce w ciągu wieków. Pracę tę podjęli przede wszystkim ci z matematyków, którzy są zarazem logikami [. . .].’

[35] The fragment starting with the words ‘Logic is filled’ [Logika jest przepełniona] repeats Zaremba’s words concerning mathematics—remark is mine.

[36] ‘Wydaje się niejednemu, że logika jest przepełniona subtelnościami, które przed zdrowym rozsądkiem nie znajdują żadnego usprawiedliwienia; subtelności te bez żadnej potrzeby utrudniają poznanie tego, co ma istotne znaczenie naukowe. [. . .] Zarówno o matematyce, jak o logice są takie sądy niesłuszne. To, co laikowi może się wydawać subtelnością logiczną, jest tylko postulatem ścisłości naukowej. Ścisłość ta nie tylko nie utrudnia poznania prawd naukowo wartościowych, lecz przeciwnie—je ułatwia.’

[37] ‘Matematyka, która uchodziła dotąd za naukę najściślejszą, okazuje się pełna braków i błędów, gdy przyłożymy do niej tę nową miarę ścisłości.’

[38] This Supplement was—as Woleński stresses in *Przedmowa* [Foreword] to the second edition of this book—the first Polish textbook on mathematical logic. For many Polish philosophers it ‘was the first competent source of information about the new logic’ (p. XXVII).

> (α) This logic constitutes a system of truths, properly justified and formulated in strict symbols, similarly as a system of any mathematical truths. [. . .] symbolic logic has at least the same value as those mathematical sciences have. [. . .]
>
> (β) the very fact that *non-mathematical questions can be expressed by symbols of mathematical exactness* gives it [symbolic logic] a significant theoretical value. Namely, the conviction that only mathematics and mathematical physics are 'exact' sciences turns out to be incorrect. Logic as a science is equally exact as mathematics. And in the symbolic treatment of logic *some deeper affinity* between logic and mathematics is revealed; this affinity leads to the view that all *a priori* sciences grow out of one stem. [. . .]
>
> (γ) For symbolic logic is also valuable as an *unequally more exact and more complete theory of logical facts* than the traditional formal logic. For the first time there appears to be an attempt to define strictly and to embrace fundamental logical principles [. . .]. A huge number of new logical questions arise. [. . .]
>
> (δ) Yet, symbolic logic is also very valuable for the *practice* of scientific thinking. [. . .] So will symbolic logic prove indispensible at all places where more complicated logical problems, which cannot be solved with the aid of ordinary, everyday means of thinking, will occur[39] (1910, pp. 181–183).[40]

Speaking of Łukasiewicz's views on logic we should mention his clearly anti-psychological attitude. Psychologism, being a popular approach in the philosophy of logic and mathematics at the end of the nineteenth century, claimed that the

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[39] Let us add as a curious detail the final remarks of Łukasiewicz who distances himself from vague philosophical speculations and juxtaposes them with the solid science of logic, writing, 'Nevertheless, symbolic logic will never be popular with certain kinds of philosophers. Since creating lofty syntheses in beautiful-sounding words is a nice and graceful thing. But one must learn symbolic logic; one must learn it like mathematics, using a pencil, not omitting any letter, not skipping over any proof. One should desire and have the skill to conduct scientific work. And it is too dry and boring for minds longing for the absolute' (1910, p. 184).

('Mimo wszystko logika symboliczna nie będzie nigdy wśród pewnego rodzaju filozofów popularna. Tworzyć bowiem w pięknie brzmiących słowach syntezy pełne polotu to rzecz miła i wdzięczna. Ale logiki symbolicznej trzeba się nauczyć, trzeba się jej uczyć tak jak matematyki, z ołówkiem w ręku, nie opuszczając żadnej literki, nie przeskakując żadnego dowodu. Trzeba chcieć i umieć pracować naukowo. A to jest praca zbyt sucha i nudna dla umysłów tęskniących za absolutem.')

[40] '(α) Logika ta stanowi *system prawd*, należycie uzasadnionych i ujętych w ścisłą symbolikę, podobnie jak system jakichkolwiek prawd matematycznych. [. . .] logika symboliczna ma przynajmniej taką samą wartość, jaką posiadają owe nauki matematyczne. [. . .]

(β) już sam fakt, iż można w *symbole o ścisłości matematycznej ująć zagadnienia niematematyczne*, nadaje jej doniosłą wartość teoretyczną. Błędnym mianowicie okazuje się przekonanie, że tylko matematyka i fizyka matematyczna są naukami "ścisłymi". Logika jest nauką równie ścisłą jak matematyka.—Objawia się przy tym w symbolicznym traktowaniu logiki jakieś głębsze pokrewieństwo między nią a matematyką, które prowadzi do poglądu, że wszystkie nauki aprioryczne z jednego pnia wyrastają. [. . .]

(γ) Logika symboliczna ma wszakże ponadto wartość jako *nierównie ściślejsza i pełniejsza teoria faktów logicznych* niż tradycyjna logika formalna. Po raz pierwszy pojawia się tu próba ścisłego określenia i uchwycenia podstawowych zasad logicznych [. . .]. Pojawia się ogromna ilość nowych zagadnień logicznych. [. . .]

(δ) Ale i dla *praktyki* myślenia naukowego posiada logika symboliczna równie wielką wartość. [. . .] Tak samo logika symboliczna okaże się niezbędna wszędzie tam, gdzie pojawią się zawilsze zadania logiczne, których nie będzie już można rozwiązać za pomocą zwykłych, codziennych środków myślenia.'

objects, which these sciences investigated, existed as psychological beings and were got to be known like other psychological facts. This conception was criticised by Frege, Husserl and Meinong. Łukasiewicz referred to the criticism of the latter two in his paper 'Logika a psychologia' [Logic vs. Psychology] (1907), firmly opting for anti-psychologism. In the paper, he argued that psychological laws could not be the reasons for the laws of logic since the former—as empirical—are probable whereas the laws of logic are certain. The laws of logic and the laws of psychology have different contents: the laws of logic concern relations between the truth and falsity of judgements whereas the laws of psychology state the relations between psychological phenomena, and after all, the concept of truth and the concept of falseness do not belong to psychology. According to Łukasiewicz, the source of psychologism can be the use of certain ambiguous concepts. However, from the perspective of psychology thinking and judgement are different from their understanding as objects of logic. The fact that logic analyses the conditions of exact thinking, and thinking is a psychological activity, does not lead to the conclusion that logic is a part of psychology. Łukasiewicz concludes:

> Exposing the attitude of logic towards psychology can be to the advantage of both sciences. Logic will be cleared from the weeds of psychologism and empiricism, which choke its right development and the psychology of cognition will get rid of *a priori* traces, which hid the light of the sincere splendour of its truths. Since one should remember that logic is an *a priori* science, like mathematics, whereas psychology, like any other natural science, is based and must be based on experience (1907, p. 491).[41]

Łukasiewicz's argumentation against psychologism was recognised and widely approved among Polish logicians. Consequently, among other things anti-psychologism meant the unacceptability of psychological explanations of the certainty of logical theorems. Łukasiewicz stressed the apriorism of logic. In his paper 'O twórczości w nauce' [On Creativity in Science] he wrote:

> Logic is an *a priori* science. Its theorems are true by virtue of definitions and axioms flowing from reason and not from experience. This science is a domain of pure mental creativity. [. . .] Logical and mathematical judgements are truths only in the world of ideal beings. We will never know whether some real objects correspond with these beings.
>
> *A priori constructions of the mind, being part of every synthesis, imbue the whole science with an ideal and creative element* (1912a, pp. 13–14).[42]

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[41] 'Wyświetlenie stosunku logiki do psychologii przynieść może korzyści obu tym naukom. Logika oczyści się z chwastów psychologistycznych i empirystycznych, które tłumią jej prawidłowy rozwój, a psychologia poznania pozbędzie się naleciałości apriorycznych, spod których szczery blask jej prawd nie mógł jakoś dotąd zajaśnieć. Należy bowiem pamiętać, że logika jest nauką aprioryczną, tak jak matematyka, a psychologia, tak jak każda nauka przyrodnicza, opiera się i opierać się musi na doświadczeniu.'

[42] 'Logika jest nauką aprioryczną. Twierdzenia jej są prawdziwe na mocy określeń i pewników płynących z rozumu, nie z doświadczenia. Nauka ta jest dziedziną czystej twórczości myślowej. [. . .] Sądy logiczne i matematyczne są prawdami jedynie w świecie bytów idealnych. Czy bytom tym odpowiadają jakieś przedmioty rzeczywiste, o tym zapewne nigdy się nie dowiemy.

Aprioryczne konstrukcje umysłu, wchodząc w skład każdej syntezy, przepajają całą naukę pierwiastkiem idealnym i twórczym.'

Thus we come to the next issue, which is important to the philosophy of logic and mathematics, namely, to the problem: logic and mathematics versus reality. Łukasiewicz changed his views in this respect many times. From the above-mentioned view that logic as an *a priori* science and 'domain of pure mental creativity' has no connection with experience, he began—influenced by the questions that came up after he had created the systems of many-valued logic, i.e. alternative to classical two-valued logic—to believe that logical systems can have ontological interpretation and that *a priori* systems must be verified on facts, analogically to physical hypotheses, they should be 'continually confronted with the data of intuition and experience as well as the results of other sciences, especially the natural ones' (1936, p. 123). He wrote:

> We know today that not only do different systems of geometry exist, but different systems of logic as well, and they have, moreover, the property that one cannot be translated into another. I am convinced that one and only one of these logical systems is valid in the real world, that is, is real, in the same way as one and only one system of geometry is real. Today, it is true, we do not yet know which system that is, but I do not doubt that empirical research will sometime demonstrate whether the space of the universe is Euclidean or non-Euclidean, and whether relationships between facts correspond to two-valued logic or to one of the many-valued logics. All *a priori* systems, as soon as they are applied to reality, become natural-science hypotheses which have to be verified by facts in a similar way as is done with physical hypotheses (1970, p. 233). [43]

Łukasiewicz contrasted his standpoint with the one of the Vienna Circle, in particular the views of Carnap and Wittgenstein, i.e. the conception interpreting logic as a set of tautologies devoid of empirical content.

In his work 'W obronie logistyki' [Defending Logistics], published a year later (1937a), he again changed his standpoint. Refuting the objection to pragmatism he claimed:

> I do not accept pragmatism as a theory of truth, and I think that no reasonable person would accept that doctrine. Nor have I ever thought of verifying pragmatically the truth of logical systems. Those systems do not need such a verification. I well know that all logical systems which we construct are necessarily true under the assumptions made in their construction. The only point would be to verify the ontological assumptions that underlie logic, and I think that I act in accordance with the methods universally adopted in natural science if I strive to verify the consequences of those assumptions in the light of facts (1970, p. 247).[44]

[43] 'Wiemy dziś, że nie tylko istnieją różne systemy geometrii, ale i różne systemy logiki, które w dodatku mają tę właściwość, że nie można jednego z nich przełożyć na drugi. Wierzę, że jeden i tylko jeden z tych systemów logicznych zrealizowany jest w świecie rzeczywistym, czyli jest realny, tak jak jeden i tylko jeden system geometryczny jest realny. Nie wiemy dziś wprawdzie, który to jest system, ale nie wątpię, że badania empiryczne wykażą kiedyś, czy przestrzeń światowa jest euklidesowa, czy jakaś nieeuklidesowa, i czy związek jednych faktów z drugimi odpowiada logice dwuwartościowej, czy jakiejś wielowartościowej. Wszystkie systemy aprioryczne, z chwilą gdy stosujemy do rzeczywistości, stają się hipotezami przyrodniczymi, które sprawdzać należy na faktach w podobny sposób jak hipotezy fizykalne' (1936, p. 128).

[44] 'Nie uznaję pragmatyzmu jako teorii prawdy i sądzę, że nikt rozsądny nie uzna tej doktryny. Nie myślałem też o tym, by sprawdzać pragmatystycznie prawdziwość systemów logicznych. Sprawdzania takiego systemy te nie potrzebują. Wiem dobrze, że wszystkie systemy logiczne,

Thus it is not the theorems of logical system that should be verified empirically but the profound ontological premises lying as its basis, for instance the principle of bivalence.

In his publication 'On the Intuitionistic Theory of Deduction' (1952) Łukasiewicz returned to his views, formulated 40 years earlier:

> We have no means to decide which of the n-valued systems of logic, $n > 2$, is true. Logic is not a science of the laws of thought or of any other real object; it is, in my opinion, only an instrument which enables us to draw asserted conclusions from asserted premises. [. . .] The more useful and richer a logical system is, the more valuable it is (1952, p. 206).

These sentences seem to suggest that Łukasiewicz again accepted pragmatism and relativism, i.e. the views that he had refuted.

We have mentioned many-valued logics. In the context of this book, it is worth considering other philosophical views of Łukasiewicz related to these alternative logics. Let us begin by stating that in Poland the philosophical context of the formulation of many-valued logics was connected with discussions concerning determinism, indeterminism and modalities, such as possibility or necessity, and finally discussion on liberty. In his rector's speech delivered at Warsaw University during the inauguration of the academic year 1922/1923 (cf. 1922) Łukasiewicz opted for the eternity of truth, rejecting its pre-existence. This thesis leads to the conclusion:

> [. . .] there are propositions which are neither true nor false but *indeterminate*. All sentences about future facts which are not yet decided belong to this category. Such sentences are neither true at the present moment, for they have no real correlate, nor are they false, for their denials too have no real correlate. If we make use of philosophical terminology which is not particularly clear, we could say that ontologically there corresponds to these sentences nether being nor non-being but possibility. Indeterminate sentences, which ontologically have possibility as their correlate, take the third truth-value.
>
> [. . .] determinism is not a view better justified than indeterminism.
>
> Therefore, without exposing myself to the charge of thoughtlessness, I may declare myself for indeterminism. I may assume that not the whole future is determined in advance (1970, pp. 126–127). [45]

które tworzymy, są przy tych założeniach, przy jakich je tworzymy, z konieczności prawdziwe. Chodzić może tylko o sprawdzenie założeń ontologicznych tkwiących gdzieś na dnie logiki, i myślę, że postępuję zgodnie z metodami przyjętymi powszechnie w naukach przyrodniczych, jeśli chcę konsekwencje tych założeń sprawdzać jakoś na faktach' (1937a, p. 162).

[45] '[. . .] istnieją zdania, które nie są ani prawdziwe, ani fałszywe, tylko jakieś *obojętne*. Takimi są wszystkie zdania o faktach przyszłych, które nie są jeszcze obecnie przesądzone. Zdania te nie są w chwili obecnej prawdziwe, bo nie mają żadnego realnego odpowiednika, ani też nie są fałszywe, bo ich zaprzeczenia także nie mają realnego odpowiednika. Posługując się niezbyt jasną terminologią filozoficzną, można by powiedzieć, że zdaniom tym nie odpowiada ontologicznie ani byt, ani niebyt, lecz *możliwość*. Zdania obojętne, którym ontologicznie odpowiada możliwość, mają trzecią wartość logiczną.

[. . .] determinizm nie jest poglądem lepiej uzasadnionym od indeterminizmu.

Wolno nam tedy, nie narażając się na zarzut lekkomyślności, opowiedzieć się przy indeterminizmie. Wolno nam przyjąć, że nie cała przyszłość jest z góry ustalona' (1922, p. 125).

At the same, time Łukasiewicz accepted the existence of propositions to which he assigned the third logical value (different from truth or falsity), which had nothing to do with the rejection of the principle of contradiction or the principle of excluded middle. The very principle of bivalence is a principle of metalogic and not a law of logic.

Łukasiewicz's motivation to take up many-valued logics was (at least partly) philosophical whereas the very systems of such a logic are, according to him, independent from philosophy. In his work 'W obronie logistyki' (1937a) Łukasiewicz clearly states that the systems of many-valued logic 'do not depend on any philosophical doctrine for they would fall with the collapse of the doctrine, but are as much an objective result of research as any established mathematical theory' (1970, p. 246). [46] On the other hand, these systems can be of philosophical significance. Among other things, Łukasiewicz connected them with the question whether there were degrees of possibility and what their number was. Assuming a negative answer we are dealing with a system of three-valued logic whereas assuming the existence of such degrees 'it is most naturally to adopt, like in probability calculus, that there are infinitely many degrees of possibility, which leads to infinitely-many-valued system of propositional calculus' (1930; quoted from Łukasiewicz 1961, p. 159).

Łukasiewicz attributed an important role to systems of many-valued logic:

> One can hardly predict what influence the creation of non-Chrysippus'[47] systems of logic will exert on philosophical speculation. However, it seems to me that the philosophical significance of the presented systems of logic can be at least equally great as the significance of non-Euclidean systems of geometry (1961, p. 161).[48]

In his talk delivered during the meeting of the Circle for Science Studies [Koło Naukoznawcze] in 1938, he said that '[e]very such logic may be the basis of slightly different mathematics and every such mathematics—basis of slightly different physics'[49] (1939, p. 215). In the paper 'Die Logik und das Grundlagenproblem' (1941) he proposed that many-valued systems of logic became the basis of research in arithmetic and set theory.

Thus Łukasiewicz attributed a double meaning: philosophical and mathematical to many-valued logics. Yet, these logics did not play the role their creator meant for them but they certainly widened the repertoire of investigations on logical systems to a large extent.

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[46] 'nie zależą od żadnej doktryny filozoficznej, z której upadkiem musiałyby upaść, lecz są równie obiektywnym rezultatem badań, jak każda ustalona teoria matematyczna' (1937a, p. 162).

[47] Łukasiewicz uses this term to describe many-valued logics, thus opposing to call them non-Aristotelian logics—remark is mine.

[48] 'Niełatwo przewidzieć, jaki wpływ wywrze powstanie niechryzypowych systemów logiki na spekulację filozoficzną. Wydaje mi się jednak, że znaczenie filozoficzne przedstawionych tutaj systemów logiki może być co najmniej równie wielkie jak znaczenie nieeuklidesowych systemów geometrii.'

[49] Zawirski, combining the ideas of Łukasiewicz and E.L. Post, tried to construct a system of logic which would be suitable to probability calculus and certain physical problems—cf. Sect. 3.3.

One of the important problems of the philosophy of mathematics and logic is the question of the mode of existence of objects that logic and mathematics analyse. Numerous Polish logicians, for example Chwistek, Leśniewski, Kotarbiński or Tarski, inclined towards nominalism (cf. Sect. 2.2, Chap. 2 and Sects. 3.4, 3.5, 3.7). Łukasiewicz's position was different. He admitted (e.g. 1936, p. 119) that mathematical logic put on a nominalistic robe. Since it does not speak about concepts and judgements but names and propositions. However, it treats the latter as inscriptions of defined shapes. It results from the fact that mathematical logic aims at formalization and wants to present all logical argumentations in such a way that 'their compatibility with the rules of deduction, i.e. transformation of inscriptions, can be controlled without referring to the meaning of the inscriptions' (1936, p. 119).

However, such a nominalistic approach raises certain doubts, which Łukasiewicz expresses. Now an individual can create only a finite number of inscriptions. Hence a set of inscriptions is finite whereas logical and mathematical systems consist of an infinite number of theses. How can these facts be reconciled? One can say that only the theses that someone has written do exist. Then this set of theses will be really finite, but 'on this basis it would be as difficult to practise formal logic, especially metalogistics, as to build arithmetic on the assumption that the set of natural numbers is finite' (1936, p. 120). It would also lead to make logic dependent on certain empirical facts, i.e. on the existence of inscriptions, which is difficult to accept. The problem will not be solved if the creations of human activities and all physical bodies of definite shape and quantity are regarded as inscriptions and if one assumes that the number of such bodies is infinite—as proposed by Tarski. Then 'we would make logic dependent on a pretty unlikely physical hypothesis, which is not desirable in any case' (1936, p. 120).

Łukasiewicz thought that the nominalism of logic was virtual. Moreover, logic was developed without solving the problem of its nominalism. In his paper 'Logistyka a filozofia' [Logistics and Philosophy] he wrote:

> We have so far been little worried by these difficulties, and this is the strangest point. It was so probably because, while we use nominalistic terminology, we are not true nominalists but incline toward some unanalysed conceptualism or even idealism (1970, p. 224).[50]

Łukasiewicz himself thought that the objects that logic investigated existed only beyond the sphere of inscriptions. He did not develop some alternative to nominalism—he just formulated his personal view. However, his view resulted from his personal religious convictions—influenced by these convictions Łukasiewicz opted for the Neo-Platonic interpretation of logic. In 'W obronie logistyki' he wrote:

> In concluding these remarks I should like to outline an image which is connected with the most profound intuitions which I always experience in the face of logistic. That image will

[50] 'Mało dotychczas przejmowaliśmy się tymi trudnościami i to jest w tym wszystkim najdziwniejsze. Działo się to chyba dlatego, że używając terminologii nominalistycznej, nie jesteśmy naprawdę nominalistami, lecz hołdujemy jakiemuś nie zanalizowanemu konceptualizmowi czy nawet idealizmowi' (1936, p. 120).

> perhaps shed more light on the true background of that discipline, at least in my case, than all discursive description could. Now, whenever I work even on the least significant logistic problem, for instance, when I search for the shortest axiom of the implicational propositional calculus I always have the impression that I am facing a powerful, most coherent and most resistant structure. I sense that structure as if it were a concrete, tangible object, made of the hardest metal, a hundred times stronger than steel and concrete. I cannot change anything in it; I do not create anything of my own will, but by strenuous work I discover in it ever new details and arrive at unshakable and eternal truth. Where is and what is that ideal structure? A believer would say that it is in God and is His thought (1970, p. 249).[51]

Łukasiewicz clearly stresses that those remarks express his personal view. He also thinks that logic is neither called nor allowed to solve the eternal philosophical debate concerning universals. Consequently, logicians and mathematicians, who claim that these sciences are nominalistic, formulate these theses groundlessly.

Łukasiewicz refutes the accusation that conventionalism lies at the basis of mathematical logic. He regarded the argument that systems of logic 'are not constraint in the structure of their axiomatic systems by any absolute rules or ideas, but are built in an arbitrary way' (1937a, p. 22; 1970, p. 242) as pointless. He shows that on the example of propositional calculus and syllogistic:

> In choosing this or that system of axioms out of all possible ones, we need not be constrained by any absolute principles, for we know in advance that such principles, e.g., the principle of consistency, are satisfied by all systems of axioms, and we are guided only by practical or didactic considerations. I do not see in all this even a trace of conventionalism, which I never favoured and do not favour now. To put it simply, the two-valued propositional calculus has the property that it can be constructed axiomatically in different ways, and that property is a logical fact which does not depend on our will and which we have to accept whether we like it or not (1970, p. 243).[52]

The above-mentioned analyses show that although some logical investigations of Łukasiewicz were motivated by philosophical problems (for instance, many-

[51] 'Chciałbym na zakończenie tych uwag nakreślić obraz związany z najgłębszymi intuicjami, jakie odczuwam zawsze wobec logistyki. Obraz ten rzuci może więcej światła na istotne podłoże, z jakiego przynajmniej u mnie wyrasta ta nauka niż wszelkie wywody dyskursywne. Otóż ilekroć zajmuję się najdrobniejszym nawet zagadnieniem logistycznym, szukając np. najkrótszego aksjomatu rachunku implikacyjnego, tylekroć mam wrażenie, że znajduję się wobec jakiejś potężnej, niesłychanie zwartej i niezmiernie odpornej konstrukcji. Konstrukcja ta działa na mnie jak jakiś konkretny dotykalny przedmiot, zrobiony z najtwardszego materiału, stokroć mocniejszego od betonu i stali. Nic w niej zmienić nie mogę, nic sam dowolnie nie tworzę, lecz w wytężonej pracy odkrywam w niej tylko coraz to nowe szczegóły, zdobywając prawdy niewzruszone i wieczne. Gdzie jest i czym jest ta idealna konstrukcja? Filozof wierzący powiedziałby, że jest w Bogu i jest myślą Jego' (1937a, p. 165).

[52] 'W wyborze takiego czy innego z możliwych układów aksjomatycznych nie mamy żadnej potrzeby krępować się jakimiś zasadami bezwzględnymi, bo wiemy już z góry, że takie, np. zasada niesprzeczności, spełnione są przez wszystkie układy, a kierujemy się tylko względami natury praktycznej czy pedagogicznej. Nie widzę w tym wszystkim ani odrobiny konwencjonalizmu, którego zwolennikiem nigdy nie byłem i nie jestem. Mówiąc po prostu, jest to pewna własność dwuwartościowego rachunku zdań, że można go zbudować aksjomatycznie w wieloraki sposób, a własność ta jest faktem logicznym, który od woli naszej nie zależy i na który chcąc nie chcąc zgodzić się musimy' (1937a, p. 22).

valued logics), generally, he separated logic from philosophy. Basically, every formal problem can be investigated. He clearly distinguished between formal constructions and exact logical and metalogical investigations of logical systems on the one hand, and their possible philosophical interpretations on the other hand. This position was shared by most Polish logicians of the interwar period.

Łukasiewicz's standpoint was accurately characterised by Sobociński:

> He did not try to construct a definite system of the foundations of the deductive sciences. His aims were, on the one hand, to provide exact and elegant structures for many domains of our thinking where such had either been wanting or insufficient; and on the other, to restore the vital historical dimension to logic (1956, p. 42).

3.3 Zygmunt Zawirski

Zygmunt Zawirski dealt mainly with the philosophy of science—he was interested in methodological, epistemological and ontological problems, which originated as a result of the development of physics, first of all the formulation of the theory of relativity and quantum mechanics. Since these issues do not belong to the fundamental subject matter of this book we will not devote much attention to them. We refer all those interested in Zawirski's works concerning the border line of physics and philosophy to the publication edited by Irena Szumilewicz-Lachman (1994) and the monographs by Jan Woleński (1985, 1989). At this point, it is sufficient to say that Zawirski engaged in polemics with the philosophical trends prevailing towards the end of the nineteenth century: neo-Kantianism and Empirio-criticism, and like most scholars of the Lvov-Warsaw School he also referred to the idea of the Vienna Circle. However, he discarded certain extremes, in particular he did not agree with the thesis of the Vienna Circle that traditional philosophical problems should be rejected and the object of philosophy should be reduced to the analysis of language. The questions of time occupied an important place in Zawirski's research. He wrote *L'Evolution de la notion du temps* (1936a), regarded as his *opus magnum* for which he received the Eugenio Rignano prize, announced by the Italian periodical *Scientia*. As far as the philosophy of science is concerned he opted for moderate realism as well as he appreciated the role and significance of both induction and deduction in natural sciences. Additionally, he dealt with issues connected with the application of the results of formal sciences in the investigations of exact sciences, especially the problem of the axiomatisation of fragments of physics (cf. 1927b, 1938a, 1948).

The issues of the philosophy of mathematics and logic were rather of minor importance in Zawirski's works. Nevertheless, he dealt with certain questions of this domain. One can differentiate two circles of problems: the relations between logic and mathematics as well as the significance of non-classical logics, in particular many-valued logics and intuitionistic logic. The main objective of his works was to inform the philosophical environment about the current achievements in these domains and to correlate them with the investigations conducted in Poland.

Thus he seldom formulated his own ideas clearly, limiting himself to referring the views of other scholars and reflecting on their importance. Naturally, it does not mean that he never formulated his own opinions—he did so in the area of problems related to many-valued logics and their possible applications.

Let us begin with the first mentioned problem on which Zawirski focused, namely the relationships between logic and mathematics. He reflected on them in his work 'Stosunek logiki do matematyki w świetle badań współczesnych' [The Relation between Logic and Mathematics from the Point of View of Contemporary Investigations], claiming that:

> Mathematics, as an exact science, was created much earlier than logic; the Greek had known how to construct proper mathematical proofs before systematic investigations on the essence of all logical deduction and argumentation began (1927a, p. 171).[53]

Then he analyses the development of logic, emphasizing—which was also stressed by Łukasiewicz—the importance of the Stoics' logic. He claims that it was more important to mathematics than Aristotle's logic. He appreciates the works of Leibniz, Peano and Frege whereas he refutes Kant's conceptions, thinking that the Kantian conception of pure mathematical theorems as *a priori* synthetic propositions 'put the relationship between logic and mathematics in a weird and mysterious light':

> By recognising the judgements of mathematics as *a priori* synthetic Kant accepted the contribution of non-logical factors in mathematical thinking, namely he accepted in this thinking the necessity to refer to intuition as well as to *a priori* forms of time and space (1927a, p. 173).[54]

The principal part of Zawirski's work is devoted to the presentation and analysis of Whitehead and Russell's work *Principia Mathematica* (1910–1913) and Russell's works *The Principles of Mathematics* (1903) and *Introduction to Mathematical Philosophy* (1919). He discusses in detail the content of *Principia* and its logicist thesis that mathematics can be reduced to logic. He also stresses the role and meaning of the axiom of reducibility, the axiom of choice and the axiom of infinity, noting that Russell and Whitehead use the latter two only conditionally (i.e. as antecedents of implications). Since they are actually axioms of existence (postulating the existence of certain objects), and 'no logical principle can introduce existence otherwise than in a hypothetical form' (1927a, p. 202). That is why there is no proof of existence of any single object in *Principia*. Zawirski writes:

[53] 'Matematyka, jako nauka ścisła, powstała znacznie wcześniej aniżeli logika; Grecy umieli budować poprawne dowody matematyczne, zanim jeszcze zaczęły się systematyczne badania nad istotą wszelkiego logicznego wnioskowania i dowodzenia.'

[54] 'Kant uznając sądy matematyki za syntetyczne a priori, przyjmował tym samym udział czynników pozalogicznych w myśleniu matematycznym, mianowicie przyjmował konieczność odwoływania się w nim do intuicji, do apriorycznych form czasu i przestrzeni.'

> Such a proof, if it had existed in *Principia*, would not have been better than the ontological argument for the existence of God. (1927a, p. 204).[55]

He agrees with Russell's opinion:

> There is only one concept of existence for all sciences and a mathematician does not use this concept in a different meaning than a physicist (1927a, p. 203).[56]

Since from the point of view of logicism mathematics and logic do not differ in a fundamental way, it is of no greater importance whether the judgements of both sciences are regarded as analytic or synthetic. Thus it is not essential whether we recognise the theorems of mathematics as tautologies or not. What is important is the problem of the consistency and independence of axioms. However, Russell did not deal with this question[57] and 'nowhere in his work does he formulate a proof that the set of axioms of logic and mathematics, he gives, creates an independent and consistent system' (1927a, p. 205). This question was considered—as Zawirski informs—by Hilbert and his followers.

Zawirski also analysed the consequences of logicism for applied mathematics, in particular he was interested in the consequences for theoretical physics. He wrote:

> Russell can see the difference between mathematics and theoretical physics in the fact that physical constants cannot be reduced to logical constants, just as mathematical constants can. However, if the geometrisation of physics as well as Hilbert's and Weyl's dreams of reducing physical constants to mathematical ones could be fulfilled, the difference that Russell can see would be only transitional, and the essential difference should be seen only in the fact that in physics the axioms of existence cannot occur in a hypothetical form (1927a, p. 206).[58]

Zawirski also reflected on this issue in 'Nauka i metafizyka' [Science and Metaphysics] published on the basis of his script discovered only in the years 1995–1996. Writing about the cognition of the world he states that deductive sciences deal with formal objects whereas in the cognition of the world:

> [. . .] we do not mean 'formal' objects, which mathematics deals with; here we do not mean only the very *ens,* but *ens existens,* and one can learn about existences only on the empirical way (1995, p. 133).[59]

[55] 'Dowód taki, gdyby w *Principiach* istniał, nie byłby lepszy od dowodu ontologicznego istnienia Boga.'

[56] 'Jedno jest tylko pojęcie istnienia dla wszystkich nauk i matematyk nie operuje tym pojęciem w innym znaczeniu niż fizyk.'

[57] Russell did not distinguish between language and metalanguage, between theory and metatheory. Consequently, he did not formulate metatheoretical and metalogical questions.

[58] 'Russell widzi różnicę między matematyką a fizyką teoretyczną w tym, iż stałe fizykalne nie dadzą się sprowadzić do stałych logicznych, podobnie jak stałe matematyczne. Jeśli by jednak geometryzacja fizyki i marzenia Hilberta i Weyla o sprowadzeniu stałych fizykalnych do stałych matematycznych dały się urzeczywistnić, wówczas różnica, jaką widzi Russell, byłaby tylko przejściowa, a istotnej należałoby się dopatrywać jedynie w tym, iż w fizyce aksjomaty istnienia nie mogą występować w formie hipotetycznej.'

[59] '[. . .] nie chodzi o przedmioty "formalne", jakimi zajmuje się matematyka; tu nie chodzi tylko o samo *ens*, ale o *ens existens*, a o egzystencjach można się dowiedzieć tylko na drodze empirycznej.'

Yet, mathematics and logic do influence our cognition of the world. Since mathematical theories can be interpreted, which is done through ordering objects or relationships between objects of the physical world to mathematical symbols. Thus mathematical constructions become elements of physical theories and the interpreted mathematical theorems can be empirically verified. Hence one can see the role and significance of mathematics and its methods for natural sciences. Zawirski dedicated much attention to the axiomatizability of such theories, for example his works 'Metoda aksjomatyczna a przyrodoznawstwo' [The Axiomatic Method and Natural Science] (1923–1924), 'Próby aksjomatyzacji fizyki i ich znaczenie filozoficzne' [Attempts of Axiomatization of Physics and Their Philosophical Significance] (1927b), 'Doniosłość badań logicznych i semantycznych dla fizyki współczesnej' [The Significance of Logical and Semantic Investigations for Modern Physics] (1938a) or 'Uwagi o metodzie nauk przyrodniczych' [Remarks on the Method of Natural Sciences] (1948).

He was also interested in the problem of the relationship between physics and geometry. Since both domains analyse space, which is at least claimed in the classical approach (before the creation of the non-Euclidean geometries). By applying the axiomatic method in physics the difference between physics and geometry allegedly disappears: physics becomes interpreted geometry (Schlick) and geometry—a natural science (Einstein, Born). According to Zawirski, physics and geometry are separate sciences and their autonomy can be reconciled despite the existing differences between their objects and applied method. Geometry constructs its object independently from experience and concretely existing physical reality, and it justifies its theorems only through deduction. Whereas physics deals with objects that are given in experiments and generally formulates its laws through the inductive method. When an appropriate physical theory is formulated, particular theses (laws worked out through experiments) are justified in it; the laws are constructed deductively from the accepted theses or axioms, which occurs naturally only in the context of justification.[60] Additionally, Zawirski stresses that the laws accepted as axioms must have certain empirical justification. In geometry, just like in the whole mathematics, the ideas of certain presumptions and laws can have an empirical source but we can accept them only just when they are deducted from axioms; experience has no justifying power in mathematics. Now, there is a certain formal link between the laws of physics and the laws of the geometry of the bodies to such an extent that the latter analyses the spatial properties of physical objects.

Discussing the relationship between logic and mathematics it is worth explaining how Zawirski understood logic itself. The answer can be found in his textbook *Logika teoretyczna* [Theoretical Logic]:

> Besides terms that are proper only for single sciences there are terms common for all sciences. Such terms include [...] logical terms and they cause that logic is a general

[60] At this point it is worth adding that Zawirski was more interested in the context of justification than the context of discovering.

> science and recounts the structure, which is common to all sciences, recounts the way every single science justifies its theorems (1938b, p. 2).[61]

On the previous page of the textbook he states:

> The name of the science, which is now called logic, comes from the Greek *logos*, i.e. 'word,' 'speech' and 'reason' as well as 'reasonable thinking'; the name of the science is associated exactly with the last meaning. Since it is not a science about reason but rather about forms of reasoning that we use in all deductions or argumentations.[62]

The quoted fragments show that Zawirski understood the scope of logic in a wide way: not only as a formal system (as a group of such systems), but he included the science on argumentations into its scope. It must have reflected the existing customs and didactic practice in Poland (and other countries). As Woleński writes, recalling the example of Łukasiewicz's textbook of mathematical logic (1929a) and Jaśkowski's textbook (1947), 'for a long time even the courses of mathematical logic for mathematicians ended with an explication on argumentations in natural sciences' (1999, p. 64).

Let us proceed to the other issue, namely to non-classical logics. At this point Zawirski focused on intuitionistic logic and many-valued logics. The former was described in 'Geneza i rozwój logiki intuicjonistycznej' [The Origin and Development of Intuitionistic Logic] (1946). The work is rather of an informational character and aims at showing Polish readers the new results. Zawirski presents the basic ideas lying at the foundations of intuitionism, and discusses widely the views and works of the creator of intuitionism Luitzen Egbertus Jan Brouwer. Then he describes Arend Heyting's attempt to construct a system of intuitionistic logic, based on the ideas of Brouwer. He also presents Kurt Gödel's result: on the inadequacy of finite-valued matrices, and the results of Stanisław Jaśkowski, who constructed adequate infinite-valued matrices. Zawirski limits himself to discussing—in a very competent way—the effects of other people's investigations, not mentioning his own sympathies or antipathies towards intuitionistic logic.

The issue of many-valued logics looks differently. Zawirski was profoundly interested in them. He himself conducted research in this area, hoping that his investigations would allow him to solve certain difficulties in physics.

It is commonly assumed that the idea to create many-valued logics, in which we deal with more than two logical values (truth and falsity), was born when Łukasiewicz was writing his book about Aristotle's principle of contradiction (cf. Woleński 1985, p. 116; also Sect. 3.2 dedicated to Łukasiewicz). Zawirski

[61] 'Obok terminów właściwych tylko pojedynczym naukom, istnieją terminy wspólne im wszystkim. Do nich należą [...] terminy logiczne i one sprawiają, iż logika jest nauką ogólną i zdaje sprawę ze wspólnej wszystkim naukom struktury, ze sposobu, w jaki pojedyncze nauki swoje twierdzenia uzasadniają.'

[62] 'Nazwa nauki zwanej obecnie logiką pochodzi od wyrazu greckiego *logos*, który znaczy tyle co słowo, mowa, rozum, a także i rozumne myślenie; z tym ostatnim znaczeniem wiąże się właśnie nazwa nauki. Nie jest ona bowiem nauką o rozumie, ale raczej o formach rozumowania, którymi się posługujemy we wszelkim wnioskowaniu jako też dowodzeniu.'

was vividly interested in this problem and attentively followed the discussions on many-valued logics. Initially opting for the classical two-valued logic he changed his mind with time and conducted research on the new logics. Then he was especially interested in the problem of the possibility of applying many-valued logics to solve the difficulties that appeared in physics in relation with the creation of the new theories, e.g. quantum mechanics, or with the introduction of statistical laws into physics. Therefore, he appreciated Łukasiewicz's idea. He thought that the new logic was the only way to understand the phenomena of the micro-world. Combining the ideas of Łukasiewicz and Emil Leon Post he tried to construct a system of logic that would be proper to interpret both certain problems of contemporary physics and probability calculus. He dealt with these problems in the following works: 'Próby stosowania logiki wielowartościowej do współczesnego przyrodoznawstwa' [Attempts of Applying Many-Valued Logic in Contemporary Natural Science] (1931), 'Logika trójwartościowa Jana Łukasiewicza. O logice L.E.J. Brouwera. Próby stosowania logiki wielowartościowej do współczesnego przyrodoznawstwa' [Jan Łukasiewicz's Three-Valued Logic. On the Logic of L. E. J. Brouwer. Attempts of Applying Many-Valued Logic in Contemporary Natural Science] (1932a), 'Les logiques nouvelles et le champ de leur application' (1932b), 'Znaczenie logiki wielowartościowej dla poznania i związek jej z rachunkiem prawdopodobieństwa' [The Significance of Many-Valued Logic for Cognition and Its Relationship With Probability Calculus] (1934a), 'Stosunek logiki wielowartościowej do rachunku prawdopodobieństwa' [The Relationship Between Many-Valued Logic and Probability Calculus] (1934b) or 'Über das Verhältniss der mehrwertigen Logik zur Wahrscheinlichkeitsrechnung' (1935). The system he was searching should fulfil the following conditions:

1. There should be correspondence between the new system of logic and classical logic, i.e. the tautologies of classical logic should be the tautologies of the new logic (Łukasiewicz's three-valued logic did not fulfil this condition since, for example the laws of contradiction or excluded middle are not tautologies),
2. The value of truth assigned to the proposition should be connected with its probability,
3. The value of complex proposition should be unequivocally determined by the values of its elements.

Zawirski presented his results during various conferences, including the International Congress of Philosophy in Prague in 1934 (cf. 1936b) and the International Congress of Scientific Philosophy in Paris in 1935 (cf. 1936c). At the latter he met Hans Reichenbach who had also worked on similar problems. It turned out that their approaches to probability calculus and non-classical logics were different (cf. Szumilewicz-Lachman 1994). Reichenbach interpreted some expressions of probability calculus as a kind of generalised logic. Whereas Zawirski outlined the parallelism between the expressions of probability calculus and formulas of the many-valued logics formulated by Łukasiewicz and Post, thus determining the formal compatibility of both. In his opinion probability calculus and many-valued logic should be treated as two separate systems, one system being the empirical basis for the other. Zawirski was convinced that such compatibility of many-valued logic, in particular three-valued logic, with probability calculus would allow its application in quantum mechanics. Let us add that the investigations of Patrick Suppes and Paulette Destouches-Fevrier followed this direction. Therefore, Zawirski was in a sense a forerunner of quantum logic.

3.4 Stanisław Leśniewski

Leśniewski's scientific activities can be divided into two distinct periods: the early one in the years 1911–1915 and the later one: 1916–1939. The former can be described as philosophical-grammatical and the latter as logical-mathematical. The turning point was his work *Podstawy ogólnej teorii mnogości* [Foundations of the General Theory of Sets] (1916). Leśniewski himself did not value his early works very much. Moreover, he negated his early views. In his paper 'O podstawach matematyki' [On the Foundations of Mathematics] he wrote:

> Living intellectually beyond the sphere of the valuable achievements of the exponents of 'Mathematical Logic', and yielding to many destructive habits resulting from the one sided, 'philosophical'—grammatical culture, I struggled in the works mentioned [i.e. in works from the period 1911–1915—remark is mine] with a number of problems which were beyond my powers at that time, discovering already-discovered Americas on the way. I have mentioned those works desiring to point out that I regret that they have appeared in print, and I formally 'repudiate' them herewith, though I have already done this within the university faculty, affirming the bankruptcy of the philosophical—grammatical work of the initial period of my work (1992b, pp. 197–198).[63]

Leaving the 'grammatical' style in logic and having doubts concerning the precision of the standard explication of logic, Leśniewski decided to seek a new system that should fulfil two postulates: (1) be the foundation of mathematics and (2) be constructed in such a way as not to have any ambiguity. Having that in mind, Leśniewski built three systems: propositional calculus called protothetic, calculus of names called ontology and theory of sets in the collective sense called mereology. Protothetic is a generalised propositional calculus in which quantifiers may bind propositional variables and in which variables refer to any syntactical categories defined on the basis of the categories of propositions. Ontology is a rich system based on the calculus of names. It embraces the calculus of classes, the calculus of relations and almost all problems of the system included in *Principia*

[63] 'Żyjąc umysłowo poza sferą cennych zdobyczy osiągniętych w nauce przez przedstawicieli logiki matematycznej, a ulegając licznym zgubnym nałogom, płynącym z kultury jednostronnie filozoficzno-gramatycznej, zmagałem się w pracach wymienionych [tzn. w pracach z lat 1911–1915—uwaga moja, R.M.] bezradnie z szeregiem zagadnień, przerastających moje ówczesne siły, odkrywając przy sposobności odkryte już Ameryki. Wspominam o tych pracach, pragnąc zaznaczyć, iż bardzo się martwię, iż zostały w ogóle wydane, uroczyście się wyrzec niniejszym tych prac, i stwierdzić bankructwo filozoficzno-gramatycznych poczynań pierwszego okresu swej działalności' (1927, pp. 182–183).

Mathematica by Whitehead and Russell. In turn mereology can be defined as a theory of parts and wholes.

At this point, it should be stressed that Leśniewski assumed the philosophical attitude towards logic, and just like Chwistek (cf. Sect. 2.2, Chap. 2) he was interested only in those logical issues that grew on his own philosophical concepts in the area of the foundations of mathematics. Thus he differed from the other representatives of the Warsaw School of Logic. In fact, he did not participate in the investigations of the school (the exception being equivalence propositional calculus)—his activities formed a separate trend in research.

Let us begin our reflection on Leśniewski's philosophical views connected with logic and mathematics by stating that he was (just like Chwistek) a determined nominalist. This view had a strong influence on his logical constructions in as far as the contents and form are concerned. Leśniewski regarded language as a collection of concrete inscriptions and expressions of language as finite sequences of signs. He treated two inscriptions of the same shape as two separate, different inscriptions. In his opinion there only exist as many expressions as have been written. One cannot speak of some potential existence of expressions. Consequently, a given logical system contains only so many theorems as have been written until a given moment, i.e. every logical system consists of only a finite number of theorems. Since Leśniewski did not allow the existence of any general objects, in particular common properties of individual objects. Another consequence of Leśniewski's nominalism was that equivalent systems, for example the system of propositional calculus based on negation and implication as well as the system of this calculus based on negation and alternative as baseline functors, which one treats as variants of the same logic, should henceforth be treated as two different systems. Leśniewski's systems are never something complete at a given moment. Thus one cannot investigate them, using standard methods—since these methods define the logical system as (infinite in its essence) a set of consequences of a given set of initial expressions (axioms). However, one should admit that Leśniewski's systems are perfect with respect to formalization. Leśniewski himself described his standpoint in this domain as constructive nominalism.

He connected this view with the so-called intuitional formalism. According to intuitional formalism, the language of logic—unambiguously and fully codified—always says 'something' and 'about something.' The other representatives of the Warsaw School of Logic shared this conviction.[64] Leśniewski wrote about this issue in 'Grundzüge eines neuen System der Grundlagen der Mathematik' [Fundamentals of a New System of the Foundations of Mathematics]:

> Having no predilection for various 'mathematical games' that consist in writing out according to one or another conventional rule various more or less picturesque formulae which need not be meaningful, or even—as some of the 'mathematical gamers' might prefer—which should necessarily be meaningless, I would not have taken the trouble to

[64] Woleński (1992, p. 23) claims that at this point the Warsaw logicians were influenced by Leśniewski.

> systematize and to often check quite scrupulously the directives of my system, had I not imputed to its theses a certain specific and completely determined sense, in virtue of which its axioms, definitions and final directives (as encoded for SS5), have for me an irresistible intuitive validity (1992b, p. 487).[65]

In his work 'O podstawach matematyki' [On the Foundations of Mathematics], we find the following words:

> They encouraged the disappearance of the feeling for the distinction between the mathematical sciences, conceived as deductive theories, which serve to capture various realities of the world in the most exact laws possible, and such non-contradictory deductive systems, which indeed ensure the possibility of obtaining, on their basis, an abundance of ever new theorems, but which simultaneously distinguish themselves by the lack of any connection with reality of any intuitive, scientific value (1992b, pp. 177–178). [66]

Since Leśniewski treated formal systems as a means to transmit certain information about the world and as a way to express what is intuitively true. Although this may seem not to be fully in accordance with his nominalism and radical formalism, Leśniewski did not consider these views as contradictory. Indeed, in 'Grundzüge eines neuen System der Grundlagen der Mathematik' he wrote:

> I see no contradiction, therefore, in saying that I advocate a rather radical 'formalism' in the construction of my system even though I am an obdurate 'intuitionist'. Having endeavoured to express some of my thoughts on various particular topics by representing them as a series of propositions meaningful in various deductive theories, and to derive one proposition from others in a way that would harmonize with the way I finally considered intuitively binding [. . .] (1992a, p. 487).[67]

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[65] 'Da ich keine Vorliebe für verschiedene "Mathematikspiele" habe, welche darin bestehen, dass man nach diesen oder jenen konventionellen Regeln verschiedene mehr oder minder malerische Formeln aufschreibt, die nicht notwendig sinnvoll zu sein brauchen oder auch sogar, wie es einige der "Mathematikspiele" lieber haben möchten, notwendig sinnlos sein sollen,—hätte ich mir nicht die Mühe der Systematisierung und der vielmaligen skrupulösen Kontrollierung der Direktiven meines Systems gegeben, wenn ich nicht in die Thesen dieses Systems einen gewissen ganz bestimmten, eben diesen und nicht einen anderen, Sinn legen würde, bei dem für mich die Axiome des Systems und die in den Direktiven zu diesem System kodifizierten Schluss- und Definitionsmethoden eine unwiderstehliche intuitive Geltung haben' (1929a, p. 78).

[66] 'Sprzyjało to zanikowi poczucia różnicy między naukami matematycznymi pojmowanymi jako teorie dedukcyjne, służące do ujęcia w prawa możliwie ścisłe różnorodnej rzeczywistości świata, a takimi niesprzecznymi systemami dedukcyjnymi, które zabezpieczają wprawdzie możność otrzymania na ich gruncie obfitości wciąż nowych twierdzeń, odznaczających się jednak jednocześnie brakiem jakichkolwiek łączących je z rzeczywistością walorów intuicyjno-naukowych.'

[67] 'Ich sähe keinen Widerspruch darin, wenn ich behaupten wollte, dass ich eben deshalb beim Aufbau meines Systems einen ziemlich radikalen "Formalismus" treibe, weil ich ein versteckter "Intuitionist" bin: indem ich mich beim Darstellen von verschiedenen deduktiven Theorien bemühe, in einer Reihe sinnvolle Sätze eine Reihe von Gedanken auszudrücken, welche ich über dieses oder jedes Thema hege, und die einen Sätze aus den anderen Sätzen auf eine Weise abzuleiten, die mit den Schlussweisen harmonisieren würden, welche ich "intuitiv" als für mich bindend betrachte [. . .]' (1929a, p. 78).

In Leśniewski's opinion axioms and rules of logic are true in an obvious way. However, he did not try to explain the source of this obviousness. Woleński claims that one can 'presume that he referred at this point to brentanism' (1996, p. 31).

For Leśniewski logic was the description of the most general features of a being (Kotarbiński claimed the same, being influenced by Leśniewski, cf. Sect. 3.5) and fulfils the role of a general theory of objects. This view was in accordance with the fact that the Warsaw School of Logic rejected the so-called analytic interpretation of logic, i.e. the thesis that logic and mathematics are a set of tautologies that do not say anything about the world. Logic and mathematics were thought to refer to the formal aspects of reality. Sobociński describes Leśniewski's view in such a way:

> [. . .] unlike Łukasiewicz, he [Leśniewski—remark is mine] held that one could find a "true" system in logic and in mathematics. His systematization of the foundations of mathematics was meant to be merely postulational; he wished to give, in deductive form, the most general laws according to which reality is built. For this reason, he had little use for any mathematical or logical theory which even though consistent, he did not consider to be in accord with the fundamental structural view of reality. (1956, p. 42).

Leśniewski also refuted conventionalism in the style of Henri Poincaré. In his work 'Próba dowodu ontologicznej zasady sprzeczności' [An Attempt at a Proof of the Ontological Principle of Contradiction] he wrote:

> I think it superfluous to state that the linguistic conventions which I have formulated and of which I make use in my reasoning have no connection whatever with the so-called 'conventionalism' as represented, for example, by Henri Poincaré. This form of 'conventionalism' always consists in accepting certain conventions as regards the objects about which the adherents of 'conventionalism' intend to pronounce certain theses which they can justify by means of various kinds of 'conventions'. Their 'conventions' do not pertain to the objects whose properties depend on the will of those who make up these conventions but refer to such objects which cannot be changed by any of the 'conventions' accepted with respect to those objects (1992a, p. 37.)[68]

Furthermore, Leśniewski gives space as an example—no conventions concerning its properties can change these properties because they are not dependent on those who assume them. The propositions that embrace such conventions either cannot be completely proved or checked—thus such conventions have not got the values of scientific propositions—or can be proved, and then there is no reason to accept them as conventions.

Leśniewski attributes a different character to language conventions. He claims that they are 'indispensible condition of the possibility to understand linguistic

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[68] 'Nie potrzebuję chyba zaznaczać, że konwencje językowe, które wyżej sformułowałem i na których się opieram w swych dowodzeniach, nie mają nic wspólnego z tak zwanym "konwencjonalizmem", reprezentowanym w nauce przez Henryka Poincarégo. "Konwencjonalizm" tego typu polega zawsze na przyjmowaniu tych lub innych konwencji względem przedmiotów, o których przedstawiciele "konwencjonalizmu" pragną wypowiadać pewne twierdzenia, których nie umieją uzasadnić inaczej, jak uciekając się do pomocy tych lub innych "umów". "Konwencje" "konwencjonalistów" nie dotyczą przedmiotów, których takie albo inne cechy zależne są od woli tych, którzy konwencje dane przyjmują, lecz mają za treść przedmioty, których w żadnym kierunku nie potrafią zmienić żadne przyjmowane w stosunku do nich "umowy".' (1913a, p. 217).

symbols since they establish rules on the basis of which a system of linguistic symbols I use is constructed; thus they are the indispensible key allowing me to decipher [. . .] these objects I use' (1912, p. 216). Therefore, they concern objects the features of which depend on their creator and user. For instance, 'symbolic functions [. . .], which I accept, change depending what functions I assign to these expressions in the conventions I accept' (1912, p. 216). The propositions expressing such conventions are true or false 'since they symbolise the state of affairs that, accepting the convention in question, I create myself' (1912, p. 216).

Leśniewski took a firm stand in the dispute concerning universals, rejecting the existence of any ideal and general objects. In his publication 'Krytyka logicznej zasady wyłączonego środka' [Critique of the Logical Principle of Excluded Middle] (1913b) he gave a proof of non-existence of such objects and the proof became popular in Poland. Leśniewski uses the concept of feature as well as the principle of excluded middle and the principle of contradiction. Kotarbiński quoted this proof, adding some modifications, in his paper 'Sprawa istnienia przedmiotów idealnych' [The Problem of the Existence of Ideal Objects] (1920) and repeated it in his book *Elementy teorji poznania, logiki formalnej i metodologji nauk* [Elements of the Theory of Cognition, Formal Logic and Methodology of Science] (1929). The proof became one of the justifications of reism he propagated (cf. Sect. 3.5). Leśniewski returned to his proof in 'O podstawach matematyki' (1927, pp. 183–184), where he gave a version without the term 'feature.'[69] The proof was preceded by the following explanations:

> At the time I wrote that passage [Leśniewski says about the appropriate fragment of his (1913a)—remark is mine] I believed that there are in existence in this world so called features and so called relations, as two special kinds of objects, and I felt no scruples about using the expressions 'feature' and 'relations'. It is a long time since I believed in the existence of objects which are features, or in the existence of objects which are relations and now nothing induces me to believe in the existence of such objects [. . .] and in situations of a more 'delicate' character I do not use the expressions 'feature' and 'relation' without the application of various extensive precautions and circumlocutions. I also have no inclination at present—considering the possibility of various interpretational misunderstandings—to ascribe this or that opinion on the question of 'general objects' to the authors mentioned in the passage mentioned above (1992b, p. 198).[70]

[69] The polemic concerning Leśniewski's proofs was reported by Woleński (1997, pp. 58–65). One of its participants was Roman Ingarden.

[70] 'W czasie, gdy ustęp ten [chodzi tu o stosowny fragment pracy (1913a)—uwaga moja, R.M.] pisałem, wierzyłem, iż istnieją na świecie tzw. cechy i tzw. stosunki jako dwa specjalne rodzaje przedmiotów, i nie odczuwałem żadnych skrupułów przy posługiwaniu się wyrazami "cecha" i "stosunek". Obecnie nie wierzę już od dawna w istnienie przedmiotów będących cechami, ani też w istnienie przedmiotów będących stosunkami, nic mnie też nie skłania do wierzenia w istnienie takich przedmiotów [. . .], wyrazami zaś "cecha" i "stosunek" staram się w sytuacjach o cokolwiek "delikatniejszym" charakterze nie posługiwać bez daleko idących ostrożności i omówień. Nie mam dziś także skłonności—wobec możliwości rozmaitych nieporozumień interpretacyjnych—do przypisywania tych lub innych poglądów w sprawie "przedmiotów ogólnych" tym lub innym z autorów, wymienionym w ustępie wyżej przytoczonym' (1927, p. 183).

Leśniewski, just like Łukasiewicz (cf. Sect. 3.2), opted for extensionalism, refuting all intentional contexts (e.g. '*X* knows that *p*') and recognising them as defective. In his opinion, they can be eliminated by treating the argument of intentional functor as the name of a proposition and not as the proposition itself.[71] Leśniewski was also an advocate of a two-valued logic (bivalentism). Extensionalism and bivalentism drew his attention away from many-valued or modal logics. He regarded many-valued logic as pure formalism without any intuitional content. It may be worth analysing as a formal system but nothing more. One of Leśniewski's letters directed to Twardowski testifies to the fact that in the 1930s he dealt with many-valued logics but he gave up his investigations. He wrote:

> My work on 'many-valued logics,' about which I wrote you, last year, was temporarily put aside [...]. Using the materials, I collected on the subject of 'many-valued logics' I prepared a two-hour lecture 'On the so-called many-valued systems of propositional calculus'; this year I am going to announce the continuation of the lecture as a separate whole under some other title (quoted after Jadczak 1993).[72]

Of special attention is the title of Leśniewski's lecture, namely the expression 'on the so-called' as it illustrates perfectly the author's attitude towards these logics.

New light on Leśniewski's attitude towards the idea of many-valued logics and the problem of intentionality in logic is thrown by the discussion (recently discovered by Jacek Juliusz Jadacki) concerning his talk entitled 'Geneza logiki trójwartościowej' [The Origin of Three-Valued Logic] from the year 1938 (summary of the talk—cf. Łukasiewicz 1939, discussion—cf. Leśniewski et al. 1939). Łukasiewicz had presented his views on the origin of many-valued logics and reminded the gathered that third logical value could be attributed to propositions of accidental (undetermined) future events. This was followed by a discussion, which Leśniewski himself began. The report says that he 'assumes a negative standpoint towards "Prof. Łukasiewicz's three-valued logic" and towards all other "many-valued logics"' (Leśniewski et al. 1939, p. 235). Then the reasons are listed: (1) so far third logical value has not been given any comprehensible sense that would lead to its interpretation showing some connection with reality; (2) if all scientific problems can be solved by two-valued logic there are no reasons to introduce additional logical values and related logics; (3) Aristotle's reasoning concerning future events, to which Łukasiewicz also referred (cf. Sect. 3.2), can also be transferred to the case of past and present events; (4) reflecting on propositions the logical value of which depends on the parameter of time requires formulating rules that would govern such a parameter; (5) three-valued logic does

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[71] Unfortunately, the details of this solution are unknown.

[72] 'Praca moja o "logikach wielowartościowych", o której pisałem Panu Profesorowi w zeszłym roku, poszła chwilowo w kąt [...]. Z materiałów, które mi się nazbierały na temat "logik wielowartościowych", zrobiłem już całoroczny dwugodzinny wykład "O tak zwanych wielowartościowych systemach rachunku zdań"; dalszy zaś ciąg tego wykładu zamierzam ogłosić na rok bieżący jako oddzielną całość, pod jakimś nowym tytułem.'

not solve problems regarding propositions expressing possibility or necessity, thus propositions containing (certain) intentional functors. The report continues:

> The speaker does not know—facing no system of 'intentional logic,' which would be satisfactory from the intuitional and formal aspect, in the world—any effective method of reasonable interpretation and logical 'mastering' of the above-mentioned 'intentional functions' apart from the method of their 'disintensionalisation,' i.e. assigning them the same-sense expressions, which are based on consequently 'extensionalistic' principles and can be reflected on without any complications on the foundation of normal 'extensionalistic' and 'two-valued' logic. The speaker mentions that he has developed in detail his conception of 'disintensionalisation' of the so-called intentional functions in various lectures for many years, and at the same time the speaker focuses on R[udolf] Carnap's conception, which in its fundamental idea is close to this conception, and which he has published in *Logische Syntax der Sprache* recently, conception, which according to the speaker, is not accurate in some details and leads to theoretical consequences that cannot be maintained (Leśniewski et al. 1939, p. 236).[73]

Leśniewski finished his talk by analysing such terms as 'possible that *p*' from the point of view of 'disintensionalisation.' He reached the conclusion that 'there are no alarming aporias that would incline him to seek some new logic in order to remove them' (Leśniewski et al. 1939, p. 237).

Let us add—to have a more complete picture—that Łukasiewicz, answering Leśniewski's remarks, stressed that he treated the systems of many-valued logic as formal ones, and the fact that one can give some intuitional interpretation to additional logical values is merely a factor, which helped him build those systems. It is worth adding that Leśniewski attached great importance to the aforementioned 'disintensionalisation' of logic.

The discussed issues, in particular the problem of third logical value and the problem of dependence of propositional logical value on the parameter of time, is related to the issue of eternity and sempiternity of truth, which was the theme of the polemic between Leśniewski and Kotarbiński. Now, in the paper 'Zagadnienie istnienia przyszłości' [The Problem of the Existence of the Future] (1913), reflecting on the possibility of free practical activity and creativity, Kotarbiński dealt with certain logical problems. Namely, he claimed that although every truth was eternal not every truth was sempiternal. Consequently, he concluded that there were propositions that were neither true nor false. Thus he propagated

[73] 'Mówca nie zna—wobec nieistnienia na świecie jakiegoś zadowalającego pod względem intuicyjnym i formalnym systemu "logiki intensjonalnej"—żadnej skutecznej metody rozsądnego interpretowania i logicznego "opanowywania" wzmiankowanych "funkcji intensjonalnych" poza metodą ich "dezintensjonalizacji", polegającej na przyporządkowaniu im posiadających ten sam sens wyrażeń, które już są na zasadach konsekwentnie "ekstensjonalistycznych" i dają się bez żadnych komplikacji rozważać na gruncie normalnej "ekstensjonalistycznej" i "dwuwartościowej" logiki. Mówca nadmienia, że jego koncepcja "dezintensjonalizacji" tzw. funkcji intensjonalnych była przez niego od wielu już lat szczegółowo rozwijana w różnych jego wykładach, i zwraca jednocześnie uwagę na zbliżoną do tej koncepcji pod względem zasadniczej idei koncepcję R[udolfa] Carnapa, ogłoszoną przez niego ostatnio w *Logische Syntax der Sprache*, koncepcję, która jest, zdaniem mówcy, w pewnych swych szczegółach nietrafna i prowadzi do nie dających się utrzymać teoretycznych konsekwencji.'

indeterminism and the need to introduce third logical value. In fact, this view became the framework of Łukasiewicz's many-valued logic.[74] In his considerations Kotarbiński used the definition of truth outlined by Twardowski and interpreted in an absolutist way. In his paper 'Czy prawda jest tylko wieczna czy też wieczna i odwieczna?' [Is Truth Eternal, or Both Eternal and Sempiternal?] (1913a) Leśniewski claimed—polemicising with Kotarbiński—that every truth was eternal and sempiternal. In 'Próba dowodu ontologicznej zasady sprzeczności' [An Attempt at a Proof of the Ontological Principle of Contradiction] (1912) he stated that there existed no propositions that would be neither true nor false. Leśniewski showed that Kotarbiński's view was not in accord with the absolutist understanding of truth.[75] He refuted completely the temporal indexing of truth. He questioned the validity of contexts of the type 'proposition *A* is true at time *t*.' According to Leśniewski, propositions are simply either true or false. In addition, here we are dealing again with the issue of bivalence, which has already been discussed. Let us add that in fact the most important result of Leśniewski's considerations on the temporality of truth is the proof that its eternity and sempiternity are equivalent providing the principle of non-contradiction is accepted.

3.5 Tadeusz Kotarbiński

Before discussing in detail the views of Tadeusz Kotarbiński concerning logic and mathematics let us look at his understanding of philosophy as such. From the beginning of his activities he was interested in the problem of the object of investigation and research methods of philosophy. Many a time did he express his dissatisfaction about the functioning philosophical concepts but at the same time, he approvingly treated most problems discussed by philosophers. First of all, he discerned the huge ambiguity of the term 'philosophy.' From our perspective, it is essential to see that he distinguished between the so-called small and great philosophy. For the first time he used these terms during the inaugural lecture as a professor of philosophy. The lecture, entitled 'O wielkiej i małej filozofii' [On Great and Small Philosophy], has never been published but it can be reconstructed because Kotarbiński often referred to the thoughts he had expressed in his talk.[76] He preferred a 'small' philosophy, which was among other things a systematic analysis of concepts used in philosophy and the application of logical tools in the analysis in question. This analysis should become a starting point to build philosophical systems. In his paper 'Filozof' [Philosopher] he wrote:

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[74] Łukasiewicz admitted that Kotarbiński's article had influenced the way he shaped his idea of many-valued logic.

[75] Leśniewski's argumentation convinced Kotarbiński who did not defend logical indeterminism later.

[76] The summary of the lecture was preserved in the foreword to Hosiasson et al. (1934).

> A philosopher as such neither counts nor experiments. He exercises thinking, mastering problems and concepts, theorems and systems of theorems, and he does that mainly through internal efforts aiming at understanding proper intentions of thoughts seeking gropingly; aiming at a more rational shaping of problems, at explaining completely generally unclear concepts, at achieving the obviousness of theorems and solidity of systems. [...] He struggles against obscurity, instability, and the indefiniteness of thought; he arms himself against any insobriety in thinking, which is so frequent because of yielding to some hardened superstition or illusion that tempts the heart, or finally, against partiality caused by the personal or social situation of the thinker himself (1957a, p. 16).[77]

At the same time Kotarbiński was not concerned whether such 'cogitations' could be called a science. This contrasts with Łukasiewicz's attitude as he claimed that philosophy was either science in the sense of an empirical science or merely a speculation. If philosophy wanted to be a science a philosopher must either count or experiment. According to Kotarbiński, a philosopher does neither of these activities—he analyses. Yet, what is important is that he should analyse in accord with all the laws of logic.

Logic and its rules guarantee the correctness of philosophical analyses. Note that Kotarbiński understood philosophy in a very wide way. He thought that it particularly embraced both formalised logical systems and principles of defining, classifying, reasoning, avoiding logical and semantic errors, etc. In Poland Kotarbiński spread the division of logic (in a wider meaning) into semiotics, formal logic and methodology of sciences. In *Kurs logiki dla prawników* [A Course of Logic for Lawyers] (1953, p. 7) he wrote:

> In a wider sense, logic embraces formal logic, i.e. logic in the narrower sense, and semantics, the theory of cognition and methodology of sciences.[78]

This understanding of logic corresponds with didactic practice, which did not only characterise Kotarbiński. Let us notice that for a long time even lectures on logic for mathematicians ended with lectures on argumentations in natural sciences, cf. for example Sect. 11 of Łukasiewicz's textbook *Elementy logiki matematycznej* (1929a) or Sect. 14 of Jaśkowski's textbook *Elementy logiki matematycznej i metodologii nauk ścisłych: skrypt z wykładów* [Elements of Mathematical Logic and Methodology of Exact Sciences: Textbook of Course Lectures] (1947).

In order to understand Kotarbiński's views on mathematics and logic we must begin with his ontological and semantic concepts, i.e. reism. For him reism is both a

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[77] 'Filozof jako taki ani nie rachuje, ani nie eksperymentuje. Uprawia on myślicielstwo, doskonaląc zagadnienia i pojęcia, twierdzenia i systemy twierdzeń i czyniąc to głównie przez wysiłek wewnętrzny, zmierzający ku zrozumieniu właściwej intencji myśli, szukającej po omacku, ku racjonalniejszemu ukształtowaniu problematów, ku doprowadzeniu do jasności zupełnej pojęć—na ogół niewyraźnych, ku uzyskaniu oczywistości twierdzeń i solidności systemów. [...] Toczy on walkę z mętnością, chwiejnością, nieokreślonością myślenia, uzbraja się przeciwko wszelkiej w myśleniu nietrzeźwości, jakże częstej skutkiem ulegania zatwardziałemu przesądowi lub ponętnej dla serca ułudzie, lub stronniczości wreszcie, która wyrasta z sytuacji osobistej lub społecznej samego myśliciela.'

[78] 'Logika w szerszym tego słowa znaczeniu obejmuje logikę formalną, czyli logikę w węższym sensie, oraz semantykę, teorię poznania i metodologię nauk.'

semantic and ontological concept, assuming that in a way both layers occur concurrently. Creating reism Kotarbiński followed—as he himself admitted—Leśniewski's logical ideas formulated within the system of the calculus of names (Leśniewski's ontology). In his foreword to *Elementy teorii poznania, logiki formalnej i metodologii nauk* [Elements of the Theory of Knowledge, Formal Logic and Methodology of Sciences] he wrote:

> Still, I have learnt most things from Prof. Dr Stanisław Leśniewski. I admit that in many places of the book. And they are the most important and clearest points. Besides I admit that all my thoughts are deeply saturated with the influences of that extraordinary mind whose precious gifts I have used, thanks to good luck, almost every day for a number of years. I am undoubtedly a disciple of my colleague Leśniewski whom here I thank cordially and respectfully for all that he has ever taught me[79] (1961, pp. 9–10). [80]

The source of Kotarbiński's reism was his doubts concerning the existence of properties and other ideal objects. For the first time he expressed his doubts when he criticised the views assuming the existence of ideal objects in his paper entitled 'Sprawa istnienia przedmiotów idealnych' [The Problem of the Existence of Ideal Objects] (1920). He wrote that there were no foundations to assume the existence of those objects. He tried to show that there were no imaginary (only conceivable) objects, no mathematical objects; there were no types (universals), features, relations, intentional objects, thinking processes and psychological contents. Relating to that Kotarbiński put forward the thesis called reism or concretism (after the war he used the latter interchangeably with 'reism').

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[79] Let us notice that Leśniewski himself valued his collaboration with Kotarbiński. He admitted that he owed him a lot. In his work 'O podstawach matematyki' he wrote, 'From the remote period of our common 'philosophical' past when each one of us [. . .] was straying along blind alleys in semantics and theories of 'truth' [. . .], I became accustomed to check my various ideas and theoretical projects in scientific discussions with Tadeusz Kotarbiński: I availed myself on various occasions of his subtle analytical help; I constantly referred to his sharp insights during the establishment of various assumptions in the different deductive theories which I was constructing; I listened to his relevant and fair critical observations and felt concerned whenever I deviated too much from his theoretical conceptions of my own views' (1992b, pp. 372–373).

('Od najbardziej zamierzchłych czasów naszej wspólnej "filozoficznej" przeszłości, kiedyśmy razem [. . .] błądzili wśród mylnych perci semantyki i teorii "prawdy" [. . .] przyzwyczaiłem się do kontrolowania rozmaitych swoich pomysłów i zamierzeń teoretycznych w naradach naukowych z Tadeuszem Kotarbińskim: korzystałem przy różnych nadarzających się sposobnościach z jego subtelnej pomocy analitycznej; odwoływałem się do jego wnikliwych intuicji przy ustalaniu pod względem rzeczowym tych lub innych założeń poszczególnych teorii dedukcyjnych, które budowałem; wysłuchiwałem jego rzeczowych i rzetelnych uwag krytycznych i doznawałem stanów niepokojącej niepewności, gdy od reprezentowanych przez niego koncepcji teoretycznych zbytnio się oddaliłem w poglądach swoich na jakieś sprawy.'(1930, p. 161))

[80] 'Najwięcej wszelako nauczyłem się od prof. dra Stanisława Leśniewskiego. W wielu miejscach książki wyraźnie z tego zdaję sprawę. Ale to są punkty najważniejsze i najwyraźniejsze. Poza tym, przyznaję, cała myśl moja przesycona jest do głębi wpływami tego niezwykłego umysłu, z którego bezcennych darów los przychylny pozwolił mi przez szereg lat korzystać w obcowaniu niemal codziennym. Jestem niewątpliwie uczniem kolegi Leśniewskiego, któremu na tym miejscu serdecznie i z głębokim szacunkiem dziękuję za wszystko, czego mnie kiedykolwiek nauczył.'

Reism was explicated in *Elementy* (1929, 1961) and in various papers. At first, the conception was developed on the ontological and semantic levels. Then Kotarbiński distinguished between reism in the ontological sense and reism in the semantic sense. The former can be reduced to two theses: (1) every object is a thing; (2) no object is a state, a relation, a feature[81] Kotarbiński also assumes that things are bodies and thus extensive beings existing in time and space. Therefore, we are dealing with somatism strengthened to become pansomatism—there are only bodies. This distinguishes reism from other concretisms, for example the concretism of Leibniz who actually (towards the end of his life) assumed that there were only concrete entities but his concretism was of spiritualistic nature because those concrete entities were spiritual monads. Let us notice that reism can be seen as a certain interpretation of Leśniewski's ontology (the latter was not a reist although he was a nominalist—cf. Sect. 3.4).

Semantic reism is a theory of language. The starting point is the distinction between genuine and apparent names. An apparent name (onomatoid) is a name (in the grammatical sense) that does not refer to things (persons are treated as special kinds of things) but to ideal objects, i.e. using Wundt's classification—to properties, relationships or states. A sentence has a literal sense only when it is constructed from logical variables, i.e. functors of logic, and real names. Moreover, Kotarbiński distinguishes sentences that have a shortened-substitute sense—they lack the literal sense but they can be transformed into sentences having a literal sense, and senseless sentences, i.e. sentences that cannot be transformed into sentences having a literal sense. Examples of apparent names can be 'redness,' 'justice,' 'fact,' 'entitlement,' etc.

Reism faces various difficulties. We are not going to describe this problem in detail[82] but stress the problems connected with the philosophy of mathematics. Using the language of reism we can speak about sets in a distributive sense that is fundamental for set theory, on which in turn the whole building of mathematics is constructed, but only providing that those statements refer to the elements of these sets. It allows us to develop the elementary algebra of sets but not to define, for instance the concept of finite or infinite set. However, it is not sufficient for mathematics. Leśniewski, to whom Kotarbiński referred, was aware of these difficulties and proposed to use the concept of a set in a collective sense (mereological). Yet, such an approach does not allow realising all that mathematicians expect of set theory. The aforementioned things made reism remain rather a semantic programme and not a theory of the world although Kotarbiński himself never rejected the ontological reism. It should be added that reism had numerous followers, the greatest one being Alfred Tarski (cf. the remarks in Sect. 3.7).[83]

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[81] Here we have a clear reference to the four categories proposed by Wilhelm Wundt.

[82] Remarks on this topic can be found, for example in Woleński's books (1990) and (1997).

[83] It is worth quoting the words of Andrzej Mostowski uttered after returning from a conference dedicated to the foundations of set theory: 'Just imagine that there I sighed for reism. The presented conceptions resulted from so breakneck speculations, so unattainable for intuition and

Furthermore, reism, thanks to its logical tools, allows achieving more than any other nominalism.

A consequence of Kotarbiński's reistic attitude was his concept of logical sentence, which is the starting point to reflect on the concept of truth. In *Elementy* (1929, 1961) he makes a distinction between idealistic ('in the spirit of Platonic idealism'—cf. 1961, p. 130; 1966, p. 104), psychological and nominalistic concepts of sentence, but he assumed only the latter, writing that this sentence is 'the symbol itself, the inscription, the statement, the linguistic phrase or formulation' (1961, p. 131; 1966, p. 105).

What were the fundamental features of the concept of truth formulated by Kotarbiński? Following reism, he claims that the truth is a characteristic of a sentence, that the 'true' predicate can only refer to sentences. Since—according to reism—there are no propositions in the logical (ideal) sense and propositions in the psychological sense this predicate cannot refer to thoughts but only to sentences understood as inscriptions. As for thought, the predicate can refer merely in the figurative sense. In *Elementy* he wrote:

> From our standpoint, it must be stated that there are no judgements in the logical sense; hence, it is not true that judgements in the logical sense are true or false. There remain judgements in the psychological sense, and sentences. But judgements in the psychological sense, if they are to be interpreted as events, also do not exist. Hence it is also not true that judgements in the psychological sense are true or false (1966, p. 105).[84]

Kotarbiński was an adherent of the absolute character of truth and enemy of the relativistic approach. The truth or falsity of a sentence does not depend on whom and in what circumstances formulated it:

> The reader must have had a clear impression that the position occupied by the relativists is weaker. Consequently, although relativism is attracting human minds even now (cf. the works of the pragmatists) as it attracted them in the epoch of the Greek sophists (when one of the masters of controversy, namely Protagoras, claimed that man is the measure of all things, by which he probably meant that while something may be true for one person, the contrary may be true for another), it does not find favour in the eyes of good experts in logic (1966, p. 113).[85]

so incomprehensible that reism seemed to be an oasis where one can breathe fresh air' (Kotarbińska 1984, p. 73).

[84] 'Z naszego stanowiska wypada stwierdzić, że nie ma sądów w znaczeniu logicznym, nieprawda przeto, jakoby sądy w znaczeniu logicznym były prawdziwe lub fałszywe. Pozostaje sprawa sądów w znaczeniu psychologicznym i sprawa zdań. Ale wszak i sądów w znaczeniu psychologicznym naprawdę nie ma, skoro miałyby to być zdarzenia. Więc nieprawda również, jakoby sądy w znaczeniu psychologicznym były prawdziwe lub fałszywe' (1961, p. 131).

[85] 'Czytelnik musiał doznać żywego wrażenia, że pozycja relatywizmu jest słabsza. Toteż, jakkolwiek relatywizm pociąga ku sobie umysły i dziś (cf. pisma pragmatystów), jak pociągał je w epoce sofistów greckich (kiedy to jeden z tych mistrzów sporu, Protagoras, głosił, że człowiek jest miarą rzeczy, rozumiejąc bodaj przez to, iż dla jednego jedno, dla drugiego coś przeciwnego bywa prawdziwe), jednakże pośród dobrych specjalistów w dziedzinie logiki relatywizm nie cieszy się mirem' (1961, p. 140).

Kotarbiński distinguished between a real and verbal understanding of truth.[86] It seems to be his original contribution to the theory of truth. According to this distinction, in certain contexts the predicates 'true' or 'false' are not needed since they play only the role of stylistic ornaments and do not bring anything new to the content of the sentence. They can be reformulated without using the words 'true' or 'false.' Therefore, 'The sentence that Warsaw is Poland's capital is true' can be replaced by the sentence 'Warsaw is Poland's capital,' which does not contain the predicate 'true.' However, Kotarbiński notices that such a replacement is not always possible. For example, the sentence 'The theory of relativity is true' or the sentence 'What Plato said is true' cannot be reformulated in this way. Eliminating the word 'true' we receive a different kind of statements—they stop being sentences and become names. Thus in various contexts the predicates 'true' and 'false' are necessary. In such cases they occur in the real sense (not merely verbal). Following the spirit of reism Kotarbiński claims clearly:

> In general, there are no "truths" and "falsehoods" if such are understood as some "ideal objects" or "objects from the sphere of content." There are only persons who think truly and persons who think falsely, as well as true sentences and false sentences. Thus the words "truth" and "falsehood" will be proper names and not empty at that, if by "truth" we mean "true sentence", and by "falsehood," "false sentence" (1966, p. 109).[87]

Consequently, the truth or falsity bearers can only be sentences understood as inscriptions.

In *Elementy* Kotarbiński also distinguished between classical and utilitarian understanding of truth and falsity. According to the former: 'truly means the same as in accordance with reality' whereas according to the latter: 'truly means the same as usefully in some respect' (1966, p. 106).[88] One of the forms of utilitarian understanding is pragmatism that holds 'that truth is nothing else than that property of a judgement that leads to effective actions' (1966, p. 106).[89]

Having distinguished those two senses Kotarbiński clearly opted for the classical understanding. He realised that 'accordance with reality' is an imprecise term and of a rather metaphorical character when understood merely as analogy or image:

[86] In (1926) Kotarbiński used the terms: real and nihilistic understanding of truth. Cf. also his paper 'W sprawie pojęcia prawdy' [On the Term of Truth] (1934), which was his review of Tarski's book *Pojęcie prawdy w językach nauk dedukcyjnych* [The Concept of Truth in Formalized Langauges] (1933).

[87] 'W ogóle nie ma "prawd" ani "fałszów", jeśliby to miały być jakieś tak zwane "przedmioty idealne", jakieś tak zwane "przedmioty ze świata treści". Są tylko osoby myślące prawdziwie i osoby myślące fałszywie oraz prawdziwe zdania i fałszywe zdania. Słowa "prawda" i "fałsz" będą więc nazwami właściwymi, przy tym nie pustymi, jeżeli przez "prawdę" rozumieć będziemy "zdanie prawdziwe", a przez "fałsz"—"zdanie fałszywe"'(1961, p. 136).

[88] 'prawdziwe—to tyle, co: zgodne z rzeczywistością'; 'prawdziwe—to pod pewnym względem pożyteczne' (1961, p. 132).

[89] 'prawdziwość nie jest niczym innym jak tylko własnością danego sądu, iż prowadzi on do działań skutecznych' (1961, p. 132).

> Let us therefore pass to the classical doctrine and ask what is understood by "accordance with reality." The point is not that a true thought should be a good copy or simile of the thing of which we are thinking, as a painted copy or photography is. A brief reflection suffices to recognize the metaphorical nature of such a comparison. A different interpretation of "accordance with reality" is required. We shall confine ourselves to the following: "John thinks truly if and only if John thinks that things are so and so, and things are so and so" (1966, pp. 106–107).[90]

In *Elementy* (and earlier: 1926) Kotarbiński also considered the problem of the criteria of truth. He claimed that 'the search for the criterion of truth seems a hopeless project, at least if we mean a universal criterion, that is such by which we should be in a position to recognise the truth of any true statement' (1966, p. 113).[91] However, one can seek partial criteria that are applied only to certain domains. Kotarbiński distinguishes between intuitional criteria (referring to the sense of obviousness), situational criteria (referring to the analysis of observations considering a given situation) and structural criteria (based on the logical analysis of the structure of utterance) as well as genetic criteria (based on the analysis of the origin of a given statement), clearly stressing their partiality and giving cases where they cannot be used and do not allow us to distinguish true and false sentences.

Concluding our reflections on Kotarbiński's theory of truth let us add that his conceptions strongly influenced the views and the description of the theory of truth formulated by Alfred Tarski—cf. Sect. 3.7.

Kotarbiński dedicated much attention to methodological issues. Of special interest to our discussion are his views concerning the deductive method. In his opinion, it is typical of the *a priori* sciences whereas the inductive method is typical of empirical sciences. The first method reached its apogee in the fundamental branches of mathematics (formal logic, set theory) and the other in experimental physics.

Elementy also contains considerations on the important characteristics of the deductive method. Kotarbiński writes that what we need first of all is clear symbols. Choosing symbols 'it is well to take into account clarity and manoeuvrability of symbols, the naturality of choice of primitive terms, and the reduction of the number of the primitive terms to a minimum' (1966, p. 243).[92] At the same time,

[90] 'Przejdźmyż tedy do doktryny klasycznej i zapytajmy, co tu się rozumie przez ową "zgodność z rzeczywistością"? Nie idzie wszak o to, że myśl prawdziwa ma być dobrą kopią, czy wierną podobizną rzeczy, o której myślimy, na wzór kopii malarskiej lub fotografii. Chwila zastanowienia wystarczy, by utwierdzić metaforyczny charakter takiego porównania. Tu potrzebna staje się jakaś inna interpretacja owej "zgodności z rzeczywistością". Poprzestaniemy na interpretacji następującej: "Jan myśli prawdziwie zawsze i tylko, jeżeli Jan myśli, że tak a tak rzeczy się mają, i jeżeli przy tym rzeczy się mają tak właśnie" ' (1961, p. 133).

[91] 'poszukiwanie kryterium prawdy wydaje się istotnie przedsięwzięciem chimerycznym, przynajmniej jeśli idzie o kryterium powszechne, czyli takie, po którym by można było poznać prawdziwość jakiegokolwiek zdania prawdziwego' (1961, p. 141).

[92] 'dobrze jest liczyć się [. . .] z przejrzystością i łatwością manipulacyjną symboli, z naturalnością wyboru terminów pierwotnych, wreszcie z tym, by terminów pierwotnych było jak najmniej' (1961, p. 289).

he realises that these postulates can sometimes cause conflicts. In the deductive method definitions that 'provide information that a given symbol may be used to replace another given symbol in the process of inference' (1966, p. 245)[93] play an important, though only auxiliary role.

The most important element of the deductive system is axioms, which should be understood as 'basic sentences of a deductive system which are not definitions' (1966, pp. 245–246),[94] assuming that principal propositions are 'sentences accepted without proof as elements of the system' (1966, p. 246). Thus the requirement of obviousness, which was traditionally connected with the concept of axiom, is not needed here. Kotarbiński writes that 'methodologists [...] do not consider it necessary that axioms should be self-evident, not to mention that simplicity is also not considered an indispensable property of axioms' (1966, p. 246).[95] Its source is 'self-evidence possessing lower credit as a criterion of truth, in view of the cases in which it fails' (1966, p. 247).[96] Another proof is that when building deductive systems we want to get to know the logical relations between theses and not become convinced of the truth of derivative theses. From this point of view, it does not actually matter which theses are chosen as the starting point. Thus deductive systems are often called hypothetical-deductive—the conventionalists particularly prefer this approach. On the other hand, the concept of obviousness is unclear and should always be relativised to a concrete person. Thus one should—as Kotarbiński writes—distinguish between obviousness from the point of view of beginners and obviousness from the point of view of specialists of the topic. However, the system of axioms should meet two conditions: axioms should not be mutually contradictory and should create a complete system.

Deducing theses from accepted axioms one should not rely only on intuition since it is often deceptive. Thus one should refer only to the shape of inscriptions and use only formal methods.

In *Elementy* we can also find philosophical remarks on mathematics as a science. According to Kotarbiński, mathematics 'owes its role to its subject matter, to its methods, and to the numerous achievements which contribute to its imposing success' (1966, p. 315)[97] since:

> [...] mathematics uses model methods of proofs, develops habits very useful in reasoning, provides ample knowledge indispensable for a profound understanding of the theory of physics, which is the fundamental discipline in natural science, make such realism the

[93] 'informują o tym, że taki a taki znak może być użyty dla zastąpienia takiego a takiego znaku przy wnioskowaniu' (1961, p. 291).

[94] 'zdania naczelne systemu dedukcyjnego, nie będące definicjami' (1961, p. 292).

[95] 'metodologowie [...] nie uważają [...] za rzecz istotną, by aksjomaty były oczywiste, nie mówiąc już o tym, że i prostota specjalna nie uchodzi za nieodzowną cnotę aksjomatu' (1961, p. 293).

[96] 'obniżenie się kredytu oczywistości, jako kryterium prawdy, wobec zawodów, jakie ono sprawia' (1961, p. 294).

[97] 'zawdzięcza swą rolę zarówno temu, czym się zajmuje, jak sposobom, których się chwyta, jak wreszcie licznym rezultatom, które się składają na imponujący jej dorobek' (1961, p. 370).

> distance between the level of perfection of other disciplines as compared with the perfection of mathematics, and in many cases enables us to appreciate what progress is achieved in a given discipline by the adoption of a quantitative approach, the axiomatic method of building theories, and the application of laws discovered by, and belonging to, mathematics (1966, p. 315).[98]

Characterising mathematics as a science and showing its constitutive features one can either try to seek an answer to the question concerning its object (Kotarbiński speaks about 'ontological' orientation) or concentrate on its methods ('methodological' orientation—cf. 1961, p. 371; 1966, p. 316). Here we are dealing with various stands and opinions. Kotarbiński clearly opts for the nominalistic standpoint:

> In this variety of opinions, let us single out, and declare for, the position of nominalism (1966, p. 317).[99]

Therefore, according to the nominalistic doctrine:

> [. . .] no object is a number, and [. . .] neither arithmetic, not the theory of numbers, nor—*a fortiori*—mathematics in general build statements which might strictly be called statements about numbers in the same sense in which zoology makes statements about animals (1966, p. 317).[100]

Mathematics speaks about all things—and hence, its universality.

Nominalism is consistent with, according to Kotarbiński, the view that mathematics is an *a priori* science. However, at the same time he differentiates between apriority in the genetic sense and in the methodological sense. We deal with the first sense when someone does not recognise a given proposition on the basis of experience whereas we deal with the other sense when a given proposition is obvious because it is self-explanatory or can be justified on the basis of obvious propositions alone. However, such an approach faces difficulties. On the one hand, assuming that mathematics is an *a priori* science implies that, for example the theses of analytic mechanics, which are traditionally included into mathematics, do not go in it. On the other hand, 'it is very doubtful whether theses that are specific to

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[98] '[. . .] stosuje wzorowe sposoby dowodzenia, wykształca nieocenione przyzwyczajenia pożyteczne przy rozumowaniu, dostarcza obfitego zasobu wiedzy niezbędnej do głębszego zrozumienia teorii fizyki, podstawowej nauki przyrodniczej, wreszcie daje świadomość dystansu między stopniem udoskonalenia innych dociekań w porównaniu ze stopniem udoskonalenia matematyki oraz w wielu przypadkach pozwala ocenić, jakim jest postępem dla danej dyscypliny naukowej wprowadzenie do niej ilościowego traktowania rzeczy, aksjomatycznego sposobu budowania teorii oraz zastosowania w niej praw przez matematykę wykrytych i do niej należących' (1961, p. 370).

[99] 'W tym nadmiarze rozmaitych stanowisk niechaj nam wolno będzie wyróżnić stanowisko nominalizmu i przy nim się opowiedzieć' (1961, p. 373).

[100] '[. . .] żaden przedmiot nie jest liczbą i [. . .] ani arytmetyka, ani tzw. "teoria liczb", ani tym bardziej matematyka w ogóle nie budują zdań, które by można nazwać ściśle zdaniami o liczbach w tym sensie, w jakim np. zoologia mówi o zwierzętach' (1961, p. 373).

geometry are methodologically *a priori*' (1966, p. 319).[101] Admitting that the problem of existence and justification of *a priori* knowledge is still open he declares clearly:

> [...] we declare ourselves rather in favour of the existence of *a priori* knowledge—of course, not in the sense that there exists an object which is *a priori* knowledge, but in the sense that we know this or that *a priori*, that is, not from experience (1966, p. 319).[102]

Characterising mathematics by stating that its feature is the use of the deductive method is also debatable since it forces the inclusion of theorems in mathematics 'which we are not willing to include in it' and thus deductibility is connected with 'the formal (in the specified sense) nature of the theses which are deduced and the theses from which other theses are deduced' (1966, p. 321).[103] Such purely formal sentences would 'contain only variable symbols and connectives between them' (1966, p. 321).[104] Such an approach characterises the logicism of Frege as well as of Russell and Whitehead. According to this logicism, 'all mathematics is practically well developed formal logic' (1966, p. 321).[105]

Kotarbiński also notices that there are conceptions in which mathematics is no science and one can only speak of the mathematical method.

Refuting firmly the conception that mathematics investigates a certain world of ideal objects independent on time, space and cognitive mind Kotarbiński does not follow any concrete conception, stating that mathematics can be characterised in at least three ways:

1. As the body of systems in which theorems are justified only in a deductive way and 'the theorems are formulated correctly as statements containing only the following types of signs—variables, connectives, what are called 'names of numbers', 'names of sets', 'names of figures', or terms defined by such signs, names of relations (such as 'greater than', 'equal to', etc,) and finally punctuation marks and signs informing about the role of the remaining signs' (1966, p. 322)[106]—mathematics thus understood embraces the whole formal logic

[101] 'jest rzeczą wysoce wątpliwą, czy tezy swoiste geometrii są aprioryczne w rozważanym tu sensie metodologicznym' (1961, p. 375).

[102] '[...] opowiadamy się tutaj raczej za istnieniem wiedzy apriorycznej, oczywiście nie w tym sensie, iżby jakiś przedmiot był wiedzą aprioryczną, lecz w tym, iż to i owo wiemy apriorycznie, czyli nie na zasadzie doświadczenia' (1961, p. 375).

[103] 'których by się widzieć w jej obrębie nie miało ochoty'; 'formalnym w określonym sensie charakterem tez wysnuwanych oraz tez, z których się je wysnuwa' (1961, p. 377).

[104] 'zawierające oprócz znaków interpunkcyjnych jedynie symbole zmienne oraz spójniki między nimi' (1961, p. 377).

[105] 'cała matematyka jest właściwie rozwiniętą logiką formalną' (1961, p. 378).

[106] 'których twierdzenia wypowiada się poprawnie w zdaniach, zawierających tylko następujące rodzaje znaków: symbole zmienne, spójniki, tzw. "nazwy liczb", tzw. "nazwy zbiorów", tzw. "nazwy figur", lub terminy przez takie znaki zdefiniowane, dalej terminy stosunkowe, jak "większy", "równy" itp., wreszcie znaki przestankowe oraz znaki informujące o roli pozostałych znaków' (1961, p. 379).

(in its propositions these ‘names’ do not occur) and the so-called proper mathematics;
2. As proper mathematics or mathematics in a narrower sense, which is characterised by the fact that those ‘names’ occur in its thesis;
3. As a science that is characterised like proper mathematics but adding the condition that its propositions have the feature of apriority, i.e. its axioms are assigned the feature of obviousness, and justifying its theorems we do not refer to empirical data.

It is worth adding that Kotarbiński realises that mathematicians use other methods besides deduction in research practice but:

> [r]easoning by analogy and inductive reasoning, and in general reductive reasoning, may play at the most an heuristic role in mathematics, understood in any of these three senses, and when the study of a given problem matures, they give place to a proper proof, that is, to a deductive foundation (1966, p. 323).[107]

Therefore, distinguishing the context of discovery and the context of justification Kotarbiński characterises the former by allowing the use of inductive argumentation besides deduction and reserving for the latter only deductive reasonings that are typical of a mature stage of development of mathematical theories.

3.6 Kazimierz Ajdukiewicz

Let us begin our reflection on Ajdukiewicz’s philosophical views on mathematics and logic with his *Habilationsschrift* entitled *Z metodologii nauk dedukcyjnych* [From the Methodology of the Deductive Sciences] (1921). It consisted of three parts: ‘Pojęcie dowodu w znaczeniu logicznym’ [The Logical Concept of Proof], ‘O dowodach niesprzeczności aksjomatów’ [On Proofs of Consistency of Axioms] and ‘O pojęciu istnienia w naukach dedukcyjnych’ [On the Notion of Existence in Deductive Sciences]. As Ajdukiewicz wrote (meaning particularly the first part) in *Przedmowa* [Foreword] to the first volume of his collected works it was ‘the first Polish work on the methodology of the deductive sciences, remaining under the influence of mathematical logic’[108] (1960a, p. V). One can see here the influences of Hilbert and his formalistic conceptions, especially that Ajdukiewicz listened to Hilbert’s lectures in the year 1913 during his stay in Göttingen. The influences in

[107] ‘[r]ozumowania przez analogię oraz rozumowania indukcyjne i w ogóle redukcyjne w matematyce rozumianej tak czy tak, mogą mieć znaczenie co najwyżej heurystyczne i w stadium dojrzałości opracowania danego problematu ustępują miejsca dowodowi właściwemu, a więc uzasadnieniu właściwemu’ (1961, p. 379).

[108] He also added that his work had began—at least in Poland—‘the structural method of defining methodological concepts (e.g. the concept of proof or the concept of consequence), which later played an important role in the magnificent development of science on the deductive systems called metamathematics’ (1960a, p. V).

question could be seen in his considerations of all the aforementioned problems as referring to formalised systems, understood as well-defined collections of formulas. Although there are no original ideas in his *Habilitationsschrift*, it should be admitted that his work contributed to the systematisation and specification of numerous issues connected with philosophy and the methodology of mathematics, or generally, deductive sciences. What is pioneering is his definition of logical consequence proposed in the first part of the work and then repeated several times in various textbooks, e.g. *Logiczne podstawy nauczania* [The Logical Foundations of Teaching] (1934b). Alfred Tarski used this definition as the basis of his theorem concerning deduction in 'O pojęciu wynikania logicznego' [On the Concept of Logical Consequence] (1936)—in a special note included in the selection of his most important logical works (1956, p. 32) Tarski stated that he had made his discovery as early as in 1921 by reflecting on Ajdukiewicz's work (cf. Batóg 1984). Another original idea is the proposal to relativise the concept of existence to a given formalised system (which in turn suggested relativising other metalogical and mathematical concepts to a defined formal system).

Characterising deductive sciences Ajdukiewicz finds their most developed forms in formalised theories. In the spirit of Hilbert's formalism he abstracts from the meaning ascribed to primitive concepts:

> Symbols of deductive theories are, then, symbols not by 'meaning' or 'denoting' anything but by playing a definite 'role,' by occurring in strictly defined relations (1966, p. 14).[109]

Speaking about axioms he states:

> What then are axioms if they are not sentences in the intuitive sense of the word? They are but strings of signs so pronounced that *they sound like sentences*.
>
> Since axioms are not sentences in their ordinary sense and in their ordinary meaning words 'true' and 'false' refer to sentences exclusively so that the property of truth is attributable to sentences alone, it is quite clear that axioms cannot be judged from this point of view. Naturally, as axioms are pronounced so that they sound like sentences, one may rightly ask about the truth or falsity of the corresponding sentence in its intuitive sense; however, axioms as such are neither true nor false, unless in some metaphorical sense[110] (1966, p. 14).[111]

[109] 'Są tedy symbole nauk dedukcyjnych symbolami nie dlatego, jakoby "coś znaczyły" albo "coś oznaczały", lecz dlatego, że mają określoną "rolę", dlatego, ponieważ występują w ściśle określonych związkach '(1921, pp. 11–12).

[110] It should be noted that Ajdukiewicz had deliberated on his concept for over 10 years before Tarski formulated his definition of the concept of satisfaction and truth.

[111] 'Czymże są zatem aksjomaty, jeśli nie są w znaczeniu intuicyjnym zdaniami? Otóż są one tylko pewną kombinacją znaków, które wymawia się tak, że *brzmią one jak zdania*.

Skoro aksjomaty nie są zdaniami w znaczeniu potocznym, a potoczne znaczenie wyrazu "prawdziwy" lub "fałszywy" odnosi się tylko do zdań, tak że tylko zdaniom ta własność może być przypisana, zatem jasną staje się rzeczą, że aksjomatów z tego punktu widzenia oceniać nie można. Oczywiście, że skoro aksjomaty wymawia się tak, że brzmią one jak zdania, można słusznie pytać o prawdziwość lub fałszywość tego zdania w znaczeniu potocznym, samym jednak aksjomatom nie można przypisać prawdziwości ani mylności, chyba tylko w znaczeniu przenośnym' (1921, p. 12).

Ajdukiewicz understands 'a logical thesis' as 'any arrangement of symbols for which there exists a proof in the logical system' (1966, p. 15).[112] He distinguishes between pure theories (isolated) and applied theories. Pure theories are those with the primitive symbols, and consequently also axioms, that were not interpreted whereas applied theories are those in which the primitive symbols were given 'the same intuitive meaning which is associated with the expressions used to pronounce axioms' (1966, p. 19).[113] Moreover, the philosopher writes:

> Absolutely abstract theories do not possess in themselves a value greater than the game of chess, at least so far as practical value is concerned. To be able, however, to justify this value-judgment we would have to make explicit our view on the value of science in general. In any case, they do not render anything that could be evaluated from the point of truth and falsity, since they contain no sentences. Applied sciences, in the sense that logical symbols occurring in them are endowed with meaning, contain sentential functions which, as we know, are neither true nor false but turn into such or such depending on the meaning that will be associated with the still meaningless symbols.
>
> If, nevertheless, truth or falsity is sometimes predicated of absolutely pure deductive theories, then certain conventional meanings of these terms are involved [. . .] (1966, p. 20).[114]

The most interesting thing from the perspective of this book is the problem of existence, which Ajdukiewicz pondered on in the third part of his book *Z metodologii nauk dedukcyjnych* (1921). He did not deal with the problem which was in focus of philosophers of those times, namely what kind of existence should be ascribed to the objects of deductive sciences, but he asked about the meaning of the word 'exist' in these sciences:

> An analysis of meaning of the word 'exist' as used in deductive theories does not amount to the problem: what kind of existence is among the attributes of existing objects of deductive theories; our own position permits us to doubt whether any kind of being at all is among the attributes of these objects. Our problem then is not the question what kind of being is attributable to objects under discussion, but the question what is the meaning of the word 'exist' as used in deductive theories. It may be that it is being used quite erroneously and has nothing at all to do with existence (1966, p. 34).[115]

[112] 'każdą kombinację symboli, posiadającą w systemie logiki zawarty dowód' (1921, p. 14).

[113] 'ten sam sens intuicyjny, który łączymy z wyrazami, w jakich symbole te wymawiamy' (1921, p. 20).

[114] 'Teorie oderwane w znaczeniu bezwzględnym nie mają same dla siebie większej wartości niż gra w szachy—przynajmniej wartości praktycznej. By móc jednak uzasadnić tę ocenę, należałoby zająć stanowisko w sprawie wartości nauki w ogóle. W każdym razie nie dają one niczego, co by można ocenić z punktu widzenia prawdy i fałszu, bo nie zawierają zdań. Nauki stosowane w tym znaczeniu, że występujące w nich symbole logiczne posiadają sens, zawierają funkcje propozycjonalne (zdaniowe), które—jak wiadomo—nie są ani prawdziwe, ani mylne, lecz stają się takimi lub takimi zależnie od przypisania takich lub innych znaczeń występującym w nich jeszcze bezsensownym symbolom.

Jeśli się mimo to mówi o prawdziwości respective mylności absolutnie czystych teorii dedukcyjnych, to czyni się to w pewnym znaczeniu konwencjonalnym [. . .]' (1921, p. 21).

[115] 'Analiza znaczenia wyrazu "istnieć" w naukach dedukcyjnych nie jest zatem równoznaczna z zagadnieniem: jaki rodzaj istnienia przysługuje istniejącym przedmiotom nauk dedukcyjnych;

Ajdukiewicz argues that existence in deductive sciences cannot be identified with consistency and that consistency is neither an indispensible nor sufficient condition of existence. He claims that the indispensible conditions of existence are: (I) being included within the domain of the given theory and (II) consistency:

> My contention is, namely, that for an object *p* defined by Ω (*p*) to exist it is necessary that *p* be an element of the domain of the given theory, in other words that Ω (*p*) entailed *A*(*p*) [. . .].
>
> [. . .]
>
> In order to exist an object must, therefore, satisfy another requirement—besides the above condition of being an element of the domain of the theory—scil. its definition must not have any consequences inconsistent with the consequences of *A(p)*. [. . .]
>
> Objects which do not satisfy either the first or the second requirement, do not exist, are nonexisting. From existing and nonexisting objects we ought to distinguish objects which are possible in the given theory (1966, pp. 42–43).[116]

Ajdukiewicz concludes that if an object is to exist it must satisfy the requirements (I) and (II) as well as 'not restrict the domain of possible objects' (1966, p. 44)[117] in the given theory. Finally, he writes:

> In the deductive sciences we do not speak of existence in absolute sense but only relatively to a given system. For there exist Euclidean straight lines and non-Euclidean straight lines; however, both cannot co-exist and their co-existence would be a consequence of their existence if this word were taken in either case in the absolute sense. We may only speak of existence in a system as we speak of inclusion in a domain. Nevertheless it is possible to construct a 'universe' consisting of the domains of several compatible theories, thus forming a system whose axioms would be all axioms of all compatible theories. We could then speak of absolute existence, not quite absolute, though, since it would be possible by choosing various theories, to construct many such 'universes,' self-compatible but mutually exclusive (1966, p. 45).[118]

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problemat nasz pozwala nam w ogóle wątpić o tym, czy jakikolwiek rodzaj bytu przedmiotom tym przysługuje. Kwestią naszą zatem nie jest pytanie, co za rodzaj bytu mają przedmioty przez nas rozważane, ale co znaczy wyraz "istnieć" w naukach dedukcyjnych. Być może, że jest on całkiem mylnie używany i nie ma z istnieniem nic wspólnego' (1921, p. 46).

[116] 'Twierdzę mianowicie, że koniecznym warunkiem na to, by przedmiot określony przez Ω (*p*) istniał, jest iżby przedmiot *p* należał do zakresu danej teorii, czyli iżby z Ω (p) wynikało *A*(*p*) [. . .].

[. . .]

Musi tedy przedmiot na to, aby istniał, spełniać prócz pierwszego (wyżej wymienionego warunku zawierania się) warunek drugi, musi mianowicie jego określenie nie posiadać następstw sprzecznych z następstwami *A*(*p*). [. . .]

Przedmioty, które nie czynią zadość pierwszemu albo drugiemu warunkowi, nie istnieją i są nieistniejące. Prócz przedmiotów istniejących i nieistniejących należy jeszcze rozróżnić, naszym zdaniem, przedmioty możliwe w danej teorii' (1921, pp. 59–60).

[117] 'nie ograniczał [on] zakresu przedmiotów możliwych' (1921, p. 62).

[118] 'O istnieniu bezwzględnym w naukach dedukcyjnych nie mówimy wcale. Zawsze tylko o istnieniu w pewnym systemie. Wszakże istnieją i proste euklidesowe, i nieeuklidesowe, obie nie mogą jednak współistnieć, a współistnienie ich byłoby konsekwencją ich istnienia, gdyby ten wyraz wziąć w odniesieniu do obu w tym samym sensie bezwzględnym. Można więc mówić tylko o istnieniu w pewnym systemie, podobnie jak o zawieraniu się tylko w pewnym zakresie. Niemniej jednak można utworzyć "uniwersum" z zakresów kilku zgodnych z sobą teorii, tworząc system, którego aksjomaty byłyby wszystkimi aksjomatami wszystkich teorii zgodnych. Można

Let us proceed to Ajdukiewicz's views on the domain, status and methods of logic. We begin by quoting a large fragment from his book *Główne kierunki filozofii w wyjątkach z dzieł ich klasycznych przedstawicieli* [Main Trends of Philosophy in Excerpts from Texts of Their Classical Exponents]:

> Regardless of our views concerning the origin of a true thought, i.e. regardless of whether we are empiricists, rationalists or criticists, we assume that if one of two thoughts stays in certain relationships with the other it can be stated, with certainty or some degree of probability, that the first thought is true [. . .]
>
> The science which solves the problem when thoughts remain just in such relationships is called formal logic. It is called 'formal' because the relationships between thoughts depend on their structure, their form, and not on the concrete contents of the thoughts.
>
> [. . .]
>
> Some think that the task of logic is not only to state the relationships between thoughts, constituting the formal conditions of truth, but also to give rules, i.e. norms defining how you should act in thinking so that you can deduce other true thoughts from true thoughts. This idea of the task of logic is quite common and perhaps, historically speaking, most faithful, i.e. it characterises best the problems that were included into logic in various periods. [. . .] From the point of theoretical logic, aiming only at analysing the relationships between thoughts, existing between them only because of their truth, logic—giving norms, i.e. rules of thinking—presents itself as practical logic. In a wider sense, logic contains a section discussing the conduct of thinking you should follow if you want to think in a formally correct way. It shows how you should deal with concepts; it says that they should be defined, and it describes how to do that. It says how you can reach sure conclusions through reasoning, how to seek proofs, how to find the laws of nature, etc. In short, it gives research methods, and the preparation of this part of logic is theoretical logic. Therefore, logic, understood in the wider sense, falls into two sections: the first one, identical with theoretical logic, forms the basis for rules, which practical logic formulates [. . .], the other gives ways and methods of scientific investigations, and is thus called methodology (1923, pp. 22–24).[119]

by wtedy mówić o istnieniu bezwzględnym, jakkolwiek niezupełnie bezwzględnym, bo można by, dobierając rozmaite teorie, potworzyć wiele takich "uniwersów" w sobie zgodnych, lecz między sobą wykluczających się' (1921, p. 63).

[119] 'Bez względu na to, jak zapatrujemy się na genezę myśli prawdziwej, tzn. niezależnie od tego, czy jesteśmy empirystami, racjonalistami czy krytycystami, przyjmujemy, że jeżeli jedna z dwóch myśli pozostaje do drugiej prawdziwej myśli w pewnych stosunkach, wówczas na pewno lub z pewnym stopniem prawdopodobieństwa można twierdzić, że ta pierwsza jest prawdziwa [. . .].

Nauka roztrząsająca zagadnienie, kiedy myśli pozostają w takich właśnie stosunkach, nazywa się logiką formalną. Nazywa się ona dlatego formalną, albowiem o zachodzeniu wyżej wspomnianych stosunków między myślami decyduje nie konkretna treść myśli, lecz ich struktura, ich forma.

[. . .]

Niektórzy uważają, że zadaniem logiki jest nie tylko stwierdzenie stosunków między myślami, stanowiących formalne warunki prawdy, lecz także podanie prawideł, czyli norm określających, jak należy w myśleniu postępować, by z myśli prawdziwych wywieść inne myśli prawdziwe. Taki pogląd na zadanie logiki jest dość rozpowszechniony i bodaj historycznie najwierniejszy, tzn. najlepiej charakteryzuje te zagadnienia, które w różnych czasach zaliczano do logiki. [. . .] Z punktu widzenia logiki teoretycznej, mającej jako jedyne zadanie badanie stosunków pomiędzy myślami, zachodzących między nimi ze względu na ich prawdziwość, przedstawia się logika podająca normy, czyli prawidła myślenia jako logika praktyczna. Logika w obszerniejszym znaczeniu zawiera dział traktujący o postępowaniu, jakiego należy się w myśleniu trzymać, jeśli

Ajdukiewicz speaks of thoughts and thinking but this fact need not lead to psychologism. Let us note that he does not only include relationships referring to truth in logic but also those pertaining to probability. He also makes a clear distinction between theoretical logic and practical logic, for which the former is the basis. In Ajdukiewicz's opinion a large domain of issues (along with the methodology of sciences) should be included in logic. This domain corresponds with what Aristotle discussed in his books called *Organon*.

The fact that practical logic is based on theoretical logic has its source in the generality of the latter. In his textbook *Główne zasady metodologii nauk i logiki formalnej* [Main Principles of the Methodology of Sciences and Formal Logic] Ajdukiewicz writes about this issue:

> A whole series of theorems of formal logic is characterised by the fact that they include only such stable words which occur in *every* science. They do not contain words that can be encountered only in zoology or words that would be only proper to chemistry. Thanks to this circumstance formal logic has wide application. As we can see everyone uses it in most of their reasonings (1928, p. 152).[120]

In this context it is worth quoting the definition of logic which Ajdukiewicz gave in his popular work *Logiczne podstawy nauczania* [The Logical Foundations of Teaching]:

> In all sciences, however, besides terms which are proper to them there are also terms that are common to all sciences. Such terms are, for example the expressions: 'is,' 'not,' 'every,' 'none,' etc. Every science uses these expressions, constructing its sentences from its proper words and also using these common terms. [. . .]
>
> There is [. . .] a science which takes special care of these terms. This science is characterised by the fact that constructing its theorems, besides variable symbols it uses only these three kinds of terms as well as those terms that can be defined with their help. This science is called *formal logic*. Whereas those terms, belonging to the aforementioned three types,[121] and those that can be defined with their help are called *logical constants. Formal logic is then a science whose theorems are constructed exclusively from logical constants and variable symbols* (1934b, p. 41).[122]

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się chce myśleć formalnie poprawnie. Przepisuje ona, jak należy obchodzić się z pojęciami, mówi ona, że należy je definiować i opisuje, jak się to czyni. Mówi, jak się dochodzi do wyników pewnych przez wnioskowanie, jak się szuka dowodów, jak się wynajduje prawa przyrody itd. Krótko mówiąc, podaje ona metody badania naukowego, a przygotowaniem tej jej części jest logika teoretyczna. Rozpada się tedy logika w obszerniejszym znaczeniu na dwa działy: pierwszy, identyczny z logiką teoretyczną, stanowi podstawę dla formułowanych przez logikę praktyczną prawideł [. . .], drugi, podający sposoby, metody badania naukowego, i zwany dlatego metodologią'.

[120] 'Cały szereg twierdzeń logiki formalnej odznacza się tym, że występują w nich tylko takie wyrazy stałe, które występują w *każdej* nauce. Nie ma w nich wyrazów, które spotkać można tylko w zoologii, ani wyrazów, które by tylko chemii były właściwe. Tej okoliczności zawdzięcza logika formalna swe szerokie zastosowanie. Korzysta z niej—jak zobaczymy—każdy w większości swych rozumowań'.

[121] Besides the aforementioned terms we also mean such quantifiers as 'every, 'some,' etc. and logical conjunctions—remark is mine.

[122] 'We wszystkich jednak naukach występują, prócz terminów naukom tym właściwych, jeszcze pewne terminy wszystkim naukom wspólne.

Consequently, Ajdukiewicz defined theoretical logic through the notion of logical constant, basically limiting it to first-order logic. In his opinion the status of logic as a theoretical science depends on the origin of knowledge—cf. the quoted words from *Główne kierunki filozofii w wyjątkach z dzieł ich klasycznych przedstawicieli* (1923, p. 22).

His views on the status and origin of logical laws evolved distinctly. They should be considered in a wider context of his epistemological opinions, in particular his conventionalism. When he opted for radical conventionalism he treated logical laws as propositions recognised by virtue of axiomatic and deductive semantic directives, which were later called sense-rules. As a result, they were regarded as analytic propositions. Logic, thus understood, was a derivative towards the sense-rules. Moreover, logic was relativised to a given language, to a given conceptual apparatus. Thus it could change passing from one to the other conceptual apparatus. Importantly, this thesis, which Ajdukiewicz treated as generalisation of radical conventionalism, reminds us of Carnap's principle of tolerance, allowing a choice of language and logic.

After the war Ajdukiewicz gave up radical conventionalism, being influenced by epistemological considerations. He paid much more attention to the role of empirical data in recognising sentences. However, some traces of conventionalism (although not radical and extreme) can still be found in his later works. Nevertheless, his turn towards empiricism was clearly visible. In this context he also considered the problem of the status of logical laws.

He provided complete explanations of his views on this topic in the paper entitled 'Logika a doświadczenie' [Logic and Experience] (1947). He analysed the status of logical laws in a wider background, namely in the context of the problem of empiricism, i.e. the question whether only empirical sentences, based on experience, express reliable cognition and are the only ones to claim citizenship in science. In particular, he poses the question whether logical laws are of empirical origin or whether they are independent from experience.[123]

Terminami takimi są np. wyrażenia: "jest", "nie", "każdy", "żaden" itd. Wyrażeń tych używa każda nauka, budując swe zdania nie tylko z wyrazów sobie właściwych, lecz nadto również z tych terminów wspólnych. [. . .]

Istnieje [. . .] nauka, która te terminy ma pod swoją specjalną opieką. Nauka ta odznacza się tym, że dla budowania swych twierdzeń posługuje się obok symboli zmiennych wyłącznie tylko tymi trzema rodzajami terminów, oraz takimi, które się przy ich pomocy dają zdefiniować. Nauka ta zwie się *logiką formalną*. Owe zaś terminy należące do wymienionych wyżej trzech rodzajów, i te, które przy ich pomocy można zdefiniować, nazywają się *stałymi logicznymi*. *Logika formalna jest to tedy nauka, której twierdzenia zbudowane są wyłącznie ze stałych logicznych oraz z symboli zmiennych*'.

[123] It should be noted that in some period Łukasiewicz, the author of non-classical many-valued logics, claimed that experience could help solve the question which system of logic was fulfilled in reality (cf. his work 'Logistyka a filozofia' (1936), see also Sect. 3.2). Ajdukiewicz, who was not especially interested in non-classical logics, did not agree with Łukasiewicz's thesis, while opting for radical conventionalism—cf. Ajdukiewicz's article 'Zagadnienie empiryzmu a koncepcja znaczenia' [The Problem of Empiricism and the Concept of Meaning] (1964).

Ajdukiewicz concludes that two standpoints are possible. In one view—represented by radical empiricism—logical theorems are:

> sentences based on experience and [radical empiricism] acknowledges them as scientific theorems only if they bear the mark of being 'based on experience' (1949–1951, p. 295).[124]

The other—represented by moderate empiricism—regards:

> the laws of logic as analytic sentences and allowing them to be accepted as scientific assertions irrespective of the test of experience (1949–1951, p. 295).[125]

Ajdukiewicz claims that there is no conflict between these approaches: both can be correct in relation to another language. However, we should add:

> All the languages whose logical theory has been elaborated are—so far as the author knows—languages governed by axiomatic and deductive rules, hence languages in which analytic sentences may be accepted without basing them on experience. To this category belongs also, as it seems, the common, every day language. [. . .] In these languages there are theses, i.e. sentences which may be accepted without recourse to experience (1949–1951, p. 296).[126]

Yet, it is possible to construct languages without axiomatic rules, only with deductive rules. For such languages the thesis of radical empiricism will be correct. Thus logical laws will assume the character of empirical sentences. How can logical laws be empirically verified? According to Ajdukiewicz:

> It seems that it would be possible if logical theorems were treated as auxiliary hypotheses verified not separately but jointly with certain scientific hypotheses (1949–1951, pp. 296–297).[127]

Consequently, in light of the new empirical data logical laws can be changed jointly with empirical hypotheses. Thus logic would have mainly a methodological and not ontological sense. Interestingly, Ajdukiewicz's conception resembles the so-called holistic empiricism presented by Willard Van Orman Quine. According to the latter, 'the unit of empirical significance' is the whole of science whereas Ajdukiewicz allows certain fragments of science.

Ajdukiewicz also perceives certain usefulness of such an approach towards logical laws for natural sciences. He writes:

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[124] 'zdania oparte na doświadczeniu, i o tyle tylko, o ile one się tym "oparciem o doświadczenie" legitymują przyznaje im prawo występowania w charakterze twierdzeń naukowych' (1947, p. 17).

[125] 'prawa logiki za zdania analityczne, i pozwala uznawać je jako twierdzenia naukowe niezależnie od świadectwa doświadczenia' (1947, p. 17).

[126] 'Wszystkie znane mi dotychczas języki, których logiczna teoria jest wypracowana, są językami z dyrektywami aksjomatycznymi i dedukcyjnymi, a więc językami, w których wolno przyjmować zdania analityczne, nie opierając ich na doświadczeniu. Językiem takim zdaje się też być język potoczny. [. . .] W tych językach należą przede wszystkim twierdzenia logiki do zdań analitycznych, a więc do takich, które wolno przyjąć bez apelu do doświadczenia' (1947, p. 17).

[127] 'Wydaje się, że byłoby to możliwe w taki sposób, że traktowałoby się twierdzenia logiki jako hipotezy pomocnicze sprawdzane nie w izolacji, lecz łącznie z pewnymi hipotezami przyrodniczymi' (1947, p. 18).

> Some physicists presume that it is incompatible with common logic to maintain the fundamental assertions of the quantum theory (the principle of complementarity) and are inclined to reject some laws of this logic while retaining the physical theses. [. . .] At any rate, the above conception of a language without analytic sentences in which also the laws of logic would be reduced to the rank of hypotheses opens a way for this kind of possibilities (1949–1951, p. 297).[128]

It is complicated to choose and justify the validity and truth of one of the aforementioned conceptions. However, Ajdukiewicz can see here a compromising solution. He claims that the empiricists' stand can be regarded as a certain programme of scientific work, and in this situation one can hardly demand to show its truth. Since scientific programmes are neither necessarily true nor necessarily false, but they should be reasonable or unreasonable. In order to be reasonable they must prove to be purposeful and practicable. Consequently, scientific usage plays a decisive role. Ajdukiewicz adds:

> However, the actual course taken so far does not seem to be consistent with the program of radical empiricism (1949–1951, p. 299).[129]

Ajdukiewicz returned to similar problems, in particular the problem of justifying analytic propositions and possibilities of constructing an extremely empirical language. He coped with the first issue, for example in 'Le problème du fondement des propositions analytiques' [The Problem of the Justification of Analytic Propositions] (1958). He concluded that justifying analytic propositions required, however, reference to experience.[130] On this occasion it is worth seeing that having considered the problem of empirical justification of logical laws Ajdukiewicz relativised it to the choice of language but here he omitted this relativisation.

Ajdukiewicz tackled the other problem in 'Zagadnienie empiryzmu a koncepcja znaczenia' [The Problem of Empiricism and the Concept of Meaning] (1964), distinguishing between the epistemological version and methodological version of the problem. He claimed that there were no languages in which only empirical sentences had cognitive value but at the same time, he allowed the possibility of constructing such languages. They would have neither axiomatic nor deductive sense-rules. Yet, he noticed that it would require a new conception of meaning. Since this issue is not of special interest to our considerations, we will not analyse it here in detail (it is discussed for instance in: Jedynak (2003)).

Speaking about the problem of the status of logical laws and the relationships between logic and experience it is worth stressing that although Ajdukiewicz

[128] 'Niektórzy fizycy wyrażają przypuszczenie, że utrzymanie zasadniczych twierdzeń teorii kwantów (zasada komplementarności) nie daje się pogodzić ze zwyczajną logiką i byliby skłonni niektóre prawa tej logiki odrzucić, a zachować swoje tezy fizykalne. [. . .] W każdym razie wyłożona wyżej koncepcja języka bez zdań analitycznych, w których także prawa logiki spadłyby do rzędu hipotez, otwiera drogę dla tego rodzaju możliwości' (1947, p. 19).

[129] 'Nie wydaje się jednak, żeby jej dotychczasowy przebieg był z programem skrajnego empiryzmu zgodny' (1947, p. 21).

[130] At this point, note again the convergence between Ajdukiewicz's and Quine's views; the latter claimed that the division into analytic and synthetic propositions was illusory.

showed 'solemn significance of modern logic to formulate properly and solve great philosophical problems, transmitted by tradition' (1937, p. 271) he thought (just like Tadeusz Czeżowski) that logic was neutral towards the dispute over universals.

Furthermore, Ajdukiewicz discussed and characterised the status of mathematics and logic as a science while reflecting on the classification of sciences, cf. *Główne zasady metodologii nauk i logiki formalnej* [Main Principles of the Methodology of Sciences and Formal Logic] (1928) and *Logika pragmatyczna* [Pragmatic Logic] (1965b). He made a twofold division of sciences: (1) with regard to the type of the used argumentations, and (2) regarding the final premises they are based on. In the first category (1) he marked out deductive and inductive sciences whereas in the other (2) deductive sciences (based on axioms), empirical sciences (based on axioms and perceptive propositions) and the humanities (based on axioms, perceptive propositions and understanding of other people's statements). Notably, in both divisions deductive sciences have the same domain embracing mathematics and logic whereas inductive sciences include empirical sciences and the humanities.

Considering the issues that are of our interest, it is worth analysing Ajdukiewicz's views on deductive sciences. He distinguishes several stages of the development of deductive sciences: intuitive pre-axiomatic, intuitive axiomatic and abstract axiomatic. In the first stage all sentences which are obvious for all researchers of the given field were regarded as primitive theorems and all expressions understood without definitions as primitive terms. In the second stage we have a fixed list of primitive terms taken in the existing meaning and axioms, i.e. sentences raising no doubts. In the third stage primitive terms lose their basic meanings—their meanings are defined by accepted axioms and only by them. Yet, here another, in a way, higher degree is possible: the axiomatic system can be treated as a formalised system, reducing the deductions of the theorems in the system to a game of symbols without any meaning, game that is played according to *a priori* rules of inference of a purely formal character, referring only to the shapes of the inscriptions (their forms). Then the question about the truth of the axioms is completely senseless. Let us stress that Ajdukiewicz began his research by reflecting on the deductive systems in the formalised phase (cf. his *Habilitationsschrift* entitled *Z metodologii nauk dedukcyjnych* (1921), which has been discussed at the beginning of this section).

The formalised systems of axioms can generally be interpreted in many different ways. Ajdukiewicz called them hypothetical-deductive or neutral-deductive systems (cf. 1960b). As a matter of fact, the axioms of these systems do not mean anything, and accordingly, there are no grounds to accept or reject them. Consequently, a similar attitude should be adopted towards theorems that have been deduced from them. As for the non-formalised systems, based on the axioms that are meaningful and asserted sentences, Ajdukiewicz regarded them as assertive-deductive. In these systems the theorems deduced from the axioms can be asserted equally as axioms. Ajdukiewicz (cf. 1960b) wrote:

> The most wide-spread opinion considers the axiomatic systems of mathematics to be assertive-deductive. The methodological structure of these systems is contended to be the following: first of all, and independently from the assertion of the theorems, the axioms are

asserted; afterwards, and by way of deduction, one is brought likewise to assert the theorems on the ground of having asserted the axioms (1960b, p. 211).

However, a natural question arises: on what basis are axioms recognised in any assertive-deductive systems, and consequently in mathematics? Ajdukiewicz answers (cf. 1965a):

> Now the axioms of an assertive-deductive system are not validated indirectly by the other sentences of the same system. They can be validated indirectly only as theorems of another system, from the axioms of which they can be deduced. But even if they are deduced from the axioms of another system, they are validated only to the same extent as these. As we see, the basis of any assertive-deductive system must, in ultimate analysis, be provided by axioms that are no more validated indirectly, i.e., are no longer inferred from other sentences, but whose validation is a direct one. Otherwise, one would either fall into a *regressus infinitus*, or base all one's affirmations, ultimately, upon unfounded premises, thus falling into the vice of *petitio principii*. Consequently, the possibility of constructing assertive-deductive systems of founding these unavoidably depends on the existence of a direct method of foundation (1960b, p. 213).

Ajdukiewicz distinguishes three methods of validation, which are listed in literature: (1) validation of sentences as the theorems directly based on observations (called protocol sentences), (2) reference to intuition and (3) validation by terminological conventions. The first method is not, however, satisfactory since its application would make the deductive sciences similar to the empirical sciences, which would not lead to sure and undeniable knowledge expected from the deductive sciences. On the other hand, the nature and character of the axioms of deductive sciences do not allow them to be validated by means of empirical methods, '[they] affirm nothing that could be seen or heard' (1960b, p. 216). The second method is not satisfactory because of its unclear concept of intuition, '[...] because of the difficulty of controlling it, of the impossibility of settling the disputes between those who appeal to its testimony' (1960b, p. 216). The third method seems to be least dubious. But it 'does not secure the truth of [...] theorems, unless they are also founded on a corresponding existential premise' (1960b, p. 215).

Having shown that there was no proper method that would allow validating directly the axioms of a deductive system, Ajdukiewicz concluded:

> [...] the axiomatic systems of mathematics would lose nothing by being constructed as neutral-deductive ones by the mathematicians and treated as assertive-reductive ones by the naturalists that would use them (1960b, p. 243).

Nevertheless, a certain difficulty appears at this point. In a deductive system the rules of deduction based on certain logical laws are necessary. Do we not need first to assume some system of logic? Ajdukiewicz proposes a solution, stating:

> [...] but to deduce sentences from one another, one need not prove that one is proceeding by unfailing rules. It is enough simply to proceed by them. Therefore, to construct neutral-deductive systems it is unnecessary to presuppose the theorems of logic. These are needed only for reflecting upon such systems from the methodological point of view, so as to evaluate the correctness of their structure (1960b, p. 216).

Therefore, mathematicians can develop mathematics constructing neutral-deductive systems according to certain accepted principles, not being much concerned about the evaluation of their accepted deductive rules from the point of their correctness and reliability, the latter being the task of logicians and those dealing with metamathematics who analyse the axiomatic systems as such.

3.7 Alfred Tarski

If we want to speak about Alfred Tarski's views on mathematics and logic[131] we should state that Tarski—though generally being a mathematician and logician, and dealing mainly with these fields—was also interested in philosophy and actively involved in the philosophical life of his days. He wrote himself:

> Being a mathematician (as well as a logician, and perhaps a philosopher of a sort) [. . .] (1944, p. 369).

The whole environment, in which Tarski developed intellectually, was connected with philosophy and became saturated with philosophy. Tarski studied philosophy under the supervision of Tadeusz Kotarbiński.[132] Łukasiewicz and Leśniewski, who taught Tarski logic, were also philosophers by profession. Tarski was a member of various scientific philosophical societies in which he had various functions. He participated in diverse conferences and scientific congresses on philosophy. He also published in specialist periodicals (for instance *Przegląd Filozoficzny*, *Ruch Filozoficzny*, *Erkenntnis, Philosophy and Phenomenological Research*, *Revue Internationale de Philosophie* or *History and Philosophy of Logic*).

He realised that his works, especially *Pojęcie prawdy w językach nauk dedukcyjnych* [The Concept of Truth in the Languages of the Deductive Sciences] (1933), had philosophical value:

> But in its essential parts the present work deviates from the main stream of methodological investigations. Its central problem—the construction of the definition of true sentence and establishing the scientific foundations of the theory of truth—belongs to the theory of knowledge and forms one of the chief problems of this branch of philosophy. I therefore hope that this work will interest the student of the theory of knowledge above all and that he

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[131] On Tarski as a philosopher see for example Woleński (1993) or (1995b).

[132] Tarski held Kotarbiński in great esteem and regarded him as his teacher. It was to him that he dedicated the collection of his fundamental logical works *Logic, Semantics, Metamathematics* (1956), writing 'To his teacher TADEUSZ KOTARBIŃSKI. The author' (in the second edition published in 1983 after Kotarbiński's death the dedication was 'To the memory of his teacher TADEUSZ KOTARBIŃSKI. The author'). When asked by one of his doctoral students at Berkeley, who his teacher had been, he answered without any hesitation: 'Kotarbiński'—although the supervisor of his doctoral dissertation was Leśniewski, and his other teachers included Łukasiewicz and Sierpiński. Kotarbiński's photo always occupied a privileged place on Tarski's desk.

> will be able to analyse the results contained in it critically and to judge their value for further researches in this field, without allowing himself to be discouraged by the apparatus of concepts and methods used here, which in places have been difficult and have not hitherto been used in this field in which he works (1956, pp. 226–267).[133]

Tarski also defended philosophy. In his letter to Alonzo Church concerning the editorial policy of *Journal of Symbolic Logic* he wrote:

> I cannot deny, however, that personally I should be happy if also another type of articles appeared in the *Journal* in a larger amount that they appeared so far; in fact articles which could be regarded as belonging not to logic in the strict sense but to philosophy, to mathematics, or to other disciplines—under the condition, that these articles either apply methods of modern logic in an essential way or have implications which are essentially relevant to logic (quoted after Woleński 1995b, p. 333).

Although Tarski knew the current literature on philosophy very well and—as many of his acquaintances or friends said—was always ready to discuss philosophical topics, he hardly ever spoke on this topic in his publications. Neither did he develop (e.g. in his seminars) his views because he wanted to give them a more mature form.[134] For instance, he wrote works on topics relating to the three main trends in the philosophy of mathematics, i.e. logicism, intuitionism and formalism. His formal results contributed greatly to the development of these trends but he never followed any of them—he did not accept the philosophical premises of these trends. He also dealt with many-valued logics and with modal logic but he was never involved in any philosophical discussions concerning these logics.[135] On the contrary, he stressed on many occasions that logical and metamathematical

[133] 'W istotnej swej części praca niniejsza leży jednak na uboczu od głównego łożyska badań metodologicznych. Centralne jej zagadnienie—konstrukcja definicji zdania prawdziwego i ugruntowanie naukowych podstaw teorii prawdy—należy do zakresu teorii poznania i zaliczane nawet bywa do naczelnych problematów tej gałęzi filozofii. Toteż liczę na to, że pracą tą zainteresują się w pierwszym rzędzie teoretycy poznania, że—nie zrażając się uciążliwym miejscami aparatem pojęć i metod, nie stosowanych dotąd w uprawianej przez nich dziedzinie wiedzy—zanalizują oni krytycznie zawarte w tej pracy wyniki i zdołają je wyzyskać w dalszych dociekaniach z tego zakresu' (1933, p. 115).

[134] P. Suppes wrote (1998, p. 80): '[. . .] he was extraordinarily cautious and careful in giving any direct philosophical interpretation of his work. In contrast, he was in conversation willing to express a much wider range of philosophical opinions—I know this from my own experience and also from reports of colleagues.'

[135] At this point, it is worth adding that in his talk during the Bicentennial Conference at Princeton in December 1946 Tarski expressed his doubts concerning many-valued logic (cf. Sinaceur 2000, p. 25): 'Historically the decision problem has had a direct bearing on the origin of many-valued systems logic. At one time it seems that logicians in general felt that the solution of the decision problem for the classical two-valued logic was too difficult to attack directly and that the problem should be attempted piecemeal, that is by first solving the decision problem for various subsystems of the classical calculus. It was in this way that the multi-valued systems were created: for they are in most cases just that—subsystems of the classical calculus [. . .]. In passing from this topic—and I hope that no creator of many-valued logics are present, so I may speak freely—I should say that the only one of these systems for which there is any hope of survival is that of Birkhoff and von Neumann. This system will survive because it does fulfil a real need.'

investigations should not be limited by any *a priori* philosophical assumptions. In his paper 'Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften' [Fundamental Concepts of the Methodology of the Deductive Sciences] he wrote:

> In conclusion it should be noted that no particular philosophical standpoint regarding the foundations of mathematics is presupposed in the present work (1956, p. 62).[136]

In 'Contribution to the Discussion of P. Bernays 'Zur Beurteilung der Situation in der beweistheoretischen Forschung" (1954) we find the following words:

> As an essential contribution of the Polish school to the development of metamathematics one can regard the fact that from the very beginning it admitted into metamathematical research all fruitful methods, whether finitary or not.

Tarski followed his teachers' attitude towards philosophy, which was characteristic of the Lvov-Warsaw School. His stand was strengthened by his contacts with the Vienna Circle. Tarski's attitude was anti-metaphysical. He supported the idea of scientific philosophy. Being influenced mainly by Kotarbiński he accepted the programme of the so-called small philosophy which does not aim at creating big universal systems of philosophy, but wants to conduct a systematic analysis of concepts used in philosophy. Thus it is rather a minimalistic philosophy, which is characterised by anti-speculative nature and certain scepticism towards many problems of traditional philosophy. In the Lvov-Warsaw School there was a belief that if philosophy was developed clinging to proper methodological standards its scientific character would be strengthened. One should add that the attitude adopted by the School was less radical than that of the Vienna Circle. In particular, it was allowed to reflect on universals, acknowledging that the dispute over universals could be conducted in a more exact form and could be solved.

Tarski saw some danger in using the logical apparatus to analyse philosophical problems. He expressed his fears discussing the work of Maria Kokoszyńska 'W sprawie względności i bezwzględności prawdy' [On Relativity and Absoluteness of Truth] (1936a). In his opinion using such an apparatus can lead to simplifying philosophical problems to some extent and as a result, losing their essence. Since it need not be completely clear whether the new, more precise formulations of some problem show all of the intentions of its creators. On the other hand, such a logical analysis forces exactness and precision in formulating philosophical problems, which allows us to avoid discussions and divagations leading nowhere. Tarski himself was always especially sensible to problems connected with the debate over the universals.

Tarski firmly emphasised his sympathy towards empiricism. In his opinion, one uses two methods in science: deduction and induction. He was inclined to identify mathematics with the deductive method. On many occasions, he remarked on the

[136] 'Zum Schluß sei bemerkt, das die Voraussetzung eines bestimmten philosophischen Standpunktes zu der Grundlagen der Mathematik bei den vorliegenden Ausführungen nicht erforderlich ist' (1930, p. 363).

relationships between formal and empirical sciences. He claimed that there was no clear boundary separating these sciences. Like John Stuart Mill he was inclined to state that in both cases—as for the sources and origin of logical and mathematical knowledge on the one hand and empirical on the other—we dealt with accumulated experience. In his letter to Morton White he wrote:

> I would be inclined to believe (following J. S. Mill) that logical and mathematical truths do not differ in their origin from empirical truth—both are results of accumulated experience (Tarski 1987, p. 31).

He allowed the possibility of rejecting logical and mathematical theses on the empirical basis. In the mentioned letter to White he admitted:

> I think that I am ready to reject logical premises (axioms) of our science in exactly the same circumstances in which I am ready to reject empirical premises (e.g., physical hypotheses): and I do not think that I am an exception in this respect.

Yet, as he writes in the letter, logical axioms are of such general nature that experience seldom 'touches' them. However, there is no difference here as far as the principle is concerned. Tarski admits that he is inclined to imagine that 'certain new experiences of a very fundamental nature may make us inclined to change just some axioms of logic' (1987, p. 31). In his opinion, the new results of quantum mechanics seem to indicate this possibility. The fact that—so far—we have not been inclined to reject axioms of logic may result from the truth that logical truths are not only more general but also much older than physical theories or even geometrical axioms.

Moreover, Tarski expressed some scepticism concerning the concept of tautology and its role in defining logic and mathematics. He thought that it was a vague concept, which was connected with his conviction that there was no distinct demarcation line between logical and factual truths. In his diary Carnap noted on 22 February 1930:

> Between 8 and 11 o'clock with Tarski in Café. On monomorphism, on tautology, he is not inclined to admit that it does not say anything about the world; he thinks that between tautological and empirical sentences there is only a slight and subjective difference[137] (quoting after Haller 1992, p. 5).

Tarski tried to define logical notions in the spirit of Klein's Erlangen Programm. The latter treated geometry as theories of invariants and thus he characterised various geometries. In 'What Are Logical Notions?' (1986a) Tarski defined logical concepts as invariants under all one-one transformations of the world onto itself.[138] He concluded that all the notions of the system of *Principia Mathematica* by

[137] '8-11 h mit Tarski im Café. Über Monomorphie, über Tautologie, er will nicht zugeben, daß sie nichts über die Welt sagt; er meint zwischen tautologischen und empirischen Sätzen sei ein bloß gradueller und subjektiver Unterschied.'

[138] Tarski's reflections on logical notions referred to his works written with Adolf Lindenbaum. Let us add that Lindenbaum also wrote three papers on the philosophy of mathematics; cf. Lindenbaum (1930), (1931) and (1936).

Whitehead and Russell are logical in this sense. In the aforementioned work he also considers whether all mathematical notions are logical in this sense (thus he asks a question in the spirit of logicism). He reaches the conclusion that the answer is not unambiguous—all depends on the way one constructs mathematics: whether based on the theory of types or axiomatic set theory like that of Zermelo, Fraenkel, von Neumann or their followers. If the first possibility is accepted the answer is positive since set theory constructed within the framework of the theory of types is a part of logic. In the second case the answer is negative—the relation 'being an element' stops being a logical notion. Tarski refrains from taking an unambiguous standpoint. He only writes:

> A monistic conception of logic, set theory, and mathematics, where the whole of mathematics would be a part of logic, appeals, I think, to a fundamental tendency of modern philosophers. Mathematicians, on the other hand, would be disappointed to hear that mathematics, which they consider the highest discipline in the world, is a part of something as trivial as logic; and they therefore prefer a development of set theory in which set-theoretical notions are not logical notions (1986a, p. 153).

And he adds:

> The suggestion which I have made does not, by itself, imply any answer to the question of whether mathematical notions are logical (1986a, p. 153).

This attitude is characteristic of Tarski and the whole Warsaw School of Logic. As one can see it lies on making various possible precise standpoints and at the same time avoiding adopting any definite standpoint.

There is still another thing that is related to the above discussed issues, namely the attempt to define what logic is. Ajdukiewicz proposed to define formal logic as a science 'with theorems which are constructed only from logical constants and variable symbols' (Ajdukiewicz 1934b, p. 41; cf. Sect. 3.6).[139] In his work 'O pojęciu wynikania logicznego' [On the Concept of Logical Consequence], (1936) Tarski paid attention to the fact that the division of terms into logical and non-logical—although not completely optional—is, however, arbitrary to some extent. He wrote:

> [. . .] no objective grounds are known to me which permit us to draw a sharp boundary between the two groups of terms. It seems to be possible to include among logical terms some which are usually regarded by logicians as extra-logical without running into consequences which stand in sharp contrast to ordinary usage. In the extreme case we could regard all terms of the language as logical. The concept of *formal* consequence would then coincide with that of *material* consequence. The sentence X would in this case follow from the class K of sentences if either X were true or at least one sentence of the class K were false (1956, pp. 418–419).[140]

[139] 'której twierdzenia zbudowane są wyłącznie ze stałych logicznych oraz z symboli zmiennych.'

[140] '[. . .] nie znam żadnych obiektywnych względów, które by pozwalały przeprowadzić dokładną granicę między obiema kategoriami terminów. Przeciwnie, mam wrażenie, że—nie naruszając wyraźnie intuicji potocznych—można zaliczyć do terminów logicznych i takie terminy, których logicy do tej kategorii nie zaliczają. Skrajny byłby ten przypadek, gdybyśmy wszystkie wyrazy języka potraktowali jako logiczne: pojęcie wynikania *formalnego* pokryłoby się wówczas z

Being influenced by Leśniewski, Tarski was—at least in some period—a follower of intuitive (or intuitionistic) formalism (cf. Sect. 3.4). In 'Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften' he admitted:

> [. . .] my personal attitude towards this question agrees in principle with that which has found emphatic expression in the writings of S. Leśniewski on the foundations of mathematics and which I would call *intuitionistic formalism* (1956, p. 62).[141]

According to this view, the language of logic—unambiguously and fully codified—always communicates 'something' and 'about something.' Logic can be, therefore, treated—according to Henryk Hiż, referring to Leśniewski himself—as a 'formal exposition of intuition' (cf. Woleński 1995b, p. 336). Then Tarski rejected intuitionistic formalism. In the reprint of the aforementioned paper in the volume of *Logic, Semantics, Metamathematics* (1956 and 1983) he added the following footnote:

> This last sentence expresses the views of the author at the time when this article was originally published and does not adequately reflect his present attitude (1956, p. 62).

In fact, Tarski favoured Łukasiewicz's attitude towards logic over the standpoint of Leśniewski (cf. Sects. 3.2 and 3.4). His approach enabled him, for example, to deal with various systems of logic without accepting their ideological or philosophical presumptions. It also influenced his understanding of metamathematical investigations, which should be conducted without accepting any philosophical assumptions and in which one should be able to use all research methods, provided that they are correct (cf. the mentioned citation from Tarski 1954).

Tarski neither explained why he rejected his earlier views connected with intuitionistic formalism nor described his new views in detail, which can be treated as a sign of the fact that his investigations concerning logic and the foundations of mathematics became increasingly independent from his preliminary philosophical assumptions. However, in his article (1995b) Woleński suggests (using the term 'ideology' instead of 'philosophical presumptions') that certain traces of intuitionistic formalism remained in Tarski's writings. In particular, they can be seen in his monograph on the concept of truth (1933), in which he explicates the relations between meaning and language. In Woleński's opinion, it was this ideology that constituted the basis of the general context of the origin of semantics. Yet, it was only an ideology and not a collection of philosophical presumptions, determining how logic should be developed. It also explains why Tarski preceded the above quoted declaration of the accordance of his personal attitude 'with that which has found emphatic expression in the writings of S. Leśniewski [. . .] and which I

pojęciem wynikania *materialnego*—zdanie *X* wynikałoby ze zdań klasy *K* wtedy i tylko wtedy, gdyby było prawdziwe, bądź choć jedno zdanie klasy *K* byłoby fałszywe' (1936, p. 67).

[141] '[. . .] meine persönliche Einstellung in diesen Fragen im Prinzip mit dem Standpunt übereinstimmt, dem S. Leśniewski in seinen Arbeiten über die Grundlagen der Mathematik einen prägnanten Ausdruck gibt und den ich als "intuitionistischen Formalismus" bezeichnen werde' (1930, p. 363).

would call *intuitionistic formalism*' (1956, p. 62) with the following words, 'Only incidentally, therefore I may mention . . .' (1956, p. 62).[142]

As we have already mentioned Tarski was convinced that his work on the concept of truth contributed to the old philosophical problem. He used to stress that his intention was to present a modern interpretation of the Aristotelian concept of truth. In 'The Semantic Conception of Truth and the Foundations of Semantics' he wrote, 'We should like our definition to do justice to the intuitions which adhere to the *classical Aristotelian conception of truth*' (1944, p. 343). In his paper 'Truth and Proof' we can find the following words, 'We shall attempt to obtain here a more precise explanation of the classical conception of truth, one that could supersede the Aristotelian formulation while preserving its basic intentions' (1969, p. 64).

Tarski opposed the nihilistic conception of truth (as he wrote (1969, p. 69) the term was suggested by Kotarbiński). In this conception the words 'true' and 'truth' have no independent meanings and can be eliminated from every context. For example, instead of saying 'It is true that all cats are black' we can simply say 'all cats are black.'[143] Tarski states: 'Employing the terminology of medieval logic, we can say that the word "true" can be used syncategorematically in some special situations, but it cannot ever be used categorematically' (1969, p. 68).

In his papers 'The Semantic Conception of Truth and the Foundations of Semantics' (1944) and 'Truth and Proof' (1969) Tarski pays attention to the difficulties caused by the acceptance of the nihilistic conception. In the first paper (cf. 1944, p. 359) he writes about the theorem that all consequences of true sentences are true. From this theorem, important from the point of logic, the word 'true' cannot be eliminated in the mentioned way; the use of this word is essential here. In the other paper (cf. 1969, p. 69) he gives the example of a historian of science who wants to formulate a hypothesis that since the known texts of some mathematician, which he studied, are true the same will apply to all his works, including those that may be discovered in the future. Sharing the nihilistic approach to the notion of truth does not allow formulating this hypothesis. Tarski concludes:

> One could say that truth theoretical "nihilism" pays lip service to some popular forms of human speech, while actually removing the notion of truth from the conceptual stock of the human mind (1969, p. 69).

In Tarski's works dedicated to the concept of truth, in particular his standard position *Pojęcie prawdy w językach nauk dedukcyjnych* (1933), Kotarbiński's influences are clearly visible (cf. the beginning of this section), especially in two issues: the concept of a sentence and the very definition of truth.[144] At the very beginning of his work (1933) Tarski referred to Kotarbiński's *Elementy logiki formalnej, teorji poznania i metodologji* (1926; see also 1929) and explicitly declared that 'in writing the present article I have repeatedly consulted this book

[142] 'Nur nebenbei erwähne ich deshalb, daß . . . .' (1930, p. 363).

[143] This example was taken from Tarski's work (1969).

[144] On the concept of truth in Tarski and his predecessors see Murawski and Woleński (2008b).

and in many points adhered to the terminology there suggested' (1956, footnote 1, p. 153).[145] Tarski accepted the classical correspondence concept of truth from Kotarbiński's formulation as he wrote himself in footnote 4 on p. 4 of (1933).[146]

Another trace of Kotarbiński's influence is the fact that Tarski did not show in his work (1933) how to define 'truth' as such but how to define the expression 'true sentence of language *L*.'

In the interwar period Tarski, influenced by Kotarbiński and Leśniewski, treated language as a set of sentences understood in a strictly nominalistic way as physical objects. In 'Fundamentale Begriffe der Methodologie der deduktiven Wissenschaften' he wrote:

> The sentences are most conveniently regarded as inscriptions, and thus as concrete physical bodies (1956, p. 62). [147]

Tarski was obviously aware of the fact that such understanding of the sentences led to certain difficulties in logical investigations, especially in metalogic and metamathematics. In *Pojęcie prawdy* (1933) he refers to Kotarbiński's *Elementy* (1926), distinguishing three interpretations of the notion of 'sentence': idealistic, psychological and nominalistic, choosing the latter for further investigations, which resulted from reism, which he accepted (cf. Sect. 3.5). In the footnote Tarski wrote:

> Statements (sentences) are always treated here as a particular kind of expression, and thus as linguistic entities. Nevertheless, when the terms 'expression', 'statement', etc., are interpreted as names of concrete series of printed signs, various formulations which occur in this work do not appear to be quite correct, and give the appearance of a widespread error which consists in identifying expressions of like shape. [. . .] In order to avoid both objections of this kind and also the introduction of superfluous complications into the discussion, which could be connected among other things with the necessity of using the concept of likeness of shape, it is convenient to stipulate that terms like 'word', 'expression', 'sentence', etc., do not denote concrete series of signs but whole classes of such series which are of like shape with the series given [. . .] (1956, footnote 1, p. 156).[148]

Therefore, Tarski does not treat language as a series of concrete single signs but as a collection of expressions-classes.

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[145] 'z książki tej korzystałem niejednokrotnie przy redagowaniu niniejszych rozważań, dostosowując się w wielu punktach do ustalonej tam terminologii' (1933, footnote 1, p. 2).

[146] [Very similar formulations are found in Kotarbiński, T. (37)] [Kotarbiński (1926) is meant here—remark is mine] (1956, footnote 2, p. 155).

[147] 'Die Aussagen sind ihrerseits am bequemsten als Schriftzeichen, also als konkrete physische Körper zu betrachten' (1930, p. 363).

[148] 'Zdania traktujemy tu stale jako pewnego rodzaju wyrażenia, a więc jako twory językowe. Jeśli jednak terminy "wyrażenie", "zdanie" itd. interpretować jako nazwy konkretnych napisów, to różne sformułowania, zawarte w niniejszej pracy, nie są zupełnie poprawne i stwarzają pozory pospolitego błędu, polegającego na utożsamianiu wyrażeń równokształtnych. [. . .] Aby uniknąć podobnych zarzutów i nie wprowadzać przy tym pewnej zbędnej komplikacji do rozważań związanej m.in. z koniecznością operowania pojęciem równokształtności, dogodnie jest umówić się, że terminy takie jak "wyraz", "wyrażenie", "zdanie" itd. oznaczać będą stale nie konkretne napisy, a całe klasy napisów, równokształtnych z pewnym napisem danym [. . .]' (1933, pp. 5–6).

In *Pojęcie prawdy* there are at least four different concepts of a sentence: (1) as an expression of a concrete syntactic category, (2) as a psychophysical product, (3) as a physical body and (4) as a function without free variables (i.e. expressions of a certain logical category). In the first sense (1) sentences are distinguished by means of purely structural properties (cf. 1933, p. 16; 1956, p. 166). The second understanding (2) has the fault that the supposition stating that there are infinitely many expressions becomes nonsensical (cf. 1933, p. 25; 1956, p. 174). The last sense (3) causes another difficulty, especially in the context of metatheoretical investigations. Tarski writes:

> The kernel of the problem is then transferred to the domain of physics. The assertion of the infinity of the number of expressions is then no longer senseless and even forms a special consequence of the hypotheses which are normally adopted in physics or in geometry (1956, footnote 2, p. 174).[149]

Regardless of the variety of the meanings of a sentence, Tarski stresses the finitistic character of language. Moreover, he treats it as a fact of fundamental importance. In the footnote he claims:

> In the course of our investigation we have repeatedly encountered similar phenomena: the impossibility of grasping the simultaneous dependence between objects which belong to infinitely many semantic categories: the lack of terms of 'infinite order'; the impossibility of including, in *one* process of definition, infinitely many concepts, and so on. [. . .] I do not believe that these phenomena can be viewed as a symptom of the formal incompleteness of the actually existing languages—the cause is to be sought rather in the nature of language itself; language, which is a product of human activity, necessarily possesses a 'finitistic' character, and cannot serve as an adequate tool for the investigation of facts, or for the construction of concepts of an eminently 'infinitistic' character (1956, footnote 1, p. 253).[150]

Let us add that these words correspond to Kotarbiński's attitude towards language.

Firstly, Tarski clearly and firmly distinguished between colloquial, natural and formalised languages. In the conclusion of *Pojęcie prawdy* he wrote:

> Philosophers who are not accustomed to use deductive methods in their daily work are inclined to regard all formalised languages with a certain disparagement, because they contrast these 'artificial' constructions with the one natural language—the colloquial

[149] 'Punkt ciężkości zagadnienia przenosi się wówczas do fizyki, twierdzenie o nieskończonej liczbie wyrażeń przestaje być niedorzeczne i przedstawia nawet pewną specjalną konsekwencję założeń, normalnie przyjmowanych w fizyce lub w geometrii' (1933, footnote 23, pp. 25–26).

[150] 'Kilkakrotnie już zetknęliśmy się w toku rozważań z pokrewnymi zjawiskami: z niemożliwością uchwycenia równoczesnej zależności między przedmiotami, należącymi do nieskończenie wielu kategorii semantycznych, z brakiem wyrazów "nieskończonego rzędu", z niemożliwością objęcia *jednym* procesem definiowania nieskończenie wielu pojęć itp. [. . .] Nie sądzę, by można było traktować te zjawiska jako symptom niedoskonałości formalnej istniejących aktualnie języków—przyczyna tkwi raczej w samej istocie języka: język, będąc wytworem działalności ludzkiej, nosi z konieczności "finitystyczny" charakter i nie może służyć jako adekwatne narzędzie do badania faktów lub konstruowania pojęć natury wybitnie "infinitystycznej"'(1933, p. 102).

> language. For that reason the fact that the results obtained concern the formalised languages most exclusively will greatly diminish the value of the foregoing investigations in the opinion of many readers. It would be difficult for me to share this view. [. . .] Whoever wishes, in spite of all difficulties, to pursue the semantics of colloquial language with the help of exact methods will be driven first to undertake the thankless task of a reform of this language. He will find it necessary to define its structure, to overcome the ambiguity of the terms which occur in it, and finally to split the language into a series of languages of greater and greater extent, each of which stands in the same relation to the next in which a formalised language stands to its metalanguage. It may, however, be doubted whether the language of everyday life, after being 'rationalised' in this way, would still preserve its naturalness and whether it would not rather take on the characteristic features of the formalised languages (1956, p. 267).[151]

Therefore, it turns out that properly formalised semantics is impossible for natural languages.

Tarski did not identify formalised and artificial languages. In his opinion the expressions of formalised languages are ascribed meanings:

> It remains perhaps to add that we are not interested here in 'formal' languages and sciences in one special sense of the word 'formal'; namely sciences to the signs and expressions of which no material sense is attached. For such sciences the problem here discussed has no relevance, it is not even meaningful. We shall always ascribe quite concrete and, for us, intelligible meanings to the signs which occur in the languages we shall consider. [. . .] The expressions which we call sentences still remain sentences after the signs which occur in them have been translated into colloquial language (1956, pp. 166–167).[152]

It is hard to say unambiguously what Tarski understood as meaning in this context. In *Pojęcie prawdy* he writes that formalised languages can be characterised as 'artificially constructed languages in which the sense of every expression is

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[151] 'Filozofowie, nie przyzwyczajeni do stosowania metod dedukcyjnych w swej codziennej pracy naukowej, skłonni są traktować wszelkie języki sformalizowane z pewnym lekceważeniem, przeciwstawiając tym "sztucznym" tworom jedyny język naturalny—język życia potocznego. Dlatego też w oczach niejednego z czytelników jako moment, istotnie obniżający wartość powyższych rozważań, zarysuje się zapewne ta okoliczność, że uzyskane wyniki dotyczą niemal wyłącznie języków sformalizowanych. Z poglądem tym trudno by im było się zgodzić [. . .]. Ktoś, kto pragnąłby mimo wszelkie trudności uprawiać ścisłymi metodami semantykę języka potocznego, musiałby uprzednio podjąć się niewdzięcznej pracy nad "reformą" tego języka: musiałby sprecyzować jego strukturę, usunąć wieloznaczność występujących w nim terminów, rozbić wreszcie język na szereg coraz to obszerniejszych języków, z których każdy pozostawałby w tym samym stosunku do następnego co język sformalizowany do swego metajęzyka. Wątpić jednak wolno, czy "zracjonalizowany" na tej drodze język potoczny zachowałby swą cechę "naturalności" i czy nie zyskałby wówczas charakterystycznych znamion języków sformalizowanych' (1933, pp. 115–116).

[152] 'Zbyteczne jest może dodawać, że nie interesują tu nas wcale języki i nauki "formalne" w pewnym specyficznym znaczeniu tego wyrazu, a mianowicie tego rodzaju nauki, iż występującym w nich znakom i wyrażeniom nie przypisuje się żadnego intuicyjnego sensu; w odniesieniu do takich nauk postawione tu zagadnienie traci wszelką rację bytu i przestaje być po prostu zrozumiałe. Znakom występującym w tych językach, których dotyczą niniejsze rozważania, przypisujemy zawsze całkiem konkretne i zrozumiałe dla nas znaczenie [. . .]; wyrażenia, które nazywamy zdaniami, pozostają zdaniami i po przełożeniu zawartych w nich znaków na język potoczny [. . .]' (1933, p. 17).

unambiguously determined by its form' (1956, pp. 165–166).[153] On the other hand, when Tarski speaks about the translatability of languages he seems to suggest that meaning is not completely designated by the syntactical properties of expressions. Woleński writes (1995b) that Tarski tried to avoid considerations on the nature of meaning.

In his later works Tarski was not inclined to set a boundary between formalised languages and the colloquial language. In 'Truth and Proof' he wrote:

> I should like to emphasize that, when using the term "formalized languages", I do not refer exclusively to linguistic systems that are formulated entirely in symbols, and I do not have in mind anything essentially opposed to natural languages. On the contrary, the only formalized languages that seem to be of real interest are those which are fragments of natural languages (fragments provided with complete vocabularies and precise syntactical rules) or those which can at least be adequately translated into natural languages (1969, pp. 69–70).

In his work 'The Semantic Conception of Truth' (1944) he claims that a language can be called formalised when defining its structure we refer only to the form of its expressions. Such languages are, for example, the languages of various systems of deductive logic. One can develop numerous branches of science in them, including mathematics and theoretical physics. However, Tarski adds that one can imagine constructing languages which have exactly defined structures and are not formalised languages—in such languages the acceptance of a sentence can depend not only on its structure but also on some other non-linguistic factors. Therefore, those languages that have strictly defined structures are in a way in the middle between the formalised languages and the ordinary colloquial language.

In Tarski's opinion, a negative feature of colloquial languages is their lack of precision and closeness (i.e. the fact that they contain their own metalanguages). In particular, this characteristic leads to the possibility of semantic antimonies occurring in these languages.

We have already mentioned Tarski's tendency towards nominalism—in the interwar period he treated language as a set of sentences understood in a strictly nominalistic way as physical objects. However, his sympathies towards nominalism were much stronger. Let us quote Mostowski's opinion:

> Tarski, in oral discussions, has often indicated his sympathies with nominalism. While he never accepted the "reism" of Tadeusz Kotarbiński, he was certainly attracted to it in the early phase of his work. However, the set-theoretical methods that form the basis of his logical and mathematical studies compel him constantly to use the abstract and general notions that a nominalist seeks to avoid. In the absence of more extensive publications by Tarski on philosophical subjects, this conflict appears to have remained unresolved (1967c, p. 81).

Before discussing a certain conflict between Tarski's views and his research practice, let us see that his pronominalistic attitude is confirmed in various sources.

[153] 'jako tego rodzaju (sztucznie skonstruowane) języki, w których sens każdego wyrażenia jest jednoznacznie wyznaczony przez jego kształt' (1933, p. 16).

Firstly, it was Tarski's remark (preserved on a tape cassette) made during the symposium organised by the Association for Symbolic Logic and the American Philosophical Association, held in Chicago on 29–30 April 1965, and dedicated to philosophical implications of Gödel's incompleteness theorems. Tarski said:

> I happen to be, you know, a much more extreme anti-Platonist. [. . .] However, I represent this very [c]rude, naive kind of anti-Platonism, one thing which I would describe as materialism, or nominalism with some materialistic taint, and it is very difficult for a man to live his whole life with this philosophical attitude, especially if he is a mathematician, especially if for some reasons he has a hobby which is called set theory (Feferman and Feferman 2004, p. 52).

Fefermans' book (2004) contains more similar words concerning Tarski himself or other people's opinions about Tarski. These opinions were expressed on Tarski's 70th birthday celebrations and remembered by Chihara, Chateaubriand and the Fefermans:

> I am a nominalist. This is a very deep conviction of mine. It is so deep, indeed, that even after my third reincarnation, I will still be a nominalist. [. . .] People have asked me, "How can you, a nominalist, do work in set theory and logic, which are theories about things you do not believe in?" . . . I believe that there is a value even in fairy tales.
>
> [I am] a tortured nominalist.
>
> Elsewhere Tarski has said more specifically that he subscribed to reism or concretism (a kind of physicalistic nominalism) of his teacher Tadeusz Kotarbiński (2004, p. 52).

The recently discovered protocols of Carnap from the discussions conducted at Harvard in the academic year 1940/41 give more details about Tarski's sympathies and inclinations towards nominalism. Besides Carnap the other participants were Tarski and Quine as well as—occasionally—Russell.

In the protocol of 10 January 1941 Carnap wrote down the following remarks concerning nominalism and finitism:

> Tarski: I understand basically only languages which satisfy the following conditions:
>
> 1. Finite number of individuals;
> 2. Realistic (Kotarbinski): the individuals are physical things;
> 3. Non-platonic: there are only variables for individuals (things) not for universals (classes and so on) [154] (Mancosu 2005, p. 342).

Mancosu notices (2005, p. 343) a mistake: instead of 'realistic' it should be 'reistic,' which is confirmed by the reference to Kotarbiński.

Carnap's notes also contain the following exchange of views:

> I [Carnap]: Should we construct the language of science with or without types?
>
> He [Tarski]: Perhaps something else will emerge. One would hope and perhaps conjecture that the whole general set theory, however beautiful it is, will in the future disappear. With the higher types Platonism begins. The tendencies of Chwistek and others

[154] 'Tarski: Ich verstehe im Grunde nur eine Sprache die folgende Bedingungen erfüllt: [1] Finite Anzahl der Individuen; [2] Realistisch (Kotarbiński): Die Individuen sind physikalische Dinge; [3] Nicht-platonisch: Es kommen nur Variable für Individuen (Dinge) vor, nicht für Universalien (Klassen usw.).'

('Nominalism') of speaking only of what can be named are healthy. The problem is only how to find a good implementation (2005, p. 334).[155]

Of special interest—in the context of the problem of passing from the systems of the theory of classes—is also Carnap's summary of his conversation with Tarski on 12 February 1941:

The Warsaw logicians, especially Leśniewski and Kotarbiński saw a system like PM (but with simple type theory) as the obvious system form. This restriction influenced strongly all the disciples; including Tarski until the 'Concept of Truth' (where the finiteness of the levels is implicitly assumed and neither transfinite types nor systems without types are taken into consideration; they are discussed only in the Postscript added later). Then Tarski realized that in set theory one uses with great success a different system form. So he eventually came to see this type-free system form as more natural and simpler (2005, p. 335).[156]

Tarski's letter to Woodger, dated 21 November 1948, testifies to the importance he attached to nominalism:

The problem of constructing nominalistic logic and mathematics has intensively interested me for many-many years. Mathematics—at least the so-called classical mathematics—is at present an indispensable tool for scientific research in empirical sciences. The main problem for me is whether this tool can be interpreted nominalistically or replaced by another nominalistic tool which should be adequate for the same purposes (Mancosu 2009, p. 147).

It is worth adding that on many occasions Tarski clearly stressed his sympathies towards Kotarbiński's reism and physicalism. He also translated into English (together with David Rynin) Kotarbiński's work 'Zasadnicze myśli pansomatyzmu' [The Fundamental Ideas of Pansomatism] (1935). The translation was published in *Mind*, one of the most important English periodicals dedicated to philosophy. It was included in Tarski's *Collected Works* (1986b).[157]

The aforementioned fact that Tarski's research practice, in particular his investigations concerning set theory or the theory of models, contradicted his nominalism to a certain extent would rather suggest that he was a follower of Platonism.

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155 'Ich: Sollen wir vielleicht die Sprache der Wissenschaften mit oder ohne Typen machen? Er: Vielleicht wird sich etwas ganz Anderes entwickeln. Es wäre zu wünschen und vielleicht zu vermuten, dass die ganze allgemeine Mengenlehre, so schön sie auch ist, in der Zukunft verschwinden wird. Mit den höheren Stufen fängt der Platonismus an. Die Tendenzen von Chwistek und anderen ("Nominalismus"), nur über Bezeichenbaren zu sprechen, sind gesund. Problem nur, wie gute Durchführung zu finden.'

156 '*Die Warschauer Logiker*, besonders Leśniewski und Kotarbiński, sahen ein System wie PM (aber mit einfacher Typentheorie) ganz selbstverständlich als die Systemform an. Diese Beschränkung wirkte stark suggestiv auf alle Schüler; auf T. selbst noch bis zu "Wahrheitsbegriff" (wo weder transfinite Stufen noch stufenloses System betrachtet wird, und Endlichkeit der Stufen stillschweigend vorausgesetzt wird, erst im später hinzugefügten Anhang werden sie besprochen). Dann aber sah T., dass in der Mengenlehre mit grossem Erfolg eine ganz andere Systemform verwendet wird. So kam er schliesslich dazu, diese stufenlose Systemform als natürlicher und einfacher zu sehen.'

157 It also testifies to Kotarbiński's strong influence on Tarski.

How can this discrepancy be explained? The answer is that it resulted from the spirit and ideological canon of the Polish School of Mathematics.[158] According to them, research should not be limited by any *a priori* philosophical foundations. One should apply free—provided that they were correct—research methods, and researchers' philosophical convictions and views were private and should have no influence on their mathematical investigations. Thus Tarski could 'privately' feel as a nominalist and at the same time, he could use infinistic methods in his mathematical investigations without having any fears that they were contrary to this doctrine.

3.8 Maria Kokoszyńska

Maria Kokoszyńska dealt mainly with logic, semantics, the methodology of sciences and epistemology. Of our interest are her views on the methodology of sciences and her considerations concerning truth.

In the field of methodology Kokoszyńska shared the view that the particular domains of science were complexes of sentences, having a homogenous structure. She thought that the formal apparatus allowed the reconstruction of the logical structure of a theory. She tried to use the logic apparatus among other things to characterise the deductive and non-deductive sciences. Our attention should be first of all focused on her work 'W sprawie różnicy między naukami dedukcyjnymi i niededukcyjnymi' [On the Difference Between Deductive and Non-Deductive Sciences] (1967), in which using the tools of mathematical logic, in particular model theory, she gives an interesting interpretation of Kazimierz Twardowski's views on the discussed problem. She also explains and defines precisely his theses included in the analysed (in Sect. 3.1 of this chapter) work 'O naukach apriorycznych, czyli racjonalnych (dedukcyjnych), i naukach aposteriorycznych, czyli empirycznych (indukcyjnych)' [*A Priori*, or Rational (Deductive) Sciences and *A Posteriori*, or Empirical (Inductive) Sciences] (1923). As mentioned before, in this work Twardowski distinguished 'deductive sciences, i.e. those which refer to axioms, definitions and postulates in finally determining their theorems, and non-deductive sciences, which in case of doubt refer to observational sentences, i.e., to experience'[159]—as Kokoszyńska summarises his conceptions (1977, p. 201). Twardowski adds that these sciences also differ by the method of justifying the theorems: in the deductive sciences it is only deduction whereas in the non-deductive sciences—various methods (including induction), but never deduction.

[158] Cf. Sierpiński's remarks on the axiom of choice (the end of Sect. 2.1, Chap. 2).

[159] 'nauki dedukcyjne jako te, które przy ostatecznym rozstrzyganiu swoich twierdzeń odwołują się do aksjomatów, definicji i postulatów, od nauk niededukcyjnych, które w wypadku jakichś wątpliwości odwołują się do zdań spostrzeżeniowych, czyli doświadczenia' (1967, p. 43).

Kokoszyńska introduces certain subtle distinctions. She speaks of direct and indirect justification. The former is when 'we refer to some sentences' while the latter—'we refer to something which itself is not a sentence' (1977, p. 201). On the other hand, she speaks of absolute and relative justification. The former occurs when 'it terminates—in a finite number of steps—by referring to something which itself is not a sentence' whereas the latter 'if in the same number of steps it terminates by referring again (if not exclusively, then also) to some sentences' (1977, p. 201).[160] In the light of these considerations one can say that according to Twardowski, the deductive sciences justify their theorems only in the relative sense, and using exclusively deduction they do not utilise any method of direct justification. As Kokoszyńska writes, 'deductive sciences then do not (simply) justify anything, they do not assert anything' (1977, p. 202).[161] Therefore, they have—as Ajdukiewicz expressed—the character of only hypothetical-deductive systems. But non-deductive sciences refer to some observational sentences and make use of direct justification as well as justify at least some of their theorems in the absolute sense. Additionally, at the end of his work Twardowski states that deductive sciences 'can boast of assertions that are certain' and 'never concern facts' (1999, p. 179),[162] which seems to contradict his earlier theses.

In her work Kokoszyńska shows, using the tools of mathematical logic, in particular model theory, that essentially, there is no contradiction here; on the contrary—Twardowski's theses assume some fuller splendour.

She begins by reflecting on the concept of N. Bourbaki, stating that the mathematical world consists of structures and the particular domains of mathematics are the theories of appropriate structures. Specifically, it allows us to order the whole of mathematics by referring to the hierarchy of structures analysed in concrete theories. Moreover, what is important here is the method which the mathematical sciences use and which is the axiomatic method. Axioms are treated here as the implicit definitions of the specific terms of the given theory. In order to provide proofs one should leave out any other assumptions, particularly one should not take into consideration any hypotheses concerning the 'nature' of the objects under investigation.

However, accepting Bourbaki's attitude Kokoszyńska thinks that one should not speak separately about the object of mathematics and its method—the latter is sufficient. Since the axiomatic method defines the object of a theory:

> Every deductive science has as object not only a distinguished model of its axioms, but equally well any model of them. We can also say that a deductive science has as object—in a natural understanding of the expression 'is an object'—the class of all the models of its axioms. [. . .] this class is empty for inconsistent theories. It is always something different

[160] 'po dowolnej skończonej ilości kroków—kończy się [ono] odwołaniem do czegoś, co już zdaniem nie jest'; 'po takiejże ilości kroków kończy się [ono] odwołaniem znowu (o ile nie wyłącznie, to m.in.) do jakichś zdań' (1967, p. 44).

[161] '[n]auki dedukcyjne niczego [. . .] nie uzasadniają (po prostu), niczego nie twierdzą' (1967, p. 44).

[162] 'mogą szczycić się twierdzeniami pewnymi'; 'nigdy nie tyczą się faktów' (1923, p. 372).

> from all its members. This, of course, also occurs when this class is empty: it exists, while it has no members (1977, p. 212).[163]

A characteristic of the deductive sciences is the use of the deductive method to justify their theorems. Kokoszyńska understands this method as a procedure in which, in order to justify a sentence on the grounds of a set of sentences, one starts from the convention that the terms, distinguished in these sentences as specific for the given considerations, will be given 'only such interpretations [. . .] by which the sentences of this set become true' (1977, p. 212).[164]

Further she claims:

> The theorems of a deductive science are always analytic (true, if only the denotata of their terms required by the linguistic conventions exist). The justification of a sentence by using the deductive method in the sense spoken of is a method of justification in the absolute sense in the language of the deductive science and guarantees that these sentences are true, if only there exists a model of this science. Apart from deriving some sentences from others on the ground of logical rules (the usual understanding of deduction), which is a relative justification, in the use of the deductive method understood as above also a way of direct justification is included: by referring to something that no longer is a sentence of a language (and is no sentence at all in the sense of truth or falsity), namely by referring to decisions concerning the interpretation of specific terms of the given science (decisions expressed by the terminological conventions adopted at the beginning of this science) (1977, pp. 212–213).[165]

Reference to terminological conventions is included, according to Kokoszyńska, in the methods of direct justification. Since she thinks that the conviction that direct justification is infallible is a superstition. She writes:

> The fact that such a conviction is a superstition can be clearly seen in that reference to sensible ideas [. . .] is considered as a method of direct justification, although it may lead—and it does in many cases (illusions, hallucinations)—to falsehoods (1977, p. 214).[166]

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[163] 'Przedmiotem każdej nauki dedukcyjnej nie jest jakiś wyróżniony model jej aksjomatów, ale równie dobrze każdy ich model. Możemy także powiedzieć, że przedmiotem nauki dedukcyjnej—w pewnym naturalnym rozumieniu wyrażenia "być przedmiotem"—jest klasa wszystkich modeli jej aksjomatów. [. . .] ta klasa jest pusta, jak wiadomo, dla teorii sprzecznych. Klasa ta jest zawsze czymś różnym od wszystkich swoich elementów. Zachodzi to oczywiście także w wypadku, gdy jest pusta: ona istnieje, elementu zaś jej nie ma' (1967, p. 56).

[164] 'jedynie takie interpretacje [. . .], przy których zdania tego zbioru stają się prawdziwe' (1967, p. 55).

[165] 'Twierdzenia nauki dedukcyjnej są zawsze analityczne (prawdziwe, o ile tylko wymagane przez ustalenia językowe denotaty ich terminów istnieją). [. . .] Uzasadnianie zdania metodą dedukcyjną w wyżej wyróżnionym sensie jest pewną metodą uzasadniania w sensie absolutnym w języku danej nauki dedukcyjnej i gwarantuje temu zdaniu prawdziwość, o ile tylko istnieje model tej nauki. Obok wywodzenia według dyrektyw logicznych jednych zdań z innych (zwykłe rozumienie dedukcji), co jest uzasadnieniem relatywnym, w stosowaniu metody dedukcyjnej w obecnie omawianym rozumieniu zawiera się bowiem też pewien sposób uzasadniania bezpośredniego: przez odwoływanie się do czegoś, co już zdaniem języka nie jest (i w ogóle nie jest zdaniem w sensie prawdy lub fałszu), a mianowicie przez odwoływanie się do postanowień dotyczących interpretowania terminów specyficznych danej teorii, postanowień, wyrażających się w poczynionych u progu tej nauki umowach terminologicznych' (1967, pp. 56–57).

[166] 'Że zaś takie przekonanie jest przesądem, widać stąd, że odwoływanie się do wyobrażeń spostrzeżeniowych [. . .] jest uznawane za metodę uzasadniania bezpośredniego, mimo iż może

Recognising the reference to the understanding of words as a method of direct justification she concludes that the deductive method is a method of justification in an absolute sense and not only relative.

Having thus characterised the deductive sciences Kokoszyńska juxtaposes them with the non-deductive sciences to which she ascribes the following features:

> [. . .] 1) an object of a non-deductive science always is *one distinguished* (although not always uniquely distinguished) model of meaning-postulates of the language employed by that science, 2) proper theses of non-deductive sciences are always *synthetic* (although when justifying them we use previously adopted analytic meaning-postulates) and 3) theses of non-deductive sciences are justified in the *absolute* sense, and the method of direct justification used by these sciences consists in basing their final premises, i.e. observation judgments, on sensible ideas; as a consequence these theses are true if only the objects of those ideas exist and are such as presented by these ideas, and if the steps of the inference have led from truth to truth (1977, p. 222).[167]

Kokoszyńska also reflects on the nature of logic. She thinks that logic 'is not included in the comparison presented' (1977, p. 227).[168] Moreover, she does not accept Aristotle's conception that logic is prior to the whole knowledge constituting its tool (*organon*). In Kokoszyńska's opinion logic is prior to the remaining sciences but in a different sense. She understands logic as a set of consequences of the axioms of logic and the latter are sentences constructed of only logical constants and variables, which are true 'under all possible interpretations of the variables in the universe of the real world (of the model distinguished by non-deductive sciences), *resp.*—as far as the propositional logic is concerned—in the universe of logical values' (1977, p. 227).[169] What these sentences are like, 'depends on the regularities that have previously been repeatedly observed in experience' (*ibid.*).[170] This conception of logic has all of the above-mentioned features of the deductive sciences because:

nas prowadzić—i w licznych wypadkach prowadzi (złudzenia, halucynacje)—do fałszów' (1967, p. 58).

[167] '[. . .] 1) przedmiotem nauki niededukcyjnej jest zawsze *jeden wyróżniony* (choć nie zawsze jednoznacznie) model postulatów znaczeniowych języka, jakim się ta nauka posługuje, 2) tezy właściwe nauk niededukcyjnych są zawsze *syntetyczne* (jakkolwiek przy ich uzasadnianiu posługujemy się analitycznymi postulatami znaczeniowymi, uprzednio przyjętymi) oraz 3) tezy nauk niededukcyjnych są uzasadniane w sensie *absolutnym*, a metodą uzasadniania bezpośredniego, z jakiej nauki te korzystają, jest opieranie swych ostatecznych przesłanek, jakimi są sądy spostrzeżeniowe, na wyobrażeniach spostrzeżeniowych; w konsekwencji tezy te są prawdziwe, o ile tylko przedmioty tych wyobrażeń istnieją i są takimi, jakimi ich w wyobrażeniu doznajemy, a kroki użyte we wnioskowaniu prowadziły od prawdy do prawdy' (1967, p. 66).

[168] 'wypada poza nawias omawianego wyżej przeciwstawienia' (1967, p. 71).

[169] 'przy wszelkich możliwych interpretacjach zmiennych w uniwersum świata rzeczywistego (modelu wyróżnionego przez nauki niededukcyjne), *resp.*—o ile chodzi o logikę zdań—w uniwersum wartości logicznych' (1967, p. 71).

[170] 'zależy chyba w ostateczności od prawidłowości stwierdzanych uprzednio wielokrotnie w doświadczeniu' (*ibid.*).

1) Its object is a ‘structure’ (the class of acceptable ‘models’ of the sentences [axioms of logic]). 2) Its theses are analytic and as such, they are true if only there exist the required models for the axioms assumed (and for the relations determined by the rules). 3) The theses of logic are justified in the absolute sense, namely by the ultimate reference to the decisions of this and not other understanding of the logical constants (1977, p. 228).[171]

Whether there exists the required model for the axioms and whether the axioms are true ‘is confirmed in the course of the development of our knowledge and its application’ (1977, p. 229).[172] In Kokoszyńska’s view, in the process of the development of knowledge the most difficult problem is to determine the interpretations for logical constants. She writes:

As this cannot be delayed until the ‘invariants of the real world’ are known—such knowledge is something like the limit of the development of all sciences—from the beginning some interpretations of them *are arbitrarily chosen* (interpretations suggested by the existing experiences). Thereby it is not excluded that also these interpretations may change some day—for the sake of accurate reflecting reality by our knowledge—in the process of adjusting the synthetic and analytic parts of the empirical sciences with one another (1977, p. 228).[173]

Let us proceed to the second problem, namely to Kokoszyńska’s considerations on truth. As shown above (cf. Sect. 3.1 of this chapter) she developed Twardowski’s arguments concerning the absoluteness of the concept of truth presented in his work ‘O tzw. prawdach względnych’ [On So-Called Relative Truths] (1900). She dealt with this question in her works (1936a, 1936b, 1939–1946, 1949–1950)—the last one contains all of her arguments. Developing Twardowski’s theses Kokoszyńska uses modern semantics, particularly the semantic theory of truth.

Kokoszyńska assumes that the expression ‘*A* is relatively true’ means only: (1) the expression ‘is true’ is an incomplete predicate and (2) there are such circumstances *X* and *Y* that *A* is true with respect to *X* and its negation is true with respect to *Y*. The fact that the predicate ‘is true’ is an incomplete predicate means that the expression ‘*A* is true’ is actually an abbreviation of the expression ‘*A* is true with respect to. . . .’ Kokoszyńska also distinguishes a radical version and a moderate version of relativism but:

To the relativistic standpoint with respect to truth in its *moderate* form we may give accordingly the following formulation: the term “*true*” is an incomplete predicate, and

[171] ‘(1) Przedmiotem jej jest pewna “struktura” (klasa dopuszczalnych “modeli” zdań [aksjomatów logiki]). (2) Tezy jej są analityczne i jako takie prawdziwe, o ile tylko istnieją żądane “modele” dla założonych aksjomatów (i związków ustalonych przez dyrektywy). (3) Tezy logiki są uzasadnione w sensie absolutnym, i to przez ostateczne odwołanie się do postanowień takiego, a nie innego rozumienia stałych logicznych’ (1967, pp. 71–72).

[172] ‘potwierdza się w toku rozwoju całej naszej wiedzy i jej stosowania’ (1967, p. 72).

[173] ‘Ponieważ nie można z tym czekać na poznanie “niezmienników realnego świata”—poznanie takie to jak gdyby granica rozwoju wszelkich nauk—*stawia się* od początku na pewne ich interpretacje (sugerowane zresztą przez doświadczenia dotychczasowe). Tym samym nie wyklucza się, że w procesie wzajemnego dopasowywania do siebie części syntetycznych i analitycznych nauk empirycznych i te interpretacje będą mogły—w imię wiernego odbijania rzeczywistości przez naszą wiedzę—ulec kiedyś zmianie’ (1967, p. 72).

> there are such propositions that they are true with respect to something, while their negations are true with respect to something else. [. . .]
>
> The *radical* form of the relativism with respect to truth affirms, then, that the term "*true*" has the mentioned character, and every proposition which is true with respect to something is such, that its negation is true with respect to something else (1949–1950, p. 94).

Besides these proper versions of relativism one can also speak of its improper version, which Kokoszyńska characterises:

> For every proposition *X* there is such a proposition "y", that if y then *X* is true, and there is also such a proposition "z", that it is possible that z, and if z then *Non-X* is true (1949–1950, p. 97).

It means that *A* is a relative truth in the improper sense if and only if *A* is true with respect to *Y*, and its negation is true with respect to possible *Z*, i.e. *A* is a relative truth in the improper sense if and only when *A* is true in the real world and there is a possible world such that the negation of *A* is true in it.

The relativists often give the example of sentences containing deictic (indicating) terms as sentences that are relatively true. However, such sentences have no definite meaning and as such cannot be proper bearers of truth. Thus we cannot speak here about the relativity of truth but only about the relativity of statements. Empirical hypotheses are relative truths but in the improper sense. They can be accepted 'only if certain empirical conditions are fulfilled' (1949–1950, p. 95). Beside these hypotheses Kokoszyńska distinguishes necessary theorems that 'have to be accepted without regard to any experiences made after having established the meaning of terms' (*ibid.*). She adds:

> Theorems of mathematics and logic seem to belong to necessary theorems. All of them are *analytic* (in a wide meaning of this term). The analytic theorems together with their negations (necessary contravalid sentences) form *necessary statements*, in other words: the *a priori part* of the language (which obviously changes with the development of language and thought). Physical, biological, and similar theorems are mostly empirical theorems of our language: they have to be affirmed only if certain facts have happened (1949–1950, p. 95).

Moreover, Kokoszyńska distinguishes between the absoluteness of the concept of truth and the relativity of our knowledge, writing:

> [. . .] the *absolute character of truth seems to go together with a relative character of our knowledge*. This feature of knowledge can be specified in two ways: (1) *in the relative character of theorems with respect to the time-points* [. . .], (2) *in the relative character of our theorems with respect to persons* by whom experiences are made [. . .]. Hence, our empirical knowledge of the real world may be called not only *relative* in a narrower meaning of the term according to (1), but also *subjective*—according to (2) (1949–1950, p. 143).

Nevertheless, she emphasises strongly that '*no relativism or subjectivism of truth* (as understood in this paper) is founded by this kind of relativism' (ibid.).

In her talk delivered during the Third Polish Philosophical Congress in Cracow Kokoszyńska proposed relativising truth to the concept of meaning (cf. her paper 'W sprawie względności i bezwzględności prawdy' [On Relativity and

Absoluteness of Truth], 1936a). During the discussion after her talk Tarski said that since the concept of the language was clearer than the concept of meaning it would be better to relativise to the language. Responding to that Kokoszyńska argued:

> If one wants to speak precisely one should relativise the concept of truth to two factors: (1) the set of sounds making sentences of the language to which the concept of truth refers, (2) the way in which we translate these sounds into the language in which the concept of truth is defined. One can take the standpoint that the language considered is determined by these two circumstances; then relativisation to the concept of meaning is a natural and purposeful thing. If we assume that the set of sounds is fixed—in my talk I meant chiefly such cases—we are left only with relativisation to the way in which the sentences of the language under consideration are translated into the sentences of the language in which the considerations are conducted. Speaking of the relativisation of the concept of truth to the concept of meaning I meant exactly the latter relativisation (1936c, p. 425).[174]

Therefore, in the semantic theory of truth where we deal with the translation of the objective language under consideration into metalanguage one should assume the concept of meaning and that the relativisation to meaning is necessary.

3.9 Cracow Circle (Bocheński, Drewnowski, Salamucha)

The term 'Cracow Circle' is used to describe a group of people who tried to apply the methods of modern formal/mathematical logic to philosophical and theological problems, in particular they attempted to modernise the contemporary Thomism (the trend which was then prevailing) by the logical tools. The group consisted of: the Dominican Father Józef (Innocenty) M. Bocheński, Rev. Jan Salamucha, Jan Franciszek Drewnowski and the logician Bolesław Sobociński who collaborated with them. 26 August 1936 is regarded as the foundation date of the Cracow Circle.[175] On that day a special meeting was held during the Third Philosophical Congress in Cracow. The meeting gathered 32 people, including professors of philosophy of the theological academies and major theological seminaries as well as the future members of the Circle. It was presided over by the outstanding

[174] 'Gdyby się chciało mówić dokładnie, należałoby pojęcie prawdy relatywizować do dwóch czynników: (1) do zespołu brzmień składających się na zdania języka, do którego pojęcie prawdy się odnosi, (2) do sposobu, w jaki te brzmienia przekładamy na język, w którym pojęcie prawdy się definiuje. Można stanąć na stanowisku, że język rozważany jest wyznaczony przez obie wspomniane okoliczności, w tym przypadku relatywizacja do pojęcia znaczenia jest rzeczą naturalną i celową. O ile jednak założymy, że zespół brzmień jest ustalony—a takie głównie wypadki miałam w referacie na uwadze—pozostaje tylko relatywizacja do sposobu, w jaki się przekłada zdania języka rozważanego na zdania języka, w którym rozważania się przeprowadza. Mówiąc o relatywizacji pojęcia prawdy do pojęcia znaczenia miałam na myśli tę właśnie drugą relatywizację.'

[175] Here we cannot give more details about the history of the Cracow Circle—more information on this theme can be found, for example in Wolak (1993) and (1996), cf. Bocheński (1989a) and (1994) as well as Woleński (2003).

philosopher and specialist in Medieval studies Rev. Konstanty Michalski. Another participant was Jan Łukasiewicz, one of the key representatives of the Lvov-Warsaw School—specifically of the Warsaw School of Logic—who himself had dealt with philosophy and formulated a programme of a radical reform of this domain, suggesting the use of the methods of modern logic. Łukasiewicz. formulated this programme in the article 'O metodę w filozofii' [On Method in Philosophy] (1927).

During the meeting Łukasiewicz, Bocheński, Salamucha and Drewnowski presented their views and then a discussion was held. The proceedings were published in 1937 in volume 15 of *Studia Gnesnensia* under the title *Myśl katolicka wobec logiki współczesnej* [Catholic Thought in Relation to Modern Logic].

In fact, the contacts and collaboration between those who were members of the Cracow Circle began earlier—cf. Bocheński (1989a)—and the above mentioned meeting was rather a public manifestation. According to Bocheński the Circle existed for 7 years—from the beginning of his friendship with Salamucha till the outbreak of World War II.

The four people who composed the nucleus of the Circle shared interests in mathematical logic as well as philosophical and theological issues. Bocheński was a doctor of philosophy and theology; he was a professor at the Angelicum in Rome. Salamucha studied philosophy, mathematics and mathematical logic at the University of Warsaw, received his PhD in philosophy at the Jagiellonian University, studied at the Gregorian University in Rome, and when the Circle was created he was a professor of philosophy at the Warsaw Major Seminary. Drewnowski, who was T. Kotarbiński's disciple, was the editor and publisher of *Rocznik Handlu i Przemysłu* [Yearly Reports on Trade and Industry] in Warsaw. Sobociński, a philosopher and logician, was an assistant at the University of Warsaw, and he dealt mainly with formal logic. As opposed to the first three men he did not publish any works on philosophy but he participated in all of the meetings of the Circle and in a way was an expert on logical problems.

The members of the Cracow Circle were fascinated with modern formal logic but were dissatisfied with the level and way of cultivating philosophical and theological reflections of their times. Consequently, they proposed a complete axiomatisation and formalization of the Catholic doctrine, especially Thomism. It should be added that they all respected Thomism. Salamucha and Bocheński regarded themselves as Thomists. Nevertheless, they wanted to change it and transform it into a normal scientific theory. They thought that Catholic thinkers were not faithful to their sources, i.e. scholasticism. Rejecting modern logic they did not follow the spirit of St Thomas Aquinas who had made use of then existing logical apparatus in his philosophical and theological analyses. The Circle postulated a reform of philosophy, first of all its methods and not its content. They did not intend to give up traditionalistic philosophy but wanted to make it precise and develop it in a scientific way. Moreover, the representatives of the Circle thought that the new research methods, using the instruments of modern logic, allowed them to discover numerous valuable elements in the old philosophical and theological texts. They were highly critical about the philosophical systems that had originated between the sixteenth and the nineteenth centuries, including Neo-Scholasticism. Their criticism focused on the methodology of those systems. In particular, they criticised Hegel's philosophy 'not because it was idealistic, but because it was confused, badly stated and insufficiently justified' (Bocheński 1989a, p. 12). Additionally, the Circle rejected Neopositivism and all minimalistic philosophies.

As mentioned before, the members of the Cracow Circle were predominantly concerned with methodological problems. They aimed at reforming the traditional way of thinking and writing, which characterised Catholic philosophers and theologians. In addition, they were convinced that modern mathematical logic could be used in philosophical and theological investigations. As Bocheński writes, they postulated that

> (1) the language of philosophers and theologians should exhibit the same standard of clarity and precision as the language of science; (2) in their scholarly practice they should replace scholastic concepts by new notions now in use by logicians, semioticians, and methodologists; (3) they should not shun occasional use of symbolic language. To put it briefly the Circle wanted to persuade catholic thinkers and writers to adopt the "style" of philosophizing cultivated by the Polish logical school (1989a, pp. 11–12).

Łukasiewicz's influence on the Circle and its programme was obvious. Bocheński writes:

> This is not surprising as all the members of the Circle, with the exception of myself, had been his pupils. His were the methodological postulates, the criticism of modern philosophy, and the doctrine of the neutrality of logic, stated explicitly for the first time at a meeting of the Circle in 1934. And again, the inquiries by some members of the Circle into the ancient and medieval logic were in fact the continuation of the pioneering work done by Łukasiewicz (1989a, p. 12).

It should be added that the Circle had to face aversion and misunderstanding shown by the followers of the official theology. Its method, using mathematical logic, aroused resistance and opposition. The philosophical interpretations formulated by means of this method were accused of anti-metaphysicism, atheism, conventionalism, relativism, pragmatism, positivism and other opposing views to Christian doctrine. The use of logical methods was connected—completely unjustifiably—with the attitude towards religion of such logicians as B. Russell, T. Kotarbiński or the whole Vienna Circle. Refuting these accusations, the representatives of the Cracow Circle firmly defended the neutrality of mathematical logic with respect to philosophy. Thus they shared the views of the Lvov-Warsaw School, opposing the Vienna Circle.

The originality and significance of the conceptions formulated by the Cracow Circle should be stressed. Later similar attempts were made by individual scientists, for example Bendiek (1949) and (1956) or Clark (1952). However, they worked on their own account and did not form any group; consequently, their achievements are not as remarkable as those of the Circle. Bocheński thinks that the efforts of the Circle aiming at changing the Catholic thinkers' attitude towards modern formal logic were completely unsuccessful (cf. Bocheński 1989a, p. 14). One of the reasons was the tragic death of Salamucha—the soul of the Circle—during World War II. However, the reasons were more complex. Bocheński writes:

> The failure of the programme proposed by the Cracow Circle is not due to some peculiar Polish circumstances. It seems to be the result of the wide-spread resistance on the part of otherwise rationally thinking philosophers and theologians to recognize the significance of mathematical logic and analytic philosophy in any intellectual endeavour.
>
> The case of the Cracow Circle is particularly sad. For Poland is one of among not so many countries that has had a flourishing school of logic and an efficient team of catholic scholars, who claimed to be rational. One would have expected that in such a country a new catholic philosophy and, in the first place, a new catholic theology should arise. Alas, this has not been the case (1989a, pp. 15–16).

Nonetheless, despite Bocheński's opinions the efforts of the Cracow Circle were continued—but not as extensively as one may expect. Besides the aforementioned works of Bendiek and Clark it is worth recalling the analyses of the five ways of Thomas Aquinas with the help of the instruments of modern logic undertaken by F. Rivetti Barbò, E. Nieznański or K. Policki.[176] These authors used the strong tools of logic like the Kuratowski-Zorn lemma (Policki) or the theory of lattices (Nieznański).

Before proceeding to analysing the philosophical views of the Cracow Circle on logic and mathematics, we should mention their main achievements as far as the implementation of the tools of mathematical logic to solve philosophical and theological problems is concerned. These achievements include:

1. Logical analysis of the proof '*ex motu*' for the existence of God, presented by St Thomas Aquinas in his *Summa contra gentiles*, undertaken by Salamucha (1934),
2. Formalisation and logical analysis of the proof for the immortality of the soul given by St Thomas Aquinas, formulated by Bocheński (1938),
3. Analysis of the scholastic concept of analogy—these investigations were initiated by Drewnowski (1934) and Salamucha (1937a), then developed by Bocheński (1948),
4. A certain number of works concerning the history of logic, particularly the history of Medieval logic—these investigations were characterised by looking at the old logic through the prism of modern logic[177]—the works of Salamucha (1935) and (1937b) or Bocheński's monograph (1956a), which was to some extent the culmination of this research trend,
5. Numerous works popularising Christian thought and the new style of its cultivation.

Our reflection on the philosophical views on logic and mathematics formulated by the scholars under consideration should begin with the analysis of Salamucha's views. Using the methods of logic to analyse the arguments of St Thomas Aquinas, Salamucha utilized the classical two-valued propositional calculus as well as the concepts of membership, relation and set. He referred to *Principia Mathematica* by Whitehead and Russell; he also used the symbols of their work. So he neither made

[176] The analysis of these attempts can be found in Nieznański's work (1987).

[177] This method was also used by Łukasiewicz—cf. Łukasiewicz (1951).

use of semantic concepts nor the concept of truth, nor referred to the fundamental work of Tarski (1933). The aforementioned instruments were sufficient for him.

Formulating his conception of logic he cut himself off from nominalism, preserving neutrality towards the philosophical problems related to his idea. In footnote 4 to his work of 1934, he wrote:

> Although this way I am adopting much from mathematical logicians, it does not mean at all that I sympathize with their nominalistic point of view in logic and materialistic or positivistic tendencies in philosophy. I think that the same way as within traditional logic grounds different philosophical systems could occur equally in agreement or disagreement, it happens similarly within mathematical logic grounds, only in the second case more responsibility is required (2003).[178]

On the one hand, he understood logic—according to Koj (1995)—as an objective science the theses of which were formulated in an objective language and not in the metalanguage. On the other hand, he treated logic as a formal science and as such, it could not be placed on any floor of abstraction. Following Aristotle and Thomas, he saw logic as the science on operating concepts concerning reality and not as a science on reality alone. Therefore, logic is the science *de entibus secundae intentionis*. However, this clearly opposes the objective concept of logic. Salamucha was aware of this difficulty but did not develop this issue.

These problems appeared because of the question concerning the applicability of mathematical logic to metaphysical issues. According to the scholastic tradition mathematical logic is placed on the second level of abstraction whereas philosophy and in particular, metaphysics, on the third level. Salamucha did not reject this Medieval classification but sought a solution in the observation that Medieval mathematics and logic differed from modern mathematics and logic. In his paper 'O możliwości ścisłego formalizowania dziedziny pojęć analogicznych' [On Possibilities of a Strict Formalization of the Domain of Analogical Notions] (1937a), he wrote that Medieval mathematics analysed the quantitative characteristics of objects whereas modern mathematics broke with this approach and 'for the majority of modern mathematicians mathematics is simply a deductive theory, in which from some axioms and definitions derivative theorems are derived with the help of logical theses, mathematics can contain no empirical elements'[179] (2003, p. 79). Thanks to that mathematics becomes similar to logic and along with the

[178] 'Chociaż w ten sposób zapożyczam wiele od logików matematycznych, nie znaczy to wcale, że solidaryzuję się z ich nominalistycznym nastawieniem w logice i z materialistycznymi czy pozytywistycznymi tendencjami w filozofii. Myślę, że tak samo jak na gruncie logiki tradycyjnej mogły występować równie zgodnie, czy równie niezgodnie, różne systemy filozoficzne, podobnie sprawa się przedstawia na gruncie logiki matematycznej, tyle, że tu obowiązuje większa odpowiedzialność' (1934).

[179] 'dla większości współczesnych matematyków matematyka jest po prostu teorią dedukcyjną, w której z pewnych aksjomatów i definicji wyprowadza się przy pomocy tez logicznych pewne twierdzenia pochodne—żadnych elementów doświadczalnych matematyka zawierać nie może' (1937a, p. 132).

latter can—as noticed above—be treated as the science dealing with objects of second intention (cf. 1937a, p. 128). Salamucha adds:

> In this way, mathematics has got closer to logic to such an extent that the boundaries between what has till recently been two branches of sciences, today slowly disappears and mathematics becomes simply a part of logic, only higher and deductively later than those parts of the same science which are commonly regarded as logic (2003, p. 79).[180]

He summarises his considerations:

> Thus, it appears that the fears that the application of logistic to metaphysics constitutes a violation of the differences between the traditional degrees of abstraction, are a result of some misunderstandings. Too great an emphasis has been laid upon the origin of logistic and modern mathematics has been confused with medieval mathematics (2003, p. 83).[181]

In Salamucha's opinion, logic is a theory of deductive argumentation. Unfortunately, he did not develop this idea. Therefore, it is not clear—as Koj writes (1995, p. 20)—'whether logic should be treated as a theory of consequences or whether only as metasystemic theses saying which objective theses should be accepted.'[182] However, logic enables us to control reasoning. Reasoning as a mental activity is not intersubjectively verifiable, but through ordering expressions to particular elements of reasoning and through the analysis of the operations conducted on these expressions we can check the conformity of inference with logical rules. Salamucha spoke here about methodological nominalism. It should be noted that it is something different than, for example, Chwistek's nominalism (cf. Chap. 2), which treats reasoning just as an operation on expressions (devoid of meaning). In Rev. Salamucha's opinion logic does not exclude meanings but only temporarily—exactly for methodological reasons—abstracts from them while analysing the arguments. Yet, Salamucha stressed that such a conception of logic did not force nominalism in philosophical theories in which it is utilised.

One of the consequences of such a conception of logic is the thesis that logic is not creative but only consists in checking the conducted activities (for instance, reasonings); it allows checking and ordering deduction. At the same time, it is to some extent a universal science, i.e. its theses can be used in all disciplines. Salamucha wrote that 'the normative consequences of logic embrace all fields of science and even ordinary life if we want it to be at least a little logical' (1936, p. 620).[183]

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[180] 'W ten sposób matematyka zbliżyła się do logiki do tego stopnia, że granice między tymi dwiema do niedawna gałęziami nauk dziś powoli się zacierają i matematyka staje się po prostu częścią logiki, wyższą tylko i dedukcyjnie późniejszą od tych części tej samej nauki, które powszechnie za logikę są uważane' (1937a, p. 132).

[181] 'Okazuje się, że obawy, jakoby zastosowanie logistyki do metafizyki było pogwałceniem różnic między tradycyjnymi stopniami abstrakcji, są wynikiem pewnych nieporozumień; kładzie się zbyt wielki nacisk na pochodzenie logistyki i miesza się matematykę współczesną z matematyką średniowieczną' (1937a, p. 137).

[182] 'czy logikę [należy] traktować jako teorię konsekwencji, czy tylko jako metasystemowe tezy mówiące, jakie tezy przedmiotowe należy przyjąć.'

[183] 'normatywne konsekwencje logiki obejmują wszystkie dziedziny naukowe i nawet życie potoczne, jeżeli chcemy, żeby ono było choć trochę logiczne.'

Salamucha did not claim that the formal logic of his times was a sufficient tool allowing the analysis and precise reconstruction of the whole of scholastic philosophy. When asked whether logic was such a sufficient tool he said that he did not know that. Referring to *Principia Mathematica*, he claimed that it was sufficient to construct the whole of mathematics. However, he did not exclude the fact that in the future it would be necessary to enlarge logic so that we might use it to make adequate analyses of philosophical problems.[184] Salamucha realised that his investigations and those conducted by the Cracow Circle were something new and belonged to the domain which had not been developed before. Concluding his paper 'O możliwości ścisłego formalizowania . . .' he wrote:

> The arguments of this paper resemble forcing one's way through a jungle, where man rarely enters; logisticians who are not interested in scholasticism do not enter there—scholastics who are not interested in logistic do not enter there (2003, p. 95).[185]

In Salamucha's opinion, one of the problems that should be solved and developed was the issues related to analogy. The adversaries of the Cracow Circle raised the reservation—because of the use of formal logic in metaphysics—that the latter used analogous concepts whereas logic aimed at providing precise concepts and making them unambiguous. Rev. Salamucha did not have a solution for that but he saw that the concept of analogy, which scholastics used, was vague and pointed to some ideas of Drewnowski included in his work 'Zarys programu filozoficznego' [Outline of a Philosophical Programme] (1934). Consequently, he formulated the following interesting opinion:

> It seems, however, that in metaphysics an adequately interpreted metalogic is going to be more useful than modern formal logic itself (2003, p. 94).[186]

As for the philosophical problems related to mathematics, we should also consider Salamucha's opinion that the appearance of non-Euclidean geometries and the creation of relativity theory allowed us to break down—as he wrote—the tyranny of time and space. He argued that both concepts were non-empirical and because of that we could not empirically confirm the influence of time and space on physical phenomena. Thanks to these new theories, the concept of space became 'empirically reversed' and 'space is only a conceptual construction and this construction can be undoubtedly and consistently developed in many different ways'

[184] This need was also presented clearly by Bocheński when he tried to formulate certain aspects of the problem of universals using the terms of modern logic—cf. Bocheński (1956b). He claimed that logical-mathematical investigations concerning certain questions connected with the problem of universals might require stronger logical and semantic tools that those that were available at that time.

[185] 'Wywody tego artykułu są trochę takie, jak przedzieranie się przez gąszcze, gdzie rzadko wdziera się człowiek; nie wchodzą tam logistycy, których scholastyka nie interesuje,—nie wchodzą tam scholastycy, którzy nie zajmują się logistyką' (1937a, p. 152).

[186] 'Zdaje się jednak, że w metafizyce bardziej przydatna okaże się metalogika, odpowiednio tylko interpretowana, aniżeli sama współczesna logika formalna' (1937a, p. 151).

(1946).[187] It is not clear what Salamucha meant by that. Since if geometry is to be understood as a formal science, experience does not play any role in recognising its theorems as true or rejecting them as false. Both types of geometry—Euclidean and non-Euclidean—have the same epistemological status in this conception. However, if this or that geometry is used to construct physical models, experience will play a fundamental role here and will determine the adequacy of the description provided by the given model.

Finally, our reflection on Salamucha's views should include his praise of Roman Ingarden's criticism of the philosophy of the Vienna Circle and his answer to the question of using formal logic in phenomenology. Salamucha agrees with Ingarden to some extent, writing:

> If one claims, together with Prof. Ingarden, that all and only those issues belong to philosophy which concern either (a) "pure possibilities or necessary relations between possibilities" or (b) "the real existence of all possible domains of being" and "the real essence of both entire domains of being and their particular elements", where the main stress is laid upon the subject (a), then one will have to accept—at most with some small reservations—that the methods of particular sciences, and hence also the deductive method, will have no application to philosophy (2003, p. 84).[188]

However, this would lead—according to Salamucha—to a radical reduction of philosophical problems. Yet, if one wants to cultivate Thomistic philosophy and theology, the utilisation of logistic tools is fully justified.

Let us proceed to the views concerning logic of another member of the Cracow Circle Jan F. Drewnowski. He formulated a more refined philosophical conception than the other members did—cf. his 'Zarys programu filozoficznego' [Outline of a Philosophical Programme] (1934), which became a kind of manifesto of the Cracow Circle although the other members of the Circle referred to it rather loosely. Drewnowski—as opposed to Salamucha or Bocheński—did not follow Thomism but chose his own way. In addition, he was an expert in natural sciences. His philosophical programme was based on the interdependence of various fields of science, especially logic, natural sciences, mathematics and theology.

Drewnowski's aim was to propose a new philosophical language that could be used to express the views of many different philosophers, in particular the theses of modern scientific philosophical theories and the theses of classical philosophy, including Thomism.

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[187] 'doświadczalnie wywracane'; 'przestrzeń jest tylko konstrukcją pojęciową i można tę konstrukcję konsekwentnie i bezsprzecznie na różne sposoby rozbudowywać.'

[188] 'Jeżeli się przyjmie, razem z prof. Ingardenem, że do filozofii należą te wszystkie zagadnienia i tylko te, które dotyczą: (a) czystych możliwości lub koniecznych związków między możliwościami lub (b) faktycznego istnienia wszelkich możliwych dziedzin bytu i faktycznej istoty zarówno całych dziedzin bytowych jak i ich poszczególnych elementów, przy tym główny nacisk położy się na tematach (a), to—co najwyżej z pewnymi małymi zastrzeżeniami—trzeba będzie uznać, że metody nauk szczegółowych, a więc i metoda dedukcyjna, nie znajdą w filozofii zastosowania' (1937a, p. 139).

One of the important components of Drewnowski's programme was his theory of signs. In his opinion, signs play a substitutive role, allowing us to get to know the real world by going beyond direct sensations and by creating systems. However, we should expect to face here certain threats, which Drewnowski specified in 'Zarys'[189]:

> Falling into a habit of constant intercourse with signs instead of reality itself, meaning—so to say—an intentional attitude towards reality, causes in the long run that the sense of this intentionality is blurred (1996, p. 58).[190]

On the one hand, identifying signs with reality can reduce reality to what the signs define and on the other hand, can recognise what the signs give as some new domain of reality.[191]

At first, signs substitute the reality under consideration and then help in theoretical considerations, which leads to the so-called pile-up of signs, i.e. groups of signs are replaced by other signs. Not paying attention to this problem may lead to a misunderstanding. Additionally, one should distinguish between signs and the instructions describing how to use these signs.

Drewnowski distinguished three kinds of theories: scientific, mathematical and theological. All of them are systems of signs. Drewnowski formulated rules of using signs for each kind of these theories and reflected on the relationships between the theories.

Having presented the general remarks we can proceed to discussing Drewnowski's views on mathematics and logic as well as the applicability of logic to other sciences. Let us begin with his remarks on axioms and definitions. He claims that:

> Axioms are either expressions of certain presumptions of the so-called laws that are binding in a given domain or they are only expressions of certain agreements accepted within a given notation. In both cases they do not express anything absolute: in the first case—it is more correct to formulate them as suitable conditions and put them in an abbreviated way in the antecedents of the theorems of a theory[192]; in the other—they belong to regulatory instructions, and it is more correct to formulate them as appropriate directives (1996, p. 67).[193]

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[189] All of the quotations come from 'Zarys programu filozoficznego,' included in Drewnowski's collection of selected works *Filozofia i precyzja* [Philosophy and Precision] (1996).

[190] 'Przyzwyczajenie do ciągłego obcowania ze znakami zamiast z samą rzeczywistością, czyli taki—że tak powiem—intencjonalny stosunek do rzeczywistości, sprawia na dalszą metę zatarcie się poczucia tej intencjonalności.'

[191] Twardowski also warned against this kind of errors (cf. his 'Symbolomania i pragmatofobia' [Symbolmania and pragmatophobia], 1927). In turn, Łukasiewicz recommended a constant contact with reality while using developed philosophical systems.

[192] We are dealing here with the theorem of deduction—remark is mine.

[193] 'Aksjomaty są wyrazem bądź pewnych przypuszczeń co do obowiązujących w danej dziedzinie tzw. praw, bądź też tylko są wyrazem pewnych umów przyjętych w obrębie danego znakowania. I w jednym, i w drugim wypadku nie wyrażają niczego bezwzględnego: w pierwszym—poprawniej jest sformułować je jako odpowiednie warunki i w skrócony sposób

Drewnowski views definitions in a similar way.

In Drewnowski's approach mathematical theories are 'the same sign mechanisms as other theories of natural sciences' (1996, p. 71).[194] He describes them more precisely in 'Zarys':

> Their characteristics are that they are tools to analyse scientific theories themselves and all other systems of signs that look like scientific theories. They deal only with the properties of the construction of the systems of signs occurring in theories, namely the dependence of various structural types of complex signs on the ways of using them, in accordance with the regulatory instructions of a given theory. [...] Therefore, the only type of operations on mathematical theories is the operations that mark the deduction of propositions and similar inter-propositional relations (1996, pp. 71–72).[195]

What is the relation of the commonly understood mathematics towards the mathematical theories thus characterised? Drewnowski claims that some parts of mathematics are scientific theories, in particular the arithmetic of natural numbers based on the primary concepts of quantity and sign. Natural theories include, in Drewnowski's opinion, 'all geometries if they concern some extensive properties and do not move to generalisations, dealing with any relations of which a special case is a given relation occurring in some empirical extension' (1996, p. 73).[196] The remaining part of 'contemporary mathematics can probably be comprised in the so-called theory of relations, i.e. it will depend on what I call here mathematical theories' (1996, p. 73).[197] In addition, for a mathematical theory it does not matter what the signs signify, and consequently, 'the propositions of mathematics are devoid of any definite meaning' (*ibid.*).[198] The identification of mathematics with mathematical theories leads to the thesis that 'the creations with which mathematics deals are any human creations' (*ibid.*).[199] The problem of existence can be reduced to the existence of signs which a given theory uses—as opposed to the scientific theories 'where the indispensible condition of correctness, the verifiability of

wymieniać je w poprzednikach twierdzeń teorii; w drugim wypadku—należą do instrukcji wykonawczej, i poprawniej jest sformułować je jako odpowiednie dyrektywy.'

[194] 'są takimi samymi mechanizmami znakowymi, jak inne teorie przyrodnicze.'

[195] 'Charakterystyczną cechą ich jest to, że są narzędziami do badania samych teorii przyrodniczych i wszelkich innych układów znaków, wyglądających jak teorie przyrodnicze. Zajmują się one wyłącznie właściwościami budowy układów znaków występujących w teoriach, mianowicie tym, jak uzależnione są różne typy strukturalne znaków złożonych od sposobów posługiwania się nimi, zgodnie z instrukcjami wykonawczymi danej teorii. [...] Jedynym więc typem operacji na gruncie teorii matematycznych są te, które znaczą wywiedlność zdań i pokrewne zależności międzyzdaniowe.'

[196] 'wszelkie geometrie o tyle, o ile zajmują się jakimiś własnościami rozciągłymi, a nie przechodzą do uogólnień zajmujących się dowolnymi stosunkami, których szczególnym przypadkiem bywa dany stosunek występujący w jakiejś rozciągłości doświadczalnej.'

[197] 'współczesnej matematyki da się prawdopodobnie objąć tzw. teorią stosunków, czyli należeć będzie do tego, co nazywam tu teoriami matematycznymi.'

[198] 'zdania matematyki są pozbawione określonego znaczenia.'

[199] 'twory, którymi zajmuje się matematyka, są dowolnymi wytworami ludzkimi.'

arguments, will always be to indicate the way of making available to the analysis what the theory is about' (1996, p. 74).[200]

According to Drewnowski, such mathematical theories include all the generalisations of philosophy and the whole part of metaphysics dealing with general laws. However, he regards the incompetent mathematisation of various domains as wrong. At this point, it results from the schematisation of mathematical domains 'which do not know the dependencies that modern mathematics investigates' or attempts 'to transfer only mathematical symbols to various considerations, e.g. historical-philosophical ones, by those who do not know mathematics' (1996, p. 75).[201] The starting point of the correct mathematisation of a theory must be suitable scientific theories based on empirical data—Drewnowski includes here 'colour or tactile qualities serving as the starting point to construct the notions of physics, such as the sensations of pain, fear, adoration, the sense of ownership, of rightness, etc., which can serve as the starting points of many different scientific theories' (1996, p. 76).[202] Such theories can then be mathematised. Drewnowski describes this process as follows:

> It will consist in that: as the given scientific theory is being developed, its dependencies are getting more and more complicated; it will be stated that such dependencies are special relations, worked on in the mathematical theories. Then the whole part of a proper mathematical theory can be used in the given scientific theory by substituting the signs of the dependences occurring in the scientific theory, which are special relations, analysed in the mathematical theory, in the correct theorems of the mathematical theory. And reversely—various new dependencies in the given scientific theory can incline us to generalise them and thus provide new problems to the mathematical theories (1996, p. 76).[203]

Drewnowski regards the features of the mathematical theories as the advantages and benefits of this mathematisation, writing:

> The value of this mathematisation of knowledge will occur even more clearly when on the one hand, it is considered that the mathematical theories owe their efficiency to their higher

[200] 'gdzie zawsze niezbędnym warunkiem poprawności, sprawdzalności wywodów będzie wskazanie sposobu udostępniania badaniu tego, o czym mowa w teorii.'

[201] 'którym obce są te zależności, jakimi zajmuje się współczesna matematyka'; 'przenoszenia samych tylko symboli matematycznych do różnych rozważań, np. historiozoficznych, przez osoby nie znające matematyki.'

[202] 'jakości barwne lub dotykowe, służące za punkt wyjścia do budowy pojęć fizyki, jak doznania bólu, strachu, uwielbienia, poczucia własności, słuszności itp., mogące służyć za punkty wyjścia szeregu innych teorii przyrodniczych.'

[203] 'Będzie to polegać na tym, że w miarę rozwijania się danej teorii przyrodniczej, komplikacji występujących w niej zależności, stwierdzać się będzie, iż pewne takie zależności są szczególnymi przypadkami stosunków, opracowywanych w teoriach matematycznych. Wówczas cała ta część odpowiedniej teorii matematycznej może być zastosowana do danej teorii przyrodniczej drogą podstawienia w odpowiednich twierdzeniach teorii matematycznej znaków tych zależności teorii przyrodniczej, które są szczególnymi przypadkami stosunków badanych w teorii matematycznej. Odwrotnie też—różne nowe zależności w danej teorii przyrodniczej mogą skłaniać do uogólniania ich i dostarczać w ten sposób nowych zagadnień teoriom matematycznym.'

> degree of generality: analysing the dependencies, without considering their meanings, allows making many attempts and modifications, which would not be easy within the framework of some scientific theory in which the meanings of signs, many a time loaded with tradition, habits, hinder the movements (1996, p. 76).[204]

On the other hand, considering the possible applications of the mathematical theories allows us to choose from 'a surplus of possible combinations' those which are more desired.

Drewnowski also considered the problem of applying symbolic logic, especially to philosophy. He wrote a special paper on this question in 1965, referring to D. Hilbert and W. Ackermann's *Grundzüge der theoretischen Logik* (1928), which—as he notices—characterises the method of this application of logic. He describes the method in the following words[205]:

> The method establishes constant symbols, expressing the specific notions of a given domain, and describes the types of objects marked by the arguments of these new functional symbols. With the help of these new symbols and the symbols of functional calculus,[206] the symbolic formulations of the premises from the given domain are provided. The formulated premises are added to the axioms of functional calculus as new axioms. From this, using the rules of inference of functional calculus, we receive theorems, being the symbolic formulations of what we want to prove in the given domain (1996, p. 199).[207]

At the same time, he notices that such an application of the predicate calculus is not an interpretation of the symbols of the language of this calculus because 'all the time these symbols are used in the same general logical meaning as in the classical logical calculus' (1996, pp. 199–200).[208] The symbolic formulation of the assumed properties of the analysed objects in the form of axioms can feature certain general dependencies in a given domain, and the axioms 'do not have to use up semantically the content of the notions and all the dependencies of this domain' (1996, p. 200).[209] Such an application of the logical tools to define precisely the given

[204] 'Wartość tak pojętego matematyzowania wiedzy wystąpi jeszcze wyraźniej, gdy się zważy, że z jednej strony teorie matematyczne zawdzięczają swoją sprawność większej swej ogólności: zajmowanie się zależnościami, bez oglądania się na ich znaczenie, pozwala na dokonywanie wielu prób i przeróbek, które nie byłyby łatwe w obrębie jakiejś teorii przyrodniczej, gdzie znaczenia znaków, obarczone nieraz tradycją, nawykami, utrudniają swobodę ruchów.'

[205] Like in the case of 'Zarys programu filozoficznego' the page numbering is from Drewnowski's selected works *Filozofia i precyzja* (1996).

[206] The old name of the predicate calculus—remark is mine.

[207] 'Metoda ta polega na tym, że ustala się nowe symbole stałe, wyrażające swoiste pojęcia danej dziedziny, i opisuje się rodzaje przedmiotów oznaczonych przez argumenty tych nowych symboli funkcyjnych. Za pomocą tych nowych symboli oraz symboli rachunku funkcyjnego podaje się symboliczne sformułowania przesłanek z danej dziedziny. Tak sformułowane przesłanki dołącza się do aksjomatów rachunku funkcyjnego jako nowe aksjomaty. Stąd zaś, stosując reguły wnioskowania rachunku funkcyjnego, otrzymuje się twierdzenia, będące symbolicznymi sformułowaniami tego, czego się chce dowieść w danej dziedzinie.'

[208] 'symbole te cały czas są użyte w tym samym ogólnologicznym znaczeniu, jakie mają w klasycznym rachunku logicznym.'

[209] 'nie muszą wyczerpywać znaczeniowo treści pojęć i wszelkich zależności tej dziedziny.'

domain of knowledge does not violate the richness of its content. The application of these tools is possible to the extent that 'the rational cognition of the given domain of reality' (1996, p. 200)[210] is possible. Moreover, Drewnowski clearly opposes the view that symbolic logic cannot be used outside of mathematics, in particular in philosophy. He criticises the arguments formulated by the followers of this standpoint, especially the opinions presented by the adherents of the so-called existential Thomism who claim that 'metaphysics cultivated in this spirit has separate methods of reasoning, and symbolic logic cannot be used here' (1996, pp. 200–201).[211] This problem was already considered by Ajdukiewicz in 'O stosowalności czystej logiki do zagadnień filozoficznych' [On Applicability of Pure Logic to Philosophical Questions] (1934). He asked whether modern logic, which was extensional, could be used to solve philosophical problems formulated in the intentional colloquial language. In the aforementioned paper (1965), Drewnowski analysed three meanings of extensionality and stated that equivalential extensionality of classical logical calculus was not an obstacle to using this calculus in philosophy. He also explained how logic is used to solve philosophical and theological problems in the works of the Cracow Circle:

> All of our attempts neither interpreted logical symbols nor translated metaphysics into the language of symbolic logic. The method of applying symbolic logic, which we have utilised, was just [...] the application of the very classical logical calculus, to which new constant symbols are added (1996, pp. 203–204).[212]

Let us proceed to the last member of the Cracow Circle—Fr Józef (Innocenty) Maria Bocheński. At this point, a certain problem is the evolution of his philosophical views. Since Bocheński was a follower of Kant, and then of neo-Thomism. He attempted to modernise the latter by using the tools of mathematical logic. Finally, he departed from the problem of being and moved towards analytic philosophy. Since our book concerns the pre-war period we are not going to analyse his post-war views but focus on his activities in the Cracow Circle (however, we will sometimes refer to his later activities).

According to the Cracow Circle, if Thomism wants to be a rational philosophy—which it has been since the very beginning—it must know and use modern formal logic. Since this logic gives precision, which Bocheński understood in the following way:

> 'Precise' is called our way of speaking, which observes the following rules: As far as words are concerned, they must be unequivocal signs of simple things, features, experiences, etc.; they are to be clearly defined in relation to these simple signs, in accordance with precisely

[210] 'rozumne poznanie danej dziedziny rzeczywistości.'

[211] 'metafizyka uprawiana w tym duchu ma odrębne metody rozumowania i logika symboliczna nie daje się tu stosować.'

[212] 'Otóż wszystkie te nasze próby nie były ani interpretowaniem symboli logicznych, ani przekładaniem metafizyki na język logiki symbolicznej. Metoda stosowania logiki symbolicznej, jaką się posługiwaliśmy, była właśnie [...] stosowaniem samego tylko klasycznego rachunku logicznego, do którego dodaje się nowe symbole stałe.'

> stated rules. Furthermore, these words should be always used in such a way that each one of them constitutes a part of a proposition, i.e. expression that is true or false. Where propositions are concerned, they cannot be accepted until we know exactly what they mean and why we assent to them. Sometimes we accept them as evident, sometimes on the basis of faith or proof—in the latter case it should be conducted on the basis of clearly formulated and efficient logical directives (1937, pp. 28–29).[213]

Additionally, precise speaking and thinking should be characterised by the use of formal logic and exclusion of such irrational factors as will, emotion, imagination. Bocheński was convinced that the best available logic is mathematical logic (formal logic, logistic)—cf. for example (1936)—but later he thought that certain philosophical problems required richer logical tools.

After the war, he rejected Thomism and followed analytic philosophy, being faithful to the discussed metaphilosophical principle and stressing the question of the method. Doubting whether one exact philosophical system, embracing all philosophical issues, could be built he analysed unconnected problems separately, always using exact logical methods.

Refuting the accusations that were made during the discussion at the aforementioned meeting of the Cracow Circle in Kraków in August 1936, Fr Bocheński paid attention to the necessity of distinguishing between formal logic and philosophy as well as to the fact that in antiquity there had been logical systems different from Aristotle's logic. Similarly, in logistic it is the classical two-valued logic that plays a fundamental role. At the same time, formal logic does not focus so much on the truth of conclusions deduced by applying logical tools—it is the task of other sciences—but on the truth of its theses. He also stressed the possibility of using many-valued logics in theology. These logics could be treated as the logics of probability and utilised to evaluate the degrees of falsity—this may allow us to realise the idea of St Thomas Aquinas. Moreover, Bocheński claimed that the process of constructing logical systems did not assume any philosophical presumptions—logic is and should be neutral. The fact that mathematical logic grew out of mathematics, and like mathematics it uses symbolic notation, does not mean that formal logic can be used only in mathematics. It can and should be used wherever deduction is used—the deduction should be always exact and accurate.

When Bocheński cultivated philosophy in the spirit of analytic philosophy, he used the broadly understood logic, embracing formal logic as well as semiotics, which was based on it, and the general methodology of sciences. In his opinion, this conception of logic is an ideal pattern of rationality; it provides notional tools to

[213] 'Ścisłym nazywamy sposób mówienia, w którym obowiązują następujące zasady: Jeśli chodzi o użyte słowa, mają one być bądź niedwuznacznymi znakami prostych rzeczy, cech, doznań itp., bądź też być na gruncie poprawnie sformułowanych dyrektyw za pomocą takich właśnie znaków jasno zdefiniowane. Słowa te mają być dalej użyte zawsze tak, by każde z nich stanowiło część zdania, to jest wyrażenia, które jest prawdziwe albo fałszywe. Jeśli chodzi o zdania mogą one być uznane dopiero wtedy, gdyśmy sobie w pełni zdali sprawę, co znaczą i dlaczego je uznajemy. Racją tego uznania będzie niekiedy oczywistość, niekiedy wiara, niekiedy dowód—w ostatnim przypadku ma on być przeprowadzony na gruncie jasno sformułowanych i sprawnych dyrektyw logicznych.'

analyse complex argumentations and to analyse notions. It constitutes the *organon* of philosophy and being a kind of ontology it constitutes a branch of philosophy.

According to Bocheński modern logic is an autonomous science—but not only this kind of logic. In fact, in every epoch a highly developed logic had the right to be characterised as autonomous (cf. his paper of 1980). Asked whether modern logic is a mathematical discipline or whether it should be included in philosophy he answers (1980) that it depends on the definitions of mathematics and philosophy. If mathematics is defined through its method, logic, using the same method and having the same characteristics (symbolic, formalistic, deductive, objective, etc.) as the mathematical sciences, should be regarded as a mathematical discipline. As a matter of fact, the boundaries between modern logic and mathematics are blurred. However, logic is distinguished from mathematics by the maximal generality of its fundamental branches and by a higher degree of exactness. On the other hand, assuming that philosophy analyses the foundations and most general properties of objects, modern logic, as any logic, becomes part of philosophy. This thesis is also supported—in Bocheński's opinion—by the fact that modern logic has given solutions to many traditional philosophical problems. He mentions Russell's conception of logical paradoxes and his theory of systematic ambiguity (solving the eternal problem of the 'univocity of being'), Tarski's definition of truth as well as Gödel's first incompleteness theorem, which among other things show that there are no philosophical systems that could embrace the whole of reality (like Hegel's system). Thus logic, as a tool of philosophy, is also its part. This is also possible when logic is a part of mathematics, which results from—according to Bocheński—the fact that it is the most general and fundamental part of mathematics. In addition, the same truths are obligatory in the fundamental parts of all sciences.

3.10 Andrzej Mostowski

The scientific activities of Andrzej Mostowski basically belong to the post-war period. However, we include him in our book dedicated to the philosophy of mathematics in Poland in the interwar period since the sources of his works, in particular his philosophical views on logic and mathematics, which are of our interest, were formed in the pre-war period. Mostowski can be reckoned as being part of the second generation of the Lvov-Warsaw School as he was Tarski's disciple.

Being taught by the aforementioned philosopher, Mostowski accepted his general philosophical views, especially his tendency towards empiricism and apparent respect for nominalism. His sympathies were also, as it seems, with Kotarbiński's reism, i.e. the view that there exist only individual physical things.

Sympathizing with nominalism in his investigations Mostowski, focused faithfully on the principles favoured by the Polish mathematicians:

1. All commonly accepted mathematical methods should be used in metamathematical research,
2. Metamathematical investigations should not be limited by any *a priori* philosophical premises.

As a result, he used infinistic methods, which caused a certain tension—just like in the case of Tarski (cf. Sect. 3.7). What Mostowski wrote about his teacher (1967c, p. 81) can be referred to him as well. Mostowski seemed to feel much more obliged to deal with philosophical problems connected with mathematics and logic in a more extensive and systematic way. He expressed this conviction in the introduction to *Logika matematyczna* [Mathematical Logic][214]:

> Finally, the third difficulty comes from the fact that we cannot deprive logic (no matter how formal it would be) of even an unconscious philosophical base; its conscious choice being more difficult, especially that—in the present state of discussion on the foundations of mathematics—one cannot state with all certainty which of the numerous clashing views is the best one or at least a good one (1948, p. IV).[215]

This was one of the difficulties which according to Mostowski would have to be overcome while writing a book on mathematical logic—such difficulties 'are not encountered when elaborating works from the classical branches of mathematics' (1948, p. III). He also thought that the choice of a proper view went beyond logic as such. Seventeen years later he wrote in *Thirty Years of Foundational Studies. Lectures on the Development of Mathematical Logic and the Study of the Foundations of Mathematics in 1930–1964*:

> We see that the issue between Platonists, formalists and intuitionists is as undecided to-day as it was fifty years ago (1965, p. 149).

Being aware of these difficulties Mostowski avoids clear philosophical declarations in his logical and mathematical texts. In *Logika matematyczna* he writes:

> As for the third difficulty, connected with taking a definite philosophical stand concerning the foundations of mathematics, I purposely avoid mentioning these problems in the text since they obviously go beyond the frames of formal logic. I have treated a logical system as a language involving sets and relations. I have accepted the axiom of extensionality for these formations and concluded that they depend on the principles known as the simple theory of types. This standpoint is a convenient foundation to develop formal problems and

[214] This excellent textbook was, unfortunately, published only in Polish although an English edition was planned, which is testified by the fact that the title *Mathematical Logic* by Mostowski was announced on the cover of Mostowski and Kuratowski's book *Teoria mnogości* [Set Theory] (1952). However, these plans have never been fulfilled.

[215] 'Trzecia wreszcie trudność pochodzi stąd, że nie potrafimy pozbawić logiki (jakkolwiek bądź byłaby ona formalna) pewnego choćby podświadomego podkładu filozoficznego, którego wybór świadomy jest tym trudniejszy, że—w obecnym stanie dyskusji nad podstawami matematyki—nie można z całą pewnością powiedzieć, który z wielu ścierających się ze sobą poglądów jest najlepszy albo chociażby dobry.'

corresponds to the less or more conscious stand taken by most mathematicians—which in fact does not mean that it must be acknowledged without reservation by philosophers (1948, p. VI).[216]

The quoted fragment suggests a certain affinity of Mostowski's views with the views of Leśniewski, shared also by Tarski, and known as 'intuitive formalism.' According to them mathematics is not a set of purely formal games because it uses languages equipped with certain meanings although they are formalized. This may explain why Mostowski was never a special enthusiast of Hilbert's formalism.

Moreover, in his work 'A Classification of Logical Systems' (1949–1950) Mostowski assumes a certain philosophical presumption on the analysed logical systems. Declaring at the beginning of his work that 'The subject itself as well as the method of its presentation will be of a mathematical rather than philosophical character' (1949–1950, p. 245) he openly states:

Although our investigations will be purely formal we shall nevertheless accept a definitive philosophical point of view with respect to logical systems. We shall not consider logical systems as void schemata deprived of any interpretation. On the contrary we shall assume the objective existence of a kind of "mathematical reality" (e.g. of the set of all integers or the set of all real numbers). By objective existence we mean existence independently of all linguistic constructions (1949–1950, pp. 246–247).

The task of logical and mathematical systems is—according to Mostowski—to describe this 'mathematical reality.' Consequently, every logical proposition is equipped with a certain meaning—it says that mathematical reality is entitled to have this or that property. If in fact the mathematical reality has a given property, this proposition will be true and if not—it will be false. The intuitions connected with the latter can be defined by the methods supplied by Tarski's theory of truth. At the same time, the fact of the existence of propositions that are true but unprovable in a specified system can be explained by the bigger complexity of the properties of this 'mathematical reality' than the complexity of the properties deductible from axioms by the accepted rules of argumentation. Mostowski ends his remarks in a characteristic way, writing:

We do not intend to defend the philosophical correctness or even the philosophical acceptability of the point of view here described. It is evident that it is entirely opposite to the point of view of nominalism and related trends (1949–1950, pp. 247).

One can clearly see the tension between Mostowski's aforementioned inclinations towards nominalism and his concrete logical and mathematical investigations.

[216] 'Co do trzeciej trudności, związanej z zajęciem określonego stanowiska filozoficznego w zakresie podstaw matematyki, to celowo unikam poruszania tych zagadnień w tekście, gdyż wykraczają one oczywiście poza ramy logiki formalnej. System logiczny potraktowałem jako język, w którym mówi się o zbiorach i relacjach. Przyjąłem dla tych tworów pewnik ekstensjonalności i uznałem, że podlegają one zasadom, znanym pod nazwą prostej teorii typów. Stanowisko takie jest dogodną podstawą do rozwinięcia zagadnień formalnych i pokrywa się z mniej lub więcej uświadomionym stanowiskiem większości matematyków—co zresztą nie znaczy, by musiało być uznane bez zastrzeżeń przez filozofów.'

The same conviction concerning the weight and significance of philosophical questions can be found in the introduction to the monograph *Teoria mnogości* [Set Theory] written by Mostowski and Kazimierz Kuratowski (cf. Kuratowski and Mostowski 1952).[217] Reflecting on the development of set theory, they write that one of the fundamental questions, which must be taken into account in the foundations of this theory, is the problem on which axioms set theory should be based. One should choose axioms that guarantee that the theory based on them will have 'an essential scientific value, i.e. will be able to serve in the process of getting known the material world, either directly or indirectly via other domains of mathematics for which it will be a tool' (1952, p. V).[218] Therefore, they reach the conclusion that:

> There exists so far no comprehensive philosophical discussion of basic assumptions of set theory. The problem whether and to what extent abstract concepts of set theory (and in particular of those parts of it in which sets of very high cardinality are considered) are connected with the basic notions of mathematics being directly connected with the practice has not been clarified so far. Such an analysis is needed because by Cantor, the inventor of set theory, basic notions of this theory were encompassed by a certain mysticism (1952, p. VI).[219]

On the other hand, the authors are convinced that the importance of set theory for the foundations of mathematics was also revealed because of certain problems concerning the philosophy of mathematics. They write:

> In this domain the influence of set theory can be especially strongly seen. In particular, thanks to the definition of a finite set and to the introduction of cardinals, the arithmetic of natural numbers could be founded on a firm basis. Simultaneously, new problems connected with the general concept of an infinite set has been established and precisely formulated. This concept has no mystical character any more as it was the case through ages (1952, p. VII).[220]

[217] The discussed remarks were repeated in the second and third editions of the monograph.

[218] 'istotną wartość naukową, tj. żeby mogła służyć do poznania świata materialnego, czy to bezpośrednio, czy też za pośrednictwem innych działów matematyki, dla których jest narzędziem.'

[219] 'Nie jest też dotychczas przeprowadzona wszechstronna dyskusja filozoficzna podstawowych założeń teorii mnogości. Zagadnienie, czy i do jakiego stopnia bliski jest związek pojęć abstrakcyjnej teorii mnogości (a zwłaszcza tych jej działów, w których jest mowa o zbiorach bardzo wysokiej mocy) z podstawowymi pojęciami matematycznymi bezpośrednio związanymi z praktyką, nie jest dotąd wyjaśnione. Potrzeba takiej analizy jest tym większa, że u twórcy teorii mnogości—Cantora—podstawowe pojęcia tej teorii były owiane duchem mistycyzmu.'

[220] 'W tej dziedzinie wpływ teorii mnogości daje się szczególnie silnie odczuć. Tak na przykład, dzięki zdefiniowaniu zbioru skończonego i wprowadzeniu liczb kardynalnych ugruntowano na mocnych podstawach arytmetykę liczb naturalnych. Równocześnie powstała nowa należycie sprecyzowana problematyka matematyczna związana z ogólnym pojęciem zbioru nieskończonego. Pojęcie to nie ma dziś już nic w sobie z charakteru mistycznego, którym przez stulecia było obarczone.'

Kuratowski and Mostowski also declare that the most essential feature of set theory is the fact that it provides *tools* for other branches of mathematics which are directly connected with applications.

These were the authors' remarks of philosophical nature included in the introduction. However, the chapters of the book explicating set theory contain no philosophical remarks at all! Furthermore, realising the controversial character of some set-theoretical axioms—in particular the axiom of choice—on the one hand and their importance to mathematics on the other, the authors assume the axiom of choice, but every time they use it to prove some theorem they clearly indicate this fact (marking the given theorem with a small circle ^{o}).[221] Thus Kuratowski and Mostowski follow the characteristic way of the Polish mathematicians, the way we have already emphasised (cf. for instance Sect. 2.1, Chap. 2). According to this way the philosophy of the axiom of choice (and other axioms of similar character) should be separated from its role in mathematics, which was aptly expressed by Sierpiński, writing (cf. Sect. 2.1, Chap. 2):

> Still, apart from our personal inclination to accept the axiom of choice, we must take into consideration, in any case, its role in the set theory and in the calculus. On the other hand, since the axiom of choice has been questioned by some mathematicians, it is important to know which theorems are proved with its aid and to realize the exact point at which the proof has been based on the axiom of choice; for it has frequently happened that various authors have made use of the axiom of choice in their proofs without being aware of it. And after all, even if no-one questioned the axiom of choice, it would not be without interest to investigate which proofs are based on it and which theorems are proved without its aid—this, as we know, is also done with regards to other axioms (1965, p. 95).

The same method to separate the theorems dependent on the axiom of choice by a little circle (to illustrate its role) was used in the English translation of Kuratowski and Mostowski's monograph (1969). On this occasion, we can note that almost all philosophical remarks on set theory and its axioms, which are included in the Polish version and which we have discussed above, were omitted.[222] We can only find the following:

> Close ties between set theory and philosophy of mathematics date back to discussions concerning the nature of antinomies and the axiomatization of set theory. The fundamental problems of philosophy of mathematics such as the meaning of existence in mathematics,

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[221] They write: 'To show the role of the axiom of choice we have marked those theorems in which the axiom was used with a little circle. Moreover, we have added numerous applications of the axiom [. . .] and remarks concerning the role of the axiom of choice in particular proofs; finally, we have supplemented our exposition by adding a fragment presenting one of the most paradoxical conclusions to which the axiom of choice leads.' ('Aby uwidocznić rolę aksjomatu wyboru, zaznaczyliśmy kółeczkiem ^{o} te twierdzenia, w których dowodzie pewnik jest użyty. Nadto dodaliśmy liczne jego zastosowania [. . .] oraz uwagi, dotyczące roli aksjomatu wyboru w poszczególnych dowodach; wreszcie uzupełniliśmy nasz wykład dodatkiem, w którym przedstawiony jest jeden z najbardziej paradoksalnych wniosków, do których prowadzi aksjomat wyboru') (1952, p. IX).

[222] Let us add that we find no remarks of philosophical nature in Mostowski's monograph (1969) dedicated to constructible sets and their applications.

axiomatics versus description of reality, the need of consistency proofs and means admissible in such proofs were never better illustrated than in these discussions (1969, p. V).

Mostowski also returned to the philosophical problems of set theory later, especially commenting on the results of Paul J. Cohen concerning the independence of the axiom of choice and the continuum hypothesis from the axioms of the Zermelo-Fraenkel set theory ZF (cf. papers 1964, 1967a, 1967b, 1968). In his (popular) works (1964) and (1968), Mostowski mentions the following questions as the most important problems of the philosophy of mathematics: (1) what are sets and how their properties can be discovered, (2) in particular, what are the sets of reals, (3) can every set be determined by defining the property of its elements (and consequently, can we identify a set with this property) or is it some abstract object existing independently of our mental constructions? Then he concludes:

Unfortunately, the problem of truth in mathematics is not easy. Let us repeat again: If sets existed in the same sense as physical objects we could expect that the truth or falsity of the continuum hypothesis will finally be discovered. However, if sets are only our own construction of thought the answer to the question whether the continuum hypothesis is true or false can depend on which constructions we will accept as permitted (1968, p. 177).[223]

In Mostowski's opinion, nothing can be said about the admissibility of Platonism in set theory and consequently, it is not known whether the question concerning the truth or falsity of the continuum hypothesis has any sense. On the other hand, formal problems regarding its consistency with the axioms of set theory or its independence from the axioms are interesting to a high degree. The results concerning the independence of the axiom of choice and the continuum hypothesis from ZF gained from Cohen (supplementing the earlier results of Kurt Gödel concerning the consistency of these propositions) do not solve the problem of truth in set theory. Moreover, if the axiom of choice and the continuum hypothesis can be solved on the basis of accepted axioms of set theory this fact can be treated as 'one of the most important arguments against mathematical Platonism' (1968, p. 176). This situation is analogous to the one in geometry: axiomatic set theory is currently in the same situation as axiomatic geometry was after Klein's and Poincaré's works, which showed the real meaning of the problem concerning the truth of parallel axiom. After Cohen's results, various possible but mutually inconsistent axiomatic set theories can be constructed. If such theories are constructed 'we shall be forced to admit that in the match between Platonism and formalism the latter has again scored one point' (1968, p. 182).

In Mostowski's opinion, another source of problems is the selection and acceptance of the axioms of infinity. In the paper 'O niektórych nowych wynikach

[223] 'Niestety problem prawdy w matematyce nie jest prosty. Powtórzmy jeszcze raz: Jeśli zbiory istniałyby w tym samym sensie jak obiekty fizyczne, moglibyśmy oczekiwać, że prawdziwość lub fałszywość hipotezy continuum zostanie w końcu odkryta. Jeśli jednak zbiory są tylko naszą własną konstrukcją myślową, odpowiedź na pytanie, czy hipoteza continuum jest prawdziwa, czy fałszywa może zależeć od tego, jakie konstrukcje przyjmiemy za dozwolone.'

metamatematycznych dotyczących teorii mnogości' [On Some New Metamathematical Results Concerning Set Theory] he wrote:

> The proofs of independence of the axioms of infinity are generally easy. But there is no hope to obtain proofs of their relative consistency. Gödel's second incompleteness theorem shows that a proof of relative consistency could not be formally conducted in set theory. In the light of what we have already said about the reconstruction of mathematics in set theory it is not easy to realise what such a non-formalisable proof would look like. Therefore, we must state that there are no rational foundations to accept strong axioms of infinity (1967b, p. 103).[224]

In his paper 'Recent Results in Set Theory' (1967a), Mostowski reaches stronger conclusions. He says that the complex and not completely clear nature of the notion of a set and consequently, the possibility of various axiomatisations of set theory leads—regardless of the fact that set theory is very important from the mathematical and philosophical point of view—to eliminating set theory as a central mathematical discipline. On the one hand, most (if not all) mathematical concepts can be interpreted and defined in set theory (which 'is a remarkable phenomenon which evidently calls for explanation'—1967a, p. 83) but on the other hand, '[. . .] there are several essentially different notions of set which are equally admissible as the intuitive basis for set theory' (1967a, p. 82). Mostowski concludes:

> Of course if there are a multitude of set theories then none of them can claim the central place in mathematics. Only their common part could claim such a position; but it is debatable whether this common part will contain all the axioms needed for a reduction of mathematics to set theory (1967a, pp. 94–95).

This confirmed and strengthened the doubts which Mostowski formulated concerning the axiomatisation of set theory in his paper 'Współczesny stan badań nad podstawami matematyki' [The Present State of Investigations of the Foundations of Mathematics] (1955b; cf. also 1955a), writing:

> A particularly disturbing fact which calls for explanation is that recently various new axioms have been added to the system of axioms of the theory of sets or the formulation of axioms have been altered; in consequence we have at present to choose between a great many essentially different systems of axioms of the set theory, yet there are no criteria indicating the proper choice among all these numerous systems (1955a, p. 19).

His statements in the paper *Sets* are similar:

> [. . .] the mere incompleteness of Z-F is not an alarming symptom by itself. What is disturbing is our ignorance of where to look for additional information which would permit us to solve problems which seem very simple and natural but which are nevertheless left open by the axioms of Z-F. We come here very close to fundamental problems of the

[224] 'Dowody niezależności aksjomatów nieskończoności są na ogół łatwe. Natomiast nie ma żadnej nadziei na uzyskanie dowodów ich względnej niesprzeczności. Drugie twierdzenie Gödla o niezupełności pokazuje, że dowód względnej niesprzeczności nie mógłby być formalnie przeprowadzony w teorii mnogości. Wobec tego, co powiedzieliśmy wyżej o rekonstrukcji matematyki w teorii mnogości, nie jest łatwo zdać sobie sprawę, jak wyglądałby taki nieformalizowalny dowód. Musimy więc stwierdzić, że nie ma racjonalnych podstaw do przyjmowania mocnych pewników nieskończoności.'

> philosophy of mathematics whose basic question is: what is mathematics about? A formalist would say that it is about nothing; that it is just a game played with arbitrary selected axioms and rules of proof. The incompleteness of Z-F is thus of no concern for a formalist. Platonists on the contrary believe in the “objective existence” of mathematical objects. A set-theoretical Platonist believes therefore that we should continue to think more about sets and experiment with them until we finally discover new axioms which, added to Z-F, will permit us to solve all outstanding problems. [. . .] Whatever the final outcome of the fight between these two opposing trends will be, it is obvious that we should concentrate on the study of concepts which seem perfectly clear and perspicuous to us. In Cantor’s time the concept of an arbitrary set seemed to be a very clear concept, but the antinomies proved that this was not so. Today, this concept has been replaced by that of an arbitrary subset of a given set. In addition, the belief that all subsets of a given set form a set is almost universally accepted. However, it is by no means true that these views are shared by all mathematicians. Even Gödel himself, who [. . .] should be counted among Platonists, has once expressed the view that the concept of an arbitrary subset of a given set is in need of clarification. [. . .] The present writer believes (although he cannot present convincing evidence to support this view) that it is in this direction where the future of set theory lies (1972b, pp. 28–29).

Mostowski is convinced that ‘the ultimate formulation of axioms of set theory should be preceded by a discussion of the fundamental assumptions of this theory, including the constructive standpoint’ (1955a, p. 20).[225]

Moreover, Mostowski analysed philosophical questions in connection with Gödel’s incompleteness theorem. Like in the case of set theory he pointed only to the philosophical problems connected with these theorems and showed possible solutions, avoiding any definite declarations. Furthermore, his philosophical remarks were reduced to a minimum. They can be found in two places: the paper ‘O zdaniach nierozstrzygalnych w sformalizowanych systemach matematyki’ [On Undecidable Propositions in Formalised Systems of Mathematics] (1946) and the introduction[226] to his book *Sentences Undecidable in Formalized Arithmetic. An Exposition of the Theory of Kurt Gödel* (1957).

Mostowski declares that he does not intend to discuss the philosophical problems whether questions which are unsolvable today are in fact ‘essentially undecidable’ or not. He sees the fundamental difficulty here in the fact that we lack a precise notion of a correct mathematical proof. A notion of a formal proof introduced and developed by mathematical logic made it possible to construct and investigate formalised systems. We hold the conviction that such systems encompass the whole of mathematics, i.e. that any intuitively correct mathematical reasoning can be formalised in such systems. However, one cannot prove that a given formalised system coincides with the intuitive mathematics and hence ‘[. . .] there is no immediate connection between the problem of completeness of any proposed formal system and the problem of existence of essentially undecidable mathematical problems’ (1957, p. 3).

[225] The problem of Mostowski’s sympathy towards constructivism will be further discussed.

[226] In the foreword it was explained that this introduction was an almost literary translation of some fragments of the article (1946).

Neither in the quoted article nor in the book is there further discussion on the consequences of Gödel's incompleteness theorems for the epistemology of mathematics. Both the book and the article give only a technical explication of Gödel's theorems.

Gödel's incompleteness theorems and Tarski's undefinability theorem were also the source of certain important (also philosophically) remarks of Mostowski concerning the relations between syntax and semantics. In fact, he was the first researcher that stated clearly that semantics required infinitistic methods while finitary methods were sufficient for syntax. The precise description of the differences between the syntactic and semantic formulation of the incompleteness theorems was a side effect of this observation and led to the following important conclusion:

> The interpretation of a language is defined by means of set-theoretical concepts, which gives rise to the close relations between semantics and the set-theoretical, infinitistic philosophy of mathematics; whereas the theory of computability leans toward a more finitistic philosophy (1965, p. 42).

Certain philosophical remarks—and even a clear declaration—on the relations between mathematicians' research practice and formalised systems can be found in Mostowski's paper 'Matematyka a logika' [Mathematics vs. Logic] (1972a).[227] He states:

> [. . .] a full formalisation of mathematics is an out-of-date idea nowadays. Antinomies in set theory do not frighten any more. Mathematics, whose prevailing part has not suffered from 'the crisis of foundations,' is being developed, not paying any attention to what is happening in its foundations (1972a, p. 82).[228]

And he adds that 'the tendency to mechanise mathematical reasonings seems to me to be a highly dehumanised activity: as E. L. Post once wrote, the essence of mathematics consists in concepts of truth and meaning' (1972a, p. 84).[229] He stresses that:

> A mathematical proof is something much more complicated than a simple succession of elementary rules contained in the so-called inference rules [. . .]. Therefore, one must necessarily show moderation in stressing the role of logical rules in proofs: if we emphasise this role too strongly we will change the mathematical lecture into something close to a

[227] This article is actually an extensive review of A. Grzegorczyk's book *Zarys arytmetyki teoretycznej* [Outline of Theoretical Arithmetic], but includes many general remarks on logic and mathematics.

[228] '[. . .] pełna formalizacja matematyki jest w tej chwili hasłem już przebrzmiałym. Antynomie teorii mnogości nikogo już nie straszą. Matematyka, której przeważna część w ogóle nie ucierpiała z powodu "kryzysu podstaw", rozwija się dalej, nie bardzo dbając o to, co dzieje się w jej podstawach'

[229] 'dążenie do mechanizacji rozumowań matematycznych wydaje mi się czynnością wysoce zdehumanizowaną: jak napisał kiedyś E. L. Post, istotą matematyki są pojęcia prawdy i znaczenia.'

> formalised system, and such systems [. . .] have no meanings nowadays and they only frighten most listeners out of mathematics (1972a, pp. 83–84).[230]

On the other hand, although the old programme of the formalisation of mathematics was practically rejected, 'the collaboration of logic and mathematics was fruitful and certainly will still bring important results' (1972a, p. 83).[231]

Philosophical remarks were the starting point of a series of lectures in which Mostowski '[. . .] sketch[ed] the development of mathematical logic and of the study of foundations of mathematics in the years 1930–1964' (1965, p. 7). He presented them during the Summer School in Vaasa, Finland, in the year 1964, and published in a book entitled *Thirty Years of Foundational Studies* (1965). During the cycle of lectures he firstly mentioned three trends of contemporary philosophy of mathematics, prevailing in the 1920s and 1930s, namely logicism, intuitionism and formalism. He stressed that they had contributed to the formation of three directions in logical-mathematical investigations: constructivism, metamathematical direction and set-theoretical direction. The work of Heyting from 1930—he gave the axiomatisation of intuitionistic logic, Gödel's dissertation from 1931, in which he proved the incompleteness theorem, and Tarski's work published in 1933, in which he presented the definition of the notions of satisfaction and truth, can be treated, respectively, as the beginnings of the mentioned 'schools of thinking' in mathematical logic. Constructivist investigations focus on various formal (mathematical) conceptual constructions and logical relations between them. Investigations in the metamathematical trend are mainly dedicated to logical relations 'inside' axiomatic systems and logical properties of such systems. The set-theoretical trend underlines the semantic properties of expressions of formal languages.

In his next lectures Mostowski did not enter into any philosophical discussions—he merely made certain sceptical remarks. Looking at his opinions, which he formulated on the margins of his main considerations, one can conclude that the fundamental problem to be solved in the philosophy of mathematics is the foundations of set theory and the question of the origin of mathematical concepts as well as the problem of laws governing the development of mathematics.

Mostowski considered these two last problems in his earlier work 'Współczesny stan badań nad podstawami matematyki' [The Present State of Investigations of the Foundations of Mathematics] (cf. 1955a and 1955b).[232] Although in the

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[230] 'Dowód matematyczny jest czymś o wiele bardziej skomplikowanym niż proste następstwo elementarnych prawideł zawartych w tzw. regułach wnioskowania [. . .]. Dlatego niezbędny jest umiar w podkreślaniu roli praw logicznych w dowodach: jeśli będziemy zbyt usilnie tę rolę podkreślali, zamienimy wykład matematyczny na coś zbliżonego do systemu sformalizowanego, a systemy takie [. . .] nie mają już dziś znaczenia i tylko odstraszają większość słuchaczy od matematyki.'

[231] 'współpraca logiki i matematyki była owocna i zapewne nadal będzie przynosić ważne rezultaty.'

[232] Both works are, respectively, the English and Polish version of the same text. Mostowski presented it (in an abbreviated form) during the Seventh Congress of Polish Mathematicians,

introduction he declares that he confines himself to 'purely mathematical problems, i.e. to problems connected with such notions and methods as are specific to mathematics' (1955a, p. 3), there are many philosophical remarks in the paper. Unfortunately, the origin of the work poses problems and interpretative questions. As the work originated in the middle 1950s the ideological atmosphere of that period could have influenced certain formulations and theses which were included in it. At present, one cannot determine to what extent the non-substantial factors influenced the author. On the other hand, the author could have confined himself to purely mathematical matters and avoided the necessity to formulate philosophical theses. Since he did not do that we have the right to treat his words and remarks as expressing his authentic convictions.

Mostowski begins by formulating two general problems concerning the whole of mathematics. He poses these questions after having discussed the foundations of the theory of sets, connected first of all with the discovery of antinomies:

A. What is the nature of notions considered in mathematics? To what extent are they formed by man and to what extent are they imposed from outside, and whence do we gain knowledge of their properties?
B. What is the nature of mathematical proofs and what are the criteria allowing us to distinguish correct from false proofs (1955a, p. 3).

Simultaneously, he indicates that:

> These problems are of a philosophical nature and we can hardly expect to solve them within the limits of mathematics alone and by applying only mathematical methods. However, these general problems have given rise to more special ones which are capable of being investigated mathematically [. . .] (1955a, pp. 3–4).

Among the latter Mostowski mentions: (1) the axiomatic method, its role in mathematics and the limits of its applicability, (2) the constructive trends in mathematics, (3) the axiomatisation of logic and finally, (4) the decision problems.

Trying—on the basis of natural numbers—to answer the general question whether mathematical objects can be treated as objects which are fully defined by proper sets of axioms Mostowski states that first of all the decision does not belong to mathematics but to philosophy, and he concludes:

> The only consistent standpoint, conforming to common sense as well as to mathematical usage, is that according to which the source and ultimate '*raison d'être*' of the notion of number, is experience and practical applicability. The same refers to notions of the theory of sets, provided we consider them within rather narrow limits, sufficient for the requirements of the classical branches of mathematics.
>
> If we adopt this point of view, we are bound to draw the conclusion that there exist only one arithmetic of natural numbers, one arithmetic of real numbers and one theory of sets; therefore it is not possible to *define* these branches of mathematics by systems of axioms which are supposed to establish once and for all their scope and their content (1955a, pp. 15–16).

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which was held in Warsaw on 6–12 September 1953. His main theses were also published as a short report (1954).

Axiomatic systems allow us to systematise certain fragments of the theories, namely those that embrace our present knowledge. Moreover, they sometimes facilitate the exposition of the given theory and thus are of didactical value. Mostowski adds:

> Materialistic philosophy has since long been opposed to such attempts and has shown the idealistic character both of Hilbert's program which consists in defining the content of mathematics by its axioms and of the neopositivistic program consisting in the explanation of the content of mathematics by an analysis of the language (1955a, p. 16).

Gödel's incompleteness theorem proving that natural numbers cannot be fully characterised by systems of axioms and that there exist non-isomorphic models of arithmetic should not, however, lead to drawing pessimistic conclusions since they provide tools with the aid of which one can gain a series of results on the independence of certain sentences of appropriate systems of axioms.

Similar and even more difficult problems arise in the foundations of the theory of sets. The main difficulty is the indefiniteness of the notion of an arbitrary set as well as the status of such axioms as, for example, the axiom of choice.

Mostowski's general final conclusions concern the problem of the foundations of mathematics both in the mathematical and philosophical sense. He writes:

> The problem of the foundations of mathematics is not a single concrete mathematical problem which, once solved, may be forgotten. The considerations regarding the foundations of science are just as old as science itself and mathematics is no exception to this rule. For many centuries the essence and content of mathematics have been, and probably will remain also in future, an object of considerations for philosophers. In the course of time mathematics itself changes and this also necessitates a change of views on the foundations of mathematics.
>
> [. . .] An explanation of the nature of mathematics does not belong to mathematics but to philosophy, and is possible only within the limits of a broadly conceived philosophical view treating mathematics not as detached from other sciences but taking into account its being rooted in natural sciences, its applications, its associations with other sciences and, finally, its history (1955a, pp. 41–42).

The investigations on the foundations of mathematics by the mathematical method obviously influence the formation of such a broad philosophical view. In Mostowski's opinion, the results obtained within the framework of these investigations:

> [. . .] confirm [. . .] the assertion of materialistic philosophy that mathematics is in the last resort a natural science, that its notions and methods are rooted in experience and attempts at establishing the foundations of mathematics without taking into account its originating in natural sciences are bound to fail (1955a, p. 42).

Therefore, we can see that Mostowski represents here an empirical standpoint in the philosophy of mathematics. As we have already mentioned it is not completely clear what influence the non-substantial factors connected with the then prevailing ideology, which was forced on the society, exerted on his views and the quoted formulations. The specific formulations using definite concepts that were characteristic of that ideology can suggest such an influence. This could have been the price which the author had to pay the officially promoted philosophy. On the other hand, it is worth noticing that the empirical (or *quasi-empirical*) trends have been increasingly more vivid in the philosophy of mathematics, commencing with the 1960s.

Mostowski repeated the thesis on the empirical sources of the mathematical notions in his popular work ‘O tzw. konstruktywnych poglądach w dziedzinie podstaw matematyki [On the So-Called Constructive Views in the Foundations of Mathematics]:’[233]

> Undoubtedly, all mathematical notions have been created by the process of abstraction from the notions formed on the basis of direct experience (1953, p. 231).[234]

However, he adds that this statement is not sufficient and the process of abstraction requires a deeper analysis.

Mostowski admitted that he had a keen interest in constructivism (particularly, in its aim but not necessarily its proposed solutions) (cf. 1959, p. 192). He even once believed that this direction would be fundamental in mathematics. In *Logika matematyczna* he wrote:

> I am inclined to think that a satisfactory solution of the foundations of mathematics will happen on the way shown by constructivism or a similar direction. However, one cannot now write a textbook of logic on this basis (1948, p. VI).[235]

This conviction was rooted in the fact that:

> [. . .] it [i.e. constructivism—remark is mine] wants to inquire into the nature of mathematical entities and to find a justification for the general laws which govern them, whereas platonism takes these laws as granted without any further discussion (1959, p. 192).

In his further investigations Mostowski gave up the idea of the superiority of constructivism over other views although he still saw and stressed its advantages in concrete cases. He claimed that the constructivist tendencies in the foundations of mathematics were closer to the nominalistic philosophy than the idealistic one (in the Platonic sense). This nominalistic character of constructivism does not accept general mathematical concepts as given, but tries to construct them. ‘This leads to the result that one can identify mathematical concepts with their definitions’ (1959, p. 178). In arithmetic constructivism allows us to give up assuming actual infinity or to use solutions requiring only the nominalist approach. Whereas one of the advantages of nominalism is that many important mathematical theories have been satisfactorily reconstructed on the nominalist basis, and these reconstructions have turned out to be equivalent to the classical theories.

Mostowski was aware of the fact that the finitary, predicative and constructive methods were not sufficient in mathematics (cf. 1972b, pp. 29–32). However, he

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[233] Considering the period of the origin of this paper can cause some interpretative problems similar to those appearing in the case of the aforementioned works (1955a) and (1955b).

[234] ‘Nie ulega żadnej wątpliwości, że wszystkie pojęcia matematyczne powstały przez abstrakcję z pojęć ukształtowanych na podstawie bezpośredniego doświadczenia.’

[235] ‘Jestem skłonny mniemać, że zadowalające rozstrzygnięcie zagadnienia podstaw matematyki nastąpi na drodze wskazanej przez konstruktywizm lub kierunek do niego zbliżony. Na tej jednak podstawie nie można by już teraz napisać podręcznika logiki.’

was not satisfied with the limits of constructivism but investigated its principles very thoroughly. He claimed that sometimes constructivism was philosophically more satisfactory, like in the case of arithmetic. Moreover, in applied mathematics it seems to reveal promising perspectives. Thus establishing the exact scope of constructive methods in classical mathematics is both important to mathematics and philosophy. This idea was connected with the thought of the degrees of constructivism ascribed to various mathematical theories, sketched in 'On Various Degrees of Constructivism' (1959) (cf. also 1953).

Mostowski's understanding of constructivism was best expressed in the following fragment:

> My conception of constructivism will be as naive as possible and will consist in the following. I shall consider theories of real numbers and real functions in which not arbitrary real numbers or real functions are considered but only numbers and functions which belong to a certain class specified in advance. According to the choice of this class, we shall obtain different theories of arithmetic and analysis. Our choice of the initial class will not be arbitrary: we shall try to make the choice so that the elements of the chosen class satisfy certain conditions of calculability or effectivity. We shall start with stringent conditions and then loosen them gradually and we shall see that it is possible in this way to systematize a good deal of older and also of more recent work of constructivists. I shall pay no attention to the way in which the classes just mentioned are defined and shall impose no limitations on methods of proof acceptable in dealing with numbers or functions belonging to these classes. This naive approach to constructivism is certainly objectionable from the constructivist point of view. It does not represent a constructivist development of a branch of mathematics but gives merely a glance of constructivism, so to say, from outside. The value (if any) of such an approach I see in the possibility of reviewing on a common background several of the simplest constructivist conceptions; but more refined ones and especially those which, like intuitionism, impose restrictions on methods of proof must necessarily be excluded from such a review (1959, p. 180).

Mostowski saw constructivism as being related with the classical point of view. He represented—as opposed to the pure constructivism of Heyting and the intuitionists—a certain combination of constructivism and a set-theoretical programme, which constituted the basis for his mathematically developed foundations of mathematics.

These reflections lead to the conclusion that Mostowski was aware of the philosophical problems connected with mathematics and their significance. On the one hand, he avoided (with several exceptions) formulating any philosophical declarations, focusing on purely mathematical and technical aspects of the discussed issues. When necessary, he made—though unwillingly—certain general philosophical remarks. He was also aware of the importance of the results in the area of the foundations of mathematics obtained (by mathematical methods) for the philosophy of mathematics but on the other hand, he was convinced that those results could not give definite solutions to the philosophical questions. For that reason he presented various possible solutions instead of making any declarations. He considered and discussed the philosophical questions on the margin of his proper metamathematical investigations, writing his remarks mostly in the introductions to his works and—what is important—these remarks never influenced his further purely (meta)mathematical reflections. In his technical works Mostowski did his best to avoid any philosophical remarks and commentaries. He clearly distinguished between the philosophical perspective on the one hand and the (meta) mathematical perspective on the other. Some of his results were, as one can suppose, inspired by his philosophical considerations (for example the independence of various definitions of finiteness, the constructions leading to the so-called hierarchy of Kleene-Mostowski, the constructions of models with automorphisms). However, he neither wrote about them nor formulated them, being fully concentrated on mathematical and metamathematical investigations. This is why we can only make undetermined assumptions and hypotheses.

Although Mostowski did not create any new '-ism' in the philosophy of mathematics, his works made an important contribution to this branch and can actually be recognised as a paradigmatic example of a certain understanding of interaction between mathematics and philosophical ideas. Mostowski is an excellent example of the attitude of the Polish School as far as the foundations and philosophy of mathematics are concerned.

In light of these reflections, the remark of Roman Suszko who wrote that 'Mostowski is a mathematician-logician who knows the philosophical aspect of logic and the theory of the foundations of mathematics' (1968, p. 169) seems to be to the point.

3.11 Henryk Mehlberg

Henryk Mehlberg belongs—just like Mostowski—to the second generation of the Lvov-Warsaw School. He dealt both with the philosophy of formal sciences and the philosophy of non-formal sciences. Naturally, of our interest is only the first domain and here his only important work is 'The Present Situation in the Philosophy of Mathematics' (1962) in which, after having considered the directions existing in the philosophy of mathematics, he proposed the so-called pluralistic logicism.

Mehlberg distinguishes three versions of logicism: radical, moderate and pluralistic, which he proposed himself (Hilary Putnam was also a follower of this idea). The main theses of radical logicism, represented by Russell and Whitehead in *Principia Mathematica*, can be reduced to two:

1. All mathematical concepts can be *explicite* defined by logical notions,
2. All mathematical theorems can be deduced from the laws of logic by purely logical ways of reasoning (common for the whole of mathematics).

Consequently, mathematics becomes a part of logic.

Thorough analyses have showed—which the creators of logicism knew—that in order to prove certain (even the basic ones) mathematical theorems (strictly speaking: arithmetical ones) we need certain principles and presumptions about which we might have doubts as for their purely logical character. This concerns first of all the axiom of infinity (which is, for example, needed to prove the theorem that there is a successor for every natural number) or the axiom of choice. Russell and Whitehead wanted to solve these difficulties by using the deduction theorem which allows us to change the consideration of a given theorem φ, which was proved by using axioms

$\varphi_1, \ldots, \varphi_n$ to consider the implication $\varphi_1, \ldots, \varphi_n \rightarrow \varphi$, already a thesis of logic. However, it is essentially an artificial technique. Consequently, the image of mathematics and of the ways of proving the theorems, which we thus obtain, differs very much from the real scientific practice of mathematicians.

Hence the idea of certain weakening of the postulates of logicism was conceived. In the year 1962, in 'Mathematics and Logic' (1962), Alonzo Church proposed the so-called moderate logicism, rejecting the second aforementioned postulate of Russell and Whitehead. According to A. Church, this logicism was fully justified by the factual situation in mathematics. Its consequence is not so much the thesis that mathematics is a part of logic but that logic is prior to mathematics and occupies a place before it.

In Mehlberg's opinion, this does not solve all problems since it is difficult to include the notion of a set to purely logical notions. What is more, one cannot construct a system of logic without using any set-theoretical notions and methods—thus they become indispensible on the level of metamathematics. On the other hand, there is no single commonly accepted version of axiomatic set theory. Then the question arises: which one should be chosen. Mehlberg proposes pluralistic logicism. In his opinion it solves the problems of the other versions of logicism, and on the other hand, '[...] is capable of providing a satisfactory adjustment to the present situation in foundational research because such a version of logicism supplies a common basis for the main trends in contemporary philosophy of mathematics' (1962, p. 79). Speaking about the contemporary situation in research concerning the foundations of mathematics he meant first of all Gödel's incompleteness theorems and Church's theorem.

According to Mehlberg, intuitionism and formalism are not very diametrically different ways to approach mathematics. Thanks to the results of Kolmogorov, Gödel, Kleene and Glivenko concerning mutual interpretability we know that these systems are in some way similar, and that the consistency of intuitionist mathematics is equal to the consistency of classical mathematics. On the other hand, intuitionism is, according to Mehlberg, in some way similar to logicism. He writes:

> Let us notice, however, that if the difference separating classicists from intuitionists would affect only their respective interpretations of logical symbols, then intuitionism would become a version of logicism, no matter how much the two interpretations of logical terms differ from each other. More importantly, it would seem that, from a practical point of view, a working mathematician could hardly be expected to appreciate the separate and individual nature of intuitionist mathematics under the circumstances just mentioned. For the mathematician is essentially an *architect of proofs*, no matter whether he lives inside or outside Holland (1962, p. 93).

This thinking allows Mehlberg to propose a certain conception of the philosophy of mathematics aiming at bringing together logicism, intuitionism and formalism—this is to be done by pluralistic logicism. It refers to the trick based on the aforementioned deduction theorem, which Russell and Whitehead knew. It allows us to consider simply the implication $\varphi_1, \ldots, \varphi_n \rightarrow \varphi$, where $\varphi_1, \ldots, \varphi_n$ are axioms used in a proof of φ instead of the given theorem φ. Thus we have the following principle:

> Any proof which is valid in some particular mathematical theory has a valid *replica* which clearly belongs to some pure logic and can therefore be established by a logician within his own field (1962, p. 94).

Another argument supporting Mehlberg's thesis is the fact that the deduction theorem is applied both in classical and intuitionist logic. Thus the author reaches the following conclusion:

> Hence, both classical *and* intuitionist mathematics have their exact *replicas* in their respective logical systems. The only difference between these two kinds of mathematics would seem to consists in the circumstance that two different logics are used in these kinds of mathematics (1962, p. 95).

Let us notice that the proposal of pluralistic logicism is similar to Aristotle's views on mathematics. Since Aristotle claimed that in mathematics certainty and necessity did not belong to particular theorems but only to the logical relations between propositions expressed by conditional statements.

It is worth stressing that pluralistic logicism allows both the realistic, or even Platonic, and the nominalistic understanding of logic and is consistent with all of them. It also solves the difficulties of Church's moderate logicism—indeed, here it does not matter whether, for example, the notion of a set is included into (purely) logical concepts or not. Pluralistic logicism stresses, in Mehlberg's opinion, the feature of mathematical knowledge that '[...] for every mathematical theory there exists *some* logic capable of providing the necessary tools for the derivation of all the relevant theorems of this theory without any recourse to extralogical "intuition"' (1962, p. 100), which is to justify and explain the use of the adjective 'pluralistic.'

It is also worth paying attention to certain epistemological conclusions which Mehlberg draws from Gödel's second incompleteness theorem. According to this theorem, there are no absolute proofs of consistency of richer (containing the arithmetic of natural numbers) and thus more interesting mathematical theories. Consequently, in practice we must follow the assumption that the discussed theory is consistent on the basis of the fact that so far no contradiction has been found in it. Naturally, it does not exclude the possibility of finding some contradiction in the future. However, it is sufficient to include this theory to the resources of human knowledge and do not place the label 'faith' on it, which resembles the situations we face dealing with empirical sciences.

The unification of mathematical knowledge—by for example its reduction to logic or set theory or logic and set theory—can be treated as constructing the foundations of mathematics only when the axioms of the unifying system are more certain and more reliable than the axioms of the system undergoing unification or reduced to logic or set theory. Mehlberg notices that it was Gödel that thought: '[...] so-called logical or set-theoretical "foundations" for number-theory, or any other well established mathematical theory, is explanatory, rather than really foundational [...]' (1962, p. 86). This quotation is another emphasis of the similarity between mathematics and empirical sciences, for instance physics.

Chapter 4
Benedykt Bornstein

Benedykt Bornstein wrote his doctoral dissertation under the supervision of Kazimierz Twardowski but he is not included in the Lvov-Warsaw School—mainly because of his metaphysical views. In some way he was an individualist; his research did not follow the main trend and that is why we have placed him in a separate chapter.

In fact, his conceptions did not win recognition and greater interest of his contemporaries. He worked with a certain level of isolation although he participated in philosophical congresses and published his works in the chief periodicals both in Poland (such as *Przegląd Filozoficzny*, *Wiedza i Życie*, *Przegląd Klasyczny*) and abroad. His scientific activities can be divided into three periods: in the first one he translated Kant's works and developed his ideas in a critical way; the second period was dedicated to investigations concerning the philosophy of mathematics, and the third period—to problems of metaphysics cultivated in the spirit of the classical trend. His works written in the second period raised some interest of Polish philosophers. His investigations concerning the philosophy of mathematics led to the formulation of a new philosophical method in the form of categorical geometrical logic. The theme of our book makes us focus on the latter investigations.

Let us begin by discussing Bornstein's reflections on the philosophy of geometry. Here Bornstein referred to Kant's transcendental aesthetics and Twardowski's theory of images and concepts. At the same time, he criticised the idea of constructing geometry on the basis of set theory or topology; he also distanced himself from Poincaré's conventionalism. In his opinion, constructing a geometry should be begun by constructing proper geometrical concepts, which have their objective references. In his book *Prolegomena filozoficzne do geometryi* [Philosophical Prolegomena to Geometry] (1912) he distinguished between the image of physical space and the concept of geometrical space, and he followed the idea that the so-called background image must be an image the object of which exists and is truly perceived, which is to guarantee that the common features of the object of the concept of geometrical space and the object of the background image will not only concern the world of objective images but also be grounded in the experiential

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6_4

reality. According to Bornstein, one of the common features of both objects is three-dimensionality. He wrote in *Prolegomena filozoficzne do geometryi*:

> If we analyse this image with respect to spatiality we will be always convinced that its object is three-dimensional, i.e. it has length, width and height (or depth); that from each of its points we can draw three perpendicular lines, belonging to the given object in some space. This objective spatiality, characterising three-dimensionality, is a common feature of our background image and the object of the concept of geometrical space, based on that image (1912, p. 8).[1]

Three-dimensionality is determined by experience and is not—as Poincaré claimed—a separate mental construction.[2]

As far as the question of the choice between Euclidean and non-Euclidean geometries is concerned, Bornstein thought that:

> From the purely logical or analytical point of view the theorems or formulas of non-Euclidean geometry contain no contradictions, and being logically possible they are equally eligible as the theorems and formulas of Euclidean geometry (1912, p. 89).[3]

At the same time, experience cannot help us choose one, true and correct geometry. Bornstein wrote in *Prolegomena*:

> If now the followers of the purely logical or analytical concept of geometry turn to experience with the question which of the three logically possible systems of theorems is important to experience and is confirmed by it, they must be prepared not to receive any answer to their question. [. . .] In a word, when we turn to experience to show us which of the possible logical systems is confirmed by it, which is true, then experience will never give us any answer since its data will present a constant in the equation with two unknowns (one geometrical and the other physical), and so they will be insufficient to solve precisely this geometrical unknown in the equation (1912, pp. 89–90).[4]

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[1] 'Jeżeli zanalizujemy takie wyobrażenie pod względem przestrzenności przekonamy się zawsze, że przedmiot jego jest trójwymiarowy, t.j. że posiada długość, szerokość i wysokość (względnie głębokość), że w każdym jego punkcie można poprowadzić trzy prostopadłe linie, należące na pewnej przestrzeni do danego przedmiotu. Ta przestrzenność przedmiotowa, którą charakteryzuje trójwymiarowość, jest cechą wspólną przedmiotu naszego wyobrażenia podkładowego i przedmiotu pojęcia przestrzeni geometrycznej, opartego na tem wyobrażeniu.'

[2] For the particular remarks on Bornstein's views concerning the problem of essence and structure of geometrical space see Śleziński (2009).

[3] 'Z punktu widzenia czysto logicznego lub czysto analitycznego twierdzenia lub formuły geometryi nieeuklidesowej nie zawierają sprzeczności, a logicznie możliwe, są równie uprawnione, jak twierdzenia i formuły geometryi euklidesowej.'

[4] 'Jeżeli teraz zwolennicy czysto logicznego lub czysto analitycznego pojmowania geometryi zwrócą się do doświadczenia z pytaniem, który z trzech logicznie możliwych systemów twierdzeń jest ważny dla doświadczenia i znajduje w niem potwierdzenie, to muszą być przygotowani na to, że odpowiedzi na to pytanie nie otrzymają. [. . .] Słowem, gdy zwracamy się do doświadczenia, by nam wskazało, który z możliwych logicznie systemów znajduje w niem potwierdzenie, który jest prawdziwy, to doświadczenie na to pytanie nigdy nie będzie mogło dać nam odpowiedzi, gdyż jego dane będą przedstawiały wielkość stałą w równaniu z dwiema niewiadomymi (jedną geometryczną, drugą fizyczną), a więc będą niedostateczne do ścisłego rozwiązania tego równania co do niewiadomej geometrycznej.'

Bornstein claimed that real spatial extensiveness could not be identified with the extensiveness defined by the continuum of real numbers. The latter has no space character. Therefore, the attempts to transfer theorems from one domain to the other are not justified. In particular, one cannot assume *a priori* that a geometrical line does not correspond to any continuous function. In his paper 'Problemat istnienia linji geometrycznych' [The Problem of the Existence of Geometrical Lines] (1913) he showed that such lines corresponded to some continuous functions and did not correspond to other ones. Assuming that all geometrical curves have tangents we have the result that only functions with derivatives correspond to them. Consequently, if every movement must have speed, and speed is the derivative of distance with respect to time, movement cannot occur along curves without tangents. Thus not all functions are of geometrical character, in particular it concerns those functions that have no derivatives.

Bornstein also dealt with the problem of infinity. In his opinion an infinite set can be given only as a certain whole embracing infinitely many elements. At the same time, the actual infinity is never given as the infinity of its particular elements—only a finite number of them can actually be given. Thus, a question arises whether all elements of an infinite set (in the sense of actual infinity) exist physically or whether they exist in themselves independently from their actualisation. Bornstein examined these questions in his book *Elementy filozofii jako nauki ścisłej* [Elements of Philosophy as an Exact Science] (1916) asking whether an actual segment is a set of potential or actual points. He concluded that an infinite set of points situated between two points of a geometrical line existed physically in nature but not all of its elements necessarily did.

Thus we come to Bornstein's considerations on the foundations of set theory. We must above all mention his work 'Podstawy filozoficzne teorji mnogości' [The Philosophical Foundations of Set Theory] (1914). This work was criticised by Stanisław Leśniewski in his paper 'Teorja mnogości na "podstawach filozoficznych" Benedykta Bornsteina' [Set Theory on the 'Philosophical Foundations' of Benedykt Bornstein] (1914). In turn Bornstein wrote a paper 'W sprawie recenzji p. Stanisława Leśniewskiego rozprawy mojej pt. "Podstawy filozoficzne teorji mnogości"' [On Mr Stanisław Leśniewski's Review of My Dissertation 'The Philosophical Foundations of Set Theory'] (1915). Thus the polemic ended.

We cannot discuss the technical details of the polemic and more, the polemic did not bring about any effects. However, some arguments of both scientists are worth mentioning.

Let us begin by stating that in his work (1914) Bornstein notices that the source of antinomy in set theory is its erroneous philosophical justification. He concludes that a set of individually existing elements can be only finite. In addition, he bases his theses concerning the existence of finite and infinite sets having individually existing elements on the following three lemmata (cf. 1914, pp. 183–185):

- The same number corresponds to two equivalent sets with individually existing elements,

- In a set of elements, existing individually, the same number cannot correspond to the proper part of this set in the same way as to the whole,
- A set of elements, existing individually, cannot be equivalent to its own part.

He explains the used terms in the following way:

> If a plurality of elements, each existing individually, i.e. as a different unit, is analysed *only as a plurality of units*, we analyse it from the point of view of quantity; at the same time, this plurality of units constitutes the *quantity, or number* of individually existing elements of the given plurality. [. . .] between the plurality of elements, existing individually, and the plurality of units, constituting its quantity, or number, there is one-one correspondence; these pluralities are, as we say, equivalent or of equal power. [. . .] since quantity is a real feature of the plurality of elements, existing individually, whereas the number is a notional equivalent of this feature (1914, p. 183).[5]

Omitting the technical details of Bornstein's reasoning we must say that he made the error of *quaternio terminorum*, i.e. the use of the same term in two different meanings—in this case it is the term 'the same number.'

Assuming the existence of an infinite set of natural numbers Bornstein shows the essential nature of infinite pluralities. Now, in the infinite plurality of natural numbers only their finite quantity—in his opinion—can be considered individually. Therefore, there can be infinite pluralities without any possible individual content. He writes:

> [. . .] here we have a perfect example, showing the essential nature of infinite pluralities, consisting in their full independence from the matters of actualising (individualising, materialising) the elements of plurality. Here we have an example of a pure form in ideal perfectness (1914, p. 190).[6]

He also concludes that the well-ordering theorem (equivalent to the axiom of choice) 'applying in general to all kinds of plurality is wrong; whereas applying to the plurality of elements, existing individually, physically, is an obvious truth' (1914, p. 190).[7]

Leśniewski began his criticism of Bornstein's work (1914) with the following words:

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[5] 'Jeżeli mnogość elementów, z których każdy istnieje indywidualnie, tj. jako różna od innych jednostka, rozpatrujemy *tylko jako mnogość jednostek*, to rozpatrujemy ją z punktu widzenia ilości, przy czym ta mnogość jednostek stanowi właśnie *ilość, względnie liczbę* istniejących indywidualnie elementów danej mnogości. [. . .] miedzy mnogością elementów, istniejących indywidualnie, a mnogością jednostek, stanowiącą jej ilość, względnie liczbę, istnieje odpowiedniość jedno-jednoznaczna; mnogości te są, jak mówimy, równoważne lub równej mocy. [. . .] ilość bowiem jest cechą rzeczywistą mnogości elementów istniejących indywidualnie, liczba zaś jest odpowiednikiem pojęciowym tej cechy.'

[6] '[. . .] mamy tu doskonały przykład, wykazujący istotną naturę mnogości nieskończonych, polegającą na ich zupełnej niezależności od spraw zaktualizowania (zindywidualizowania, zmaterializowania) elementów mnogości. Mamy tu przykład czystej formy w idealnej doskonałości.'

[7] 'w zastosowaniu do wszelkiej mnogości w ogóle jest błędne; w zastosowaniu natomiast do mnogości elementów, istniejących indywidualnie, aktualnie, jest prawdą oczywistą.'

> Dr Benedykt Bornstein wrote a treatise in which he tried to provide set theory with 'philosophical foundations'; he thought that certain contradictions, which can be seen in set theory, are not caused by set theory but by its wrong philosophical justification, and this view of the problems, prevailing in set theory, must have been the origin of the author's desire to add to this science some thoughts, which could justify it 'philosophically' (1914, p. 488).[8]

Further, Leśniewski analyses Bornstein's formal argumentations—ignoring the ontological questions, which were so important to the latter. In particular, Leśniewski criticises Bornstein's terms 'existing individually' and 'existing formally,' accusing him of not giving any precise definition of the concept of 'unit.' In addition, he proposes to replace the term 'unit' by the term 'object,' which, however, as seen in Bornstein's response (1915) does not satisfy the latter. Leśniewski also criticises Bornstein's interpretation of Zermelo's well-ordering theorem.

Avoiding any complicated (and devoid of deeper meaning now) technical questions concerning the polemic between Leśniewski and Bornstein it would be sufficient to say that their levels of discourse were entirely different. Leśniewski defended the standard approach towards set theory (which he then refuted for the cause of mereology) against Bornstein's criticism flowing from philosophical motives. As Śleziński (2010) notices 'for Leśniewski the formal analyses are binding whereas for Bornstein the argumentations, apart from formal correctness, must refer to the objective layer of the problems under consideration' (p. 110). Leśniewski summarised his critical review of Bornstein's words in the following way:

> The work of Mr Bornstein has no value for the 'foundations' of set theory. It does not remove any 'contradictions' from set theory as Mr Bornstein seems to be claiming; on the contrary, he creates them to a much bigger extent; he does not justify them 'philosophically' and in no other way does he justify even one theorem of set theory; since one cannot justify something with the help of 'definitions' and 'lemmata' that are full of errors and contradictions; he explains nothing because the seemingly devised conceptions of something, for example the conception of 'capacity,' are inconsistent and unclear (1914, p. 507).[9]

In his response (1915) to Leśniewski's criticism Bornstein tried to specify his conception of set theory. He also saw certain inconsistencies in Leśniewski's

[8] 'Dr Benedykt Bornstein napisał rozprawę, w której starał się zaopatrzyć teorię mnogości w "podstawy filozoficzne"; uważał on, iż do pewnych sprzeczności, które dają się widzieć w teorii mnogości, prowadzi nie sama teoria mnogości, lecz błędne jej uzasadnienie filozoficzne, a pogląd taki na stan rzeczy, panujący w teorii mnogości, stanowił właśnie zapewne genezę pragnienia autora, by przysporzyć tej nauce trochę myśli, które by ją mogły "filozoficznie" uzasadnić.'

[9] 'Praca p. Bornsteina nie ma żadnej w ogóle wartości dla "podstaw" teorii mnogości. Nie usuwa ona żadnych "sprzeczności" z teorii mnogości, jak się to zdaje p. Bornsteinowi, lecz je przeciwnie w wielkiej obfitości stwarza; nie uzasadnia "filozoficznie" ani też w żaden inny sposób ani jednego twierdzenia teorii mnogości, nie można bowiem uzasadnić czegoś za pomocą "definicji" i "lematów", pełnych błędów i sprzeczności; nie wyjaśnia nic, bo obmyślone niby czegoś koncepcje, jak np. koncepcje "pojemności", są sprzeczne i niejasne.'

arguments. He was not convinced about the validity of the accusations and concluded his answer:

> Facing the foregoing arguments it seems to me that I will be impartial responding to Mr Leśniewski's review: *primo*—it does not show, even to the slightest extent, any contradictions which are to be stuck in the concepts I have used, and *secundo*—it is an example of Mr Leśniewski's extremely careless disregard of the elementary principles of logic (1915, pp. 139–140).[10]

As we have seen both debaters remained on different planes. Leśniewski conducted his argumentation and analyses in the spirit preferred by the Lvov-Warsaw School, i.e. using the apparatus of mathematical logic and focusing on formal matters, whereas Bornstein favoured ontological questions and worked in the spirit of the concept of the mathematics of quality, which he was developing himself. In particular, the latter might have been the reason why there were no polemics (except the one held by Leśniewski) with Bornstein's later works—in fact, the concept of the mathematics of quality was so different from the universally accepted tendencies and styles of thinking that it was difficult to find any common points. On the other hand, Bornstein criticised the widespread practice of treating mathematics as the science on quantity and magnitude, number and measure—in his opinion there is also qualitative mathematics, especially qualitative algebra or geometry.

Let us proceed to the next idea of Bornstein, namely, his conception of the geometrisation of logic, i.e. geometrical logic. Referring to Leibniz, who was always closer to the intensional than the extensional conception of logical forms and who wanted to construct logic based on the content of expressions and not only on the extensions of concepts, Bornstein tried to create a new logic—logic of content. Since he thought that the content of a concept sets out its extension, and thus the exactness and definiteness of the content determine the precision and definiteness of the extensions and in general, of the classes.

Bornstein divided concepts and judgements into those which were set out objectively and those which were set out logically. The former parallel objects in reality and the latter gain their meaning through definitions. In addition, Bornstein distinguishes between nominal and real definitions. In nominal definitions the definiendum as if synthesizes the essence of words constituting the definiens. In real definitions we have the reverse process—the definiendum is divided into a combination of simpler constituents occurring in the definiens. However, both types of definition are definitions *per genus proximum et differentiam specificam*. Likewise, we have judgements set out objectively and judgements set out logically. At the same time, Bornstein assumes that all judgements have subject-predicative structures.

[10] 'Wobec powyższego wydaje mi się, że będę obiektywnym, gdy o recenzji w mowie będącej p. Leśniewskiego powiem: *primo*—że w najmniejszym nawet stopniu nie wykazuje sprzeczności, tkwić mających w używanych przeze mnie pojęciach, i *secundo*—że jest przykładem niebywale lekkomyślnego nieliczenia się p. Leśniewskiego z elementarnymi zasadami logiki.'

Bornstein, following the conceptions of Edward Vermilye Huntington (1904), proposed his own system of the algebra of logic, which he formulated as categorial. He accepted three logical operations: negation, addition and multiplication. Addition consists in integrating the contents of concepts whereas multiplication sets out the biggest common element of concepts. Here two constants appear: 0 and 1, where 0 is the lower limit of the content and 1 is the upper limit of the whole content. Element 0 expresses the content of the concept of 'something' or 'the object in general' whereas element 1 presents the substantially richest concept, 'whole' and 'everythingness.' Moreover, there is a relation of the subordinance of content marked as $<$, but it has not the property of connectedness.

Furthermore, Bornstein tried to give a geometrical interpretation to his categorial logic of content. His first attempts can be found in 'Zarys architektoniki i geometrji świata logicznego' [Outline of Architectonics and Geometry of the Logical World] (1922), and then in his more mature work 'Geometrja logiki kategorialnej i jej znaczenie dla filozofii' [Geometry of Categorial Logic and Its Importance for Philosophy] (1926).[11] However, we cannot get entangled in complicated (and not always clear) technical details. Suffice it to say that Bornstein refers to projective geometry stressing its qualitative character. He shows the structure of his logic of content through various diagrams, both two-dimensional and three-dimensional. Thus he refers to the works of the old authors who used a geometrical exposition of certain logical dependencies, for example Euler's wheels, the diagrams of Venn and Haase or certain conceptions of Leibniz, Peirce and Grassmann.

The analyses on logic and the use of geometrical interpretations led Bornstein to the conclusion that both domains could be linked and thus a qualitative-categorial geometrical logic could be created. This logic can help us discover and reveal the universal structures of reality. In his work *La logique géométrique et sa portée philosophique* [Geometrical Logic and Its Meaning for Philosophy] (1928) he tried to show the similarity of the domain of thought and the domain of space objects. He tried to unite both of his systems: algebraic logic and geometrical logic in one system called topologic (Polish: topologika).

Then he generalised this system as a dialectical geometrical logic and presented it in his unpublished work *Zarys teorii logiki dialektycznej* [Outline of the Theory of Dialectical Logic] (1946). It should be stressed that he assumed the possibility of various degrees of dialecticality and consequently, various kinds of dialectical logics. In his opinion traditional logic is the least dialectical one whereas mathematical logic is partially-dialectical. In the quoted work he wanted to show that dialectical logic could be treated in a mathematical way, could be axiomatized and given a geometrical interpretation. However, the problem of the consistency of dialectical logic appeared. The need to show consistency was very essential and more, this logic was to help examine the real world. The sought-after proof of consistency would refute the accusation of the irrationality of this logic.

[11] Cf. also his unpublished works (? − a), (? − b) and (? − c).

Unfortunately, Bornstein did not give such a proof—he gave only certain arguments supporting consistency but they were disputable.

Bornstein's considerations were based on his conviction that there existed a harmony between the world of non-spatial thoughts and the world of spatial beings. He thought that mathematics and the logic of quality were objectively grounded in the real world. At the same time, he treated mathematics as an auxiliary domain of philosophy. Bornstein wanted to construct a philosophical system using mathematical concepts. He sought certain universal structures and principles of the real world; besides the quantitative aspect he looked for the qualitative aspect. In his opinion the order of the world concerns both of these aspects. Thus he spoke about the mathematics of quantity and the mathematics of quality. Mathematics is not only the science of quantity and measure but of order, in particular the order between qualities. For Bornstein metrical geometry was an example of the mathematics of quantity whereas projective geometry—the mathematics of quality. Philosophy should look for the qualitative structures of the world—its starting point should be qualitative mathematical logic.

Bornstein's conceptions did not win recognition and acceptance of his contemporaries. The reasons for this included the lack of clarity and precision of his ideas. Moreover, they were not completely worked out. Bornstein's investigations did not follow the main trend of research. The mathematical and logical tools he constructed were to create a metaphysical system and not to serve analyses, which was decidedly different from the conception of cultivating philosophy accepted and developed in the Lvov-Warsaw School.

Chapter 5
Cracow Centre

This chapter analyses the philosophical views on mathematics and logic of scientists connected with the Cracow Centre, i.e. the views of Jan Sleszyński (his last name is also given by the Polish 'Śleszyński'), Stanisław Zaremba and Witold Wilkosz. Let us add that for a certain period Zygmunt Zawirski and Leon Chwistek worked within the Cracow Centre. However, for several reasons (cf. Introduction) we have decided to discuss their works and views in the other chapters (namely, Zawirski's works in Chap. 3 and Chwistek's in Chap. 2).

5.1 Jan Sleszyński

In the Odessa period Sleszyński dealt mainly with mathematical analysis and the calculus of variations whereas in the Cracow period—mathematical logic, especially propositional calculus, which he called 'theory of proof.' Towards the end of his life he worked on the theory of numbers. Nonetheless, it should be noticed that in the Odessa period he was also interested in logic, which is confirmed by his work 'Логическая машина Жевонса' [Logičeskaja mašina Dżevonsa, Jevons' Logic Machine] written in 1893. He was an erudite person but he did not publish much on *stricte* philosophical problems.[1]

In some way Sleszyński can be regarded as the pioneer of mathematical logic in Poland. He instilled this new and intensively developing domain in Poland, including the Cracow scientific environment. Thanks to him logical elements were permanently included in lectures in mathematics at the Jagiellonian University. His works express his strong conviction of the importance of logic for mathematics and the role logic plays (or should play) in mathematics. This conviction flowed

[1] The library of Warsaw University contains numerous Sleszyński's scripts, including his lecture notes and excerpts from his reading matters.

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6_5

from his discernment of gaps in mathematical proofs. In his paper 'O znaczeniu logiki dla matematyki' [On the Significance of Logic for Mathematics] he wrote:

> The content of mathematics is wonderful but its form leaves a lot to be desired. I am uttering these words thoughtfully in order to contrast them with the hymns praising science in general and mathematics in particular (1923, p. 39).[2]

Yet, the situation is not—in Sleszyński's opinion—better than the one in other sciences. On the other hand, he stated clearly that 'all that is dark and complicated is of no value' and 'seeking clarity and simplicity should guide all investigations in this domain' (1925–1929, vol. 2, p. 212).[3] He saw the remedy in logic, which in his opinion was the only tool that could 'weed the scientific field and through analysis it could shorten and condense this huge material given by scientific works. [Since] logic is the only means to revive scientists, showing them the norms of importance of a given theme and the norms of maturity of their works' (1925–1929, vol. 1, p. 4).[4] That is why, as Antoni Maria Hoborski writes:

> He was not one of those immersed in their activities who were not interested in their students, regarding lectures as burdens; he did not belong to those who being in steady contacts with their students gather their future disciples and thus build a school of mathematics and create a scientific centre.[5] Prof. Sleszyński chose a completely different and in fact, quite original, task: in his lectures on the elements of analysis or the theory of determinants, or probability calculus, etc., he desired to explain all difficulties of logical and mathematical nature, stuck in the elements of these sciences and usually ignored by mathematicians; he wanted to bridge the gaps in the occasionally classical reasonings so that the mathematical reasonings, he gave, were of 'complete' character (1934, p. 74).[6]

As he wrote in 'O znaczeniu logiki dla matematyki' the sources of the ambiguity in mathematical works should be sought in:

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[2] 'Treść matematyki jest wspaniała, lecz forma jej pozostawia dużo do życzenia. Te słowa wypowiadam z rozmysłem, aby je przeciwstawić hymnom wygłaszanym na cześć nauki w ogóle, a matematyki w szczególności.'

[3] 'wszystko, co jest ciemne i skomplikowane, jest bez wartości'; 'poszukiwanie jasności i prostoty powinno być myślą przewodnią wszelkich badań w tym zakresie.'

[4] 'wyplenić chwasty na polu naukowym i drogą analizy skracać i kondensować ten przeogromny materiał, który daje twórczość naukowa. Logika [bowiem] jest jedynym środkiem otrzeźwiającym badacza, wskazującym mu normy ważności tematu i dojrzałości jego pracy.'

[5] In fact, Sleszyński had few disciples. They included Stanisław Krystyn Zaremba (1903–1990) (son of Stanisław Zaremba mentioned in Sect. 5.2 of this chapter) and Wacław Borejko. He also influenced the following: Antoni Maria Hoborski (1879−1940), Leon Chwistek (1884−1944), Tadeusz Ważewski (1896−1972) and Stefan Rozental (1903−1944?) [footnote is mine].

[6] 'Nie był jednym z tych, którzy pogrążeni w swojej twórczej pracy, nie interesują się swymi studentami, a wykład uważają za balast; nie należał również do tych, którzy w stałym kontakcie ze studentami gromadzą koło siebie przyszłych pracowników naukowych, budują tym samym szkołę matematyczną i tworzą ośrodek naukowy. Prof. Śleszyński nakreślił sobie zadanie zupełnie odmienne i wręcz oryginalne: pragnął w swych wykładach elementów, czy to analizy, czy teorii wyznaczników, czy też rachunku prawdopodobieństwa itd. wyjaśnić wszelkie trudności natury logicznej i matematycznej, tkwiące właśnie w elementach tych nauk, zwykle nie dostrzeganych przez matematyków; pragnął zapełnić luki w klasycznych niekiedy rozumowaniach, by wywody matematyczne, które podawał, miały charakter rozumowań "zupełnych".'

1) immense complexity and difficulties of mathematical investigations and
2) immense difficulty of presenting the results of these investigations (1923, p. 41).[7]

The first depends on 'the imprecision of research methods' and 'the essence of things under investigation, i.e. mathematical truths' (1923, p. 45).[8] The other consists in including as proofs these reasonings 'that are proofs of some other theorems, interwoven into the given proof, but formulated in an imprecise way' (1923, p. 46),[9] in including as proofs the unproven themes and misusing proofs by contradiction. Moreover, one should not forget that authors (especially the older ones) often omit proofs on purpose[10] or deform them. Distracting the object of mathematical investigations from sensual experience can also cause ambiguity.[11] At the same time, Sleszyński clearly distinguishes between 'ready science' and its 'creation' (cf. 1925–1929, vol. 1, p. 149), i.e. the context of discovery and the context of justification in mathematics. In the aforementioned article 'O znaczeniu logiki dla matematyki' he writes:

> The discovery of mathematical truths is usually done by intuition, with the help of creative fantasy, and cannot be determined by any definite rules (1923, p. 44).[12]

This confidence in intuition, connected with 'neglecting logical culture' (1923, p. 46),[13] is another source of difficulty and ambiguity.[14] Then one can check the 'speculations' with the help of 'exact proofs' (*ibid.*).

Sleszyński's solution to these troubles was giving complete proofs, i.e. proofs consisting of:

> [. . .] parts (links), each of them being an implementation of some previously proven or accepted theorem on the basis of *modus ponens*, i.e. if p implies q, and p is true, then q is also true (1923, p. 49).[15]

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[7] '1) ogromnej zawiłości i trudności badań matematycznych i
2) ogromnej trudności wykładu wyników tych badań.'

[8] 'niedokładności metod badania'; 'istoty rzeczy badanych, tj. od prawd matematycznych.'

[9] 'które są dowodami pewnych innych twierdzeń, wplecionych do danego dowodu, ale niesformułowanych dokładnie.'

[10] This is what, for example, Descartes did in *La géométrie* [The Geometry]: he omitted proofs, replacing them with such remarks as: 'I hope that posterity will judge me kindly, not only as to the things which I have explained, but also to those which I have intentionally omitted so as to leave to others the pleasure of discovery.'

[11] Sleszyński illustrates his remarks (1923) by giving large quotations from Descartes, Leibniz, Jocobi, Eisenstein, Gauss and Galois.

[12] 'Odkrycie prawd matematycznych odbywa się przeważnie drogą intuicji przy pomocy fantazji twórczej i nie daje się ująć w żadne określone prawidła.'

[13] 'zaniedbaniem kultury logicznej.'

[14] Some considerations of the sources of ambiguity in mathematics can be also found in Sleszyński's work 'Sur le raisonnement dans les sciences deductives' [On a Reasining in Deductive Sciences] (1921b). In this work he formulates similar theses.

[15] '[. . .] części (ogniw), z których każde jest zastosowaniem jakiegoś poprzednio dowiedzionego albo przyjętego twierdzenia na podstawie *modus ponens*, tj. na podstawie tego, że jeżeli z p wynika q, i p jest prawdą, to i q jest prawdą.'

However, complete proofs are very long and thus illegible. Therefore, mathematicians use only the abbreviations of proofs. In order to avoid problems and errors the abbreviations should be used carefully and in accordance with certain principles. Consequently, Sleszyński postulates to conduct a logical analysis of mathematical theories and find out methods of shortening the conclusive procedure. At this point logic proves to be a tool. Yet, traditional logic is not sufficient:

> [. . .] this logic is not sufficient for future mathematics. If we are to deal with the foundations of mathematics we need a new logic, its first outline being included in *Principia Mathematica*. Since in these investigations all theorems are obvious, and the idea is that a logical relationship between the theorems should be established (1921a, p. 11).[16]

Therefore, logic is indispensable to mathematics. Its role and connection with mathematics were analysed in the conclusions to 'O znaczeniu logiki dla matematyki':

> The previous considerations lead to an answer to the question concerning the significance of logic for mathematics. Without complete proofs serious investigations concerning the foundations of mathematics are just impossible. As a matter of fact, the abbreviations of proofs are sufficient for other mathematical investigations but proofs that would give certainty cannot be constructed without the exact knowledge of proof theory.
>
> Thus we can see that logic as proof theory is necessary to take mathematics from its present sad—in my opinion—condition to a state where its form, which has been neglected so far, would correspond to its splendour and sublime content. Only then mathematics would exert a decisive influence on scientific thinking in general! The wonder of imaginativeness of fully free mathematical fantasy and the statuesque beauty of mathematical truths will not be darkened with the logical maze of its previous form and will become the sources of the greatest delight for wide circles of the thinking men (1923, p. 52).[17]

Sleszyński realised this task through his talks. From the academic year 1914/1915 till 1920 he lectured on the methodology of mathematics and mathematical logic, and in the years 1921–1924 he lectured on his 'Teorja dowodu' [The Theory of Proof]. The latter led to the origin of the book bearing the same title (1925–1929). It is a two volume textbook published by his disciples: edited by Stanisław

[16] '[. . .] ta logika nie wystarcza dla matematyki przyszłości. Jeśli mianowicie będziemy się zajmowali badaniem podstaw matematyki, to do tego potrzebna jest logika nowa, której pierwsze zarysy znajdujemy w *Principia Mathematica*. W tych badaniach bowiem wszystkie twierdzenia są oczywiste, a chodzi o to, aby ustanowić związek logiczny między twierdzeniami.'

[17] 'Z poprzednich rozważań wypływa odpowiedź na pytanie o znaczenie logiki dla matematyki. Bez dowodów zupełnych poważne badania nad podstawami matematyki są właściwie niemożliwe. Dla innych badań matematycznych wystarczą, co prawda skróty dowodów, ale takie skróty, które dawałyby pewność, nie mogą być zbudowane bez dokładnej znajomości teorii dowodu.

Widzimy więc, że logika jako teoria dowodu, jest konieczna dla wyprowadzenia matematyki z jej obecnego smutnego, moim zdaniem, stanu na drogę, gdzie forma jej, zaniedbywana dotychczas, odpowiadałaby jej wspaniałości i wzniosłej treści. Wtedy dopiero znajdzie matematyka jak najszersze rozpowszechnienie, a potężna metoda matematycznego myślenia wywrze decydujący wpływ na myślenie naukowe w ogólności! Cudowność polotu całkiem swobodnej fantazji matematycznej i posągowa piękność prawd matematycznych nie będą omraczane gmatwaniną logiczną jej dotychczasowej formy i staną się źródłem najwyższej rozkoszy dla szerokich kół ludzi myśli.'

Krystyn Zaremba and Ryszard M. Wasserberger, and published by the Mathematical-Physical Circle at the Jagiellonian University (financed by the Ministry of Religious Denominations and Public Enlightenment). The textbook was prepared on the basis of students' notes and Sleszyński's notes. In fact, he himself was giving content-related consultations all the time.

Teorja dowodu presents the history of logic and is a modern exposition of the discipline in question. Volume One begins with general considerations on the concept of the deductive system as a set of propositions, out of which some propositions were accepted without proof (these are axioms and definitions), and all the remaining ones should be deduced. Then Sleszyński presents an outline of the history of logic (first of all, formal logic) against a background of the development of mathematics. He writes about Zeno of Elea and his paradoxes (he stresses both their historical significance and validity), about the Sophists as well as Gorgias, Protagoras, Socrates, Plato, Aristotle, Peter of Spain, Bacon, Ockham, Descartes, Leibniz, Bolzano and Mill. The final methodological part contains an analysis of the concept of deductive proof in mathematics. Volume Two discusses the contributions of Leibniz, Boole, Jevons, Hermann Günther and Robert Grassman, Schröder, Porecki, and then Peano, Burali-Forti, Russell and Whitehead in the development of mathematical logic. He emphasises the transition from logical calculus solving logical tasks to calculus as a tool to justify theorems. Then he gives an exposition of propositional calculus, containing the proofs of 250 laws (44 of them were formulated by Stanisław Krystyn Zaremba) in the system based on 11 axioms as well as the rules of substitution and *modus ponens*. The author shows how the laws of logic can be used to construct completely new mathematical theorems—he did that through analysing various examples.

Sleszyński's contribution was the formulation of a programme of reconstruction of the real course of mathematical proofs, which he attempted to realise. His attempt was continued in the form of a system of natural deduction by Stanisław Jaśkowski. Additionally, his departure from psychologism and epistemological traces should be stressed. Sleszyński treated the deductive system as hypothetical. He emphasised that mathematical theorems are 'conditional theorems, their content being the relation between the antecedent and the consequent' (1912, p. 119a).[18]

Stressing the role and significance of formalization and symbols in the context of justification, Sleszyński warned against what Kazimierz Twardowski would call (1927) 'symbolmania and pragmatophobia.' Symbols only serve to simplify proofs and make them clearer, and in logic symbols facilitate the analysis and exact formulation of the so-called primary laws of thinking (cf. 1913, p. 23a).

Dealing with the methodology of mathematics Sleszyński did not actually conduct any reflections of *sensu stricto* ontological or epistemological nature. However, he was an antifictionist, i.e. he regarded the introduction of fiction, in

[18] 'twierdzeniami warunkowymi, których treścią jest związek między poprzednikiem a następnikiem.'

particular contradictory fictions, into science as destructive, especially to mathematics. He wrote sneeringly:

> Contradictory fictions occur in mathematics only where mathematical concepts have not been carefully examined (1914, p. 199b).[19]

From the point of view of our interest, i.e. the development of the philosophy of mathematics, Sleszyński's most important achievement was his conviction of the significance and meaning of logic and formal methods for mathematics and its methodology. Thus Sleszyński falls in line with the important and essential trend that was prevailing in Poland. Nonetheless, it should be stressed that he ascribed to logic the role of an ancillary science with regard to mathematics and he did not treat it as an independent and autonomous discipline. Consequently, he considered it only from the perspective of its application in mathematics. The same standpoint was taken by Stanisław Zaremba, who was also connected with Cracow (cf. Sect. 5.2 of this chapter), and who spoke about logic in mathematics, treating it as *ancilla mathematicae*. This stands in apparent contrast to the attitude prevailing in the Warsaw School of Logic that treated logic as an independent and autonomous discipline lying at the foundations and methodology of mathematics (cf. Chap. 3).

5.2 Stanisław Zaremba

Considering Stanisław Zaremba's philosophical views on mathematics, let us begin by stating that he belonged to the Cracow group that was interested in modern mathematical logic. In some way, he was really a pioneer in this domain. Although he himself did not work creatively in this field he appreciated the role of logic in mathematics, which we are going to discuss further. Together with Sleszyński (cf. the previous section) he aroused the interest in logic and the philosophy of mathematics of the young Cracow mathematicians, particularly Witold Wilkosz (cf. Sect. 5.3 of this chapter).

Zaremba was not so much interested in logic as such but in the foundations of mathematics. One can surely see the influence of his studies at the Sorbonne, where he received his doctoral degree in 1899—his attitude towards logic and his views on its role in mathematics were consistent with the standpoints of most French mathematicians. Zaremba wrote several important books in which he presented the logical structure of mathematics, cf. for example *Arytmetyka teoretyczna* [Theoretical Arithmetic] (1912), *Zarys pierwszych zasad teorii liczb całkowitych* [An Outline of First Principles of the Theory of Integers] (1907), *Wstęp do analizy* [Introduction to Analysis] (1915), or finally *La logique des mathématiques* [The Logic of Mathematics] (1926). They were of methodological character. We will

[19] 'Fikcje sprzeczne ukazują się w matematyce tylko tam, gdzie pojęcia matematyczne nie zostały dokładnie zbadane.'

come back to some of them since they are good illustrations of Zaremba's conception of logic and its role in mathematics. Suffice it to say that although Zaremba was interested in mathematical logic it was not him but Sleszyński who introduced it as an academic discipline at the Jagiellonian University.

Zaremba was a versatile mathematician. He worked mainly in the domain of mathematical analysis, more precisely, in partial differential equations of second order. His interests might have resulted from his conviction that mathematics should not be an aim in itself; that the ultimate goal of mathematics was its implementations, especially in natural sciences. However, he appreciated the significance and beauty of pure mathematics—for him it was a source of new tools and methods for theoretical investigations—hence his interest in mathematical physics and the role of mathematics in physics. He also dealt with philosophical problems and the methodology of mathematics.

Zaremba saw the relationships between research in mathematics and philosophical issues, which could be testified by his paper 'Pogląd na te kierunki w badaniach matematycznych, które mają znaczenie teoretyczno-poznawcze' [View on These Directions in Mathematical Investigations That Have an Epistemological Meaning] (1911). The paper concerns his investigations on the foundations of geometry and set theory, which in his opinion 'border on the theory of knowledge, and even to some extent, belong to this branch of philosophical inquiries' (1911, p. 217).[20]

Analysing the independence of Euclid's fifth postulate—the parallel postulate—and the creation of non-Euclidean geometries, Zaremba distinguishes subtly between the question of provability or non-provability of certain theorems on the basis of the accepted axioms and the question of the truth of these theorems:

> On the basis of these remarks it would seem that the question of the logical independence of Euclid's postulate from other axioms in geometry merges with the question whether Euclid's postulate expresses truth or error. But it is not so in reality (1911, p. 219).[21]

Reflecting on geometry Zaremba reaches the following conclusions:

> The significance of epistemological investigations on the foundations of geometry can be, I think, presented in the following way:
>
> 1. These investigations instruct us that we can create a very general concept, namely the concept of n-dimensional extension that embraces the concept of space as a special case of tree-dimensional extension.
> 2. On the occasion of the investigations on the foundations of geometry, the general concept of logical independence of certain theorems from other theorems was developed.
> 3. Since the investigations on the foundations of geometry led to fixing the list [system—remark is mine] of logically independent statements [theorems—remark is mine], from which the whole classical geometry results, on a purely deductive way, the indispensible

[20] 'graniczą z teorią poznania, a nawet w pewnej mierze należą do tej gałęzi dociekań filozoficznych.'

[21] 'Na podstawie tych uwag mogłoby się wydawać, że kwestia niezależności logicznej postulatu Euklidesa od innych aksjomatów geometrii zlewa się z pytaniem, czy postulat Euklidesa wyraża prawdę, czy też błąd. W rzeczywistości jednak tak nie jest.'

> foundations to examine closely the psychological nature of the concept of space were achieved (1911, p. 220). [22]

According to Zaremba the last problem leads to the question about the truth of geometrical axioms. However, mathematicians have very different views concerning this area.

The other domain of mathematics, which Zaremba analysed and the results of which influence philosophy, especially the theory of knowledge, is set theory. The latter is essential since the concept of infinity, more precisely, 'one of the many forms in which the concept of infinity occurs in mathematical analysis' (1911, p. 221)[23] was examined closely within it. This achievement:

> deserves philosophers' special attention for the reason that from such a vague concept, which is the ordinary concept of infinity, it [set theory] managed to draw a whole series of concepts and theorems, which are not inferior to any other mathematical concepts with regard to precision and clarity (1911, p. 221).[24]

Let us repeat: Zaremba was interested in the applications of mathematics, especially in physics. He also considered these problems from the perspective of philosophy and methodology. He dedicated two papers to these problems: 'O stosunku wzajemnym fizyki i matematyki' [On the Mutual Relation Between Physics and Mathematics] (1923) placed in *Poradnik dla samouków* [A Guide for Autodidacts] and 'Uwagi o metodzie w matematyce i fizyce' [Remarks on the Method in Mathematics and Physics] (1938).

Zaremba treated the relations between physics and mathematics as 'extremely interesting from a philosophical standpoint' as well as 'necessary in order to gain a deeper understanding of both of these branches of human knowledge and the course of evolution of each of them' (1923, p. 131).[25] In his opinion the role of mathematics in physics can be reduced to:

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[22] 'Znaczenie teoriopoznawcze badań nad podstawami geometrii możemy, jak sądzę, przedstawić w sposób następujący:

1. Badania te pouczają nas, iż wytworzyć sobie możemy bardzo ogólne pojęcie, mianowicie pojęcie rozciągłości *n*-wymiarowej, która mieści w sobie, jako przypadek szczególny rozciągłości trójwymiarowej, pojęcie przestrzeni.
2. Przy sposobności badań nad podstawami geometrii rozwinęło się ogólne pojęcie niezależności logicznej pewnych twierdzeń od pewnych innych.
3. Przez to, iż badania nad podstawami geometrii doprowadziły do ustawienia wykazu [tzn. układu—uwaga moja, R.M.] logicznie niezależnych od siebie orzeczeń [czyli stwierdzeń—uwaga moja, R.M.], z których cała geometria klasyczna wynika już na drodze czysto dedukcyjnej, uzyskane zostały niezbędne podstawy do bliższego zbadania natury psychologicznej pojęcia przestrzeni.'

[23] 'jedna z wielu postaci, w której pojęcie nieskończoności występuje w analizie matematycznej'

[24] 'zasługuje na szczególną uwagę filozofa z tej przyczyny, iż z pojęcia tak mętnego, jakim jest pospolite pojęcie nieskończoności, zdołała wysnuć cały szereg pojęć i twierdzeń, które pod względem precyzji i jasności nie ustępują żadnym innym pojęciom matematycznym.'

[25] 'nadzwyczaj zajmujące ze stanowiska filozoficznego'; 'konieczne do głębszego zrozumienia obu tych gałęzi wiedzy ludzkiej i biegu ewolucji każdej z nich.'

> The very formulation of the majority of the most important physical hypotheses indispensably requires using mathematics. [. . .]
>
> With time the theories of physics increasingly assume the character of mathematical theories, and at present mathematics is the main tool that physics uses to realise its logical reasonings; thus mathematics constitutes the major means that physics uses to reach its aims as far as possible. [. . .]
>
> The history of physics teaches us that mathematical deduction sometimes leads to discovering new phenomena the possibilities of which could not have just been foreseen otherwise, and which only *a posteriori* have been realised experimentally. [. . .]
>
> Between certain classes of physical phenomena, which seemingly have nothing in common, is a strict relation made evident that one and the same mathematical theory, depending on the meaning given to its symbols, can be a theory of the phenomena of any of these classes (1923, pp. 148–150).[26]

In Zaremba's opinion differential calculus and integral calculus are convenient tools allowing us to formulate hypotheses. However, mathematics did not construct any adequate tool 'to effect logical reasonings' and 'the tasks that physics imposes on mathematics may be connected with the difficulties that now mathematics cannot solve in a satisfying way' (1923, p. 150).[27]

Yet, the dependencies between mathematics and physics are not one-sided—mathematics owes a lot to physics, too. Examples can be taken from the issues related to string theory or problems related to heat theory, which stimulated investigations on partial differential equations.

Zaremba repeated the above-mentioned theses in his paper 'Uwagi o metodzie w matematyce i fizyce' [Remarks on the Method in Mathematics and Physics] (1938). He stressed that 'in mathematics, as well as in physics, deductive thinking, i.e. deductive reasoning plays a fundamental role' (1938, p. 31).[28] At the same time, he stressed that deductive reasoning did not allow us to confirm the truth of any proven theses, but it only 'allows us to say that if all the assumptions, accepted in the proof of theorem T, are true propositions, then theorem T is also true' (1938,

[26] 'Samo sformułowanie większości z najważniejszych hipotez fizyki nieodzownie wymaga posługiwania się matematyką. [. . .]

Z biegiem czasu teorie fizyki przybierają coraz bardziej charakter teorii matematycznych, a już w dobie obecnej matematyka jest głównym narzędziem, którym fizyka posługuje się do urzeczywistnienia swoich wywodów logicznych; wobec tego matematyka stanowi główny środek, którym posługuje się fizyka do dopięcia w miarę możności swych celów. [. . .]

Historia fizyki poucza, że dedukcja matematyczna doprowadza niekiedy do odkrycia zjawisk nowych, których możliwości zgoła skądinąd nie przewidywano, a które dopiero a posteriori zostawały eksperymentalnie urzeczywistnione. [. . .]

Pomiędzy pewnymi klasami zjawisk fizycznych, pozornie nic wspólnego nie mającymi, zachodzi ścisły związek ujawniający się w tym, że jedna i ta sama teoria matematyczna, zależnie od znaczenia nadanego jej symbolom, przedstawiać może teorię zjawisk którejkolwiek z tych klas.'

[27] 'uskuteczniania wywodów logicznych'; 'zagadnienia nastręczane matematyce przez fizykę, połączone bywają z trudnościami, którym obecnie matematyka nie może jeszcze podołać w sposób zadowalający.'

[28] 'w matematyce, jak i w fizyce rozumowanie dedukcyjne, czyli, dokładniej mówiąc, dowodzenie dedukcyjne odgrywa pierwszorzędną rolę.'

p. 32).[29] He also paid attention to the fact that the postulates of the deductive theory could be interpreted diversely. Consequently, such theories did not concern any concrete objects. Speaking of the influence of physics on the evolution of mathematics, he compared it 'to the teacher's influence on the development of his pupil if the former was only satisfied with giving the pupil well-thought questions together with giving him some hints to the answers from time to time' (1938, pp. 33–34).[30] He also quoted the words of Joseph Fourier, 'one of the major founders of mathematical physics' (1938, p. 35), who wrote:

> Profound study of nature is the most fertile source of mathematical discoveries. Not only has this study, in offering a determinate object to investigation, the advantage of excluding vague questions and calculations without issue; it is besides a sure method of forming analysis itself, and of discovering the elements which it concerns us to know, and which natural science ought always to preserve: these are the fundamental elements which are reproduced in all natural effects[31] (Fourier 1978, p. 7).

Zaremba summaries his arguments:

> The role of mathematics in the development of physics can be described in many words: accepting as postulates the hypotheses, imposed by observation in relation to certain class of phenomena, we turn to mathematics to get to know the logical consequences of the above-mentioned postulates; if the facts confirm (with a satisfactory degree of precision) these results, the theory, which has been developed, coordinates the phenomena of the discussed class; otherwise, this theory is a proof that the system of postulates, from which we have started, should be modified; the last circumstance can also occur without turning to the facts; if it is to happen, it is needed and sufficient that while developing the consequences of the accepted postulates they turn out to be mutually contradictory (1938, p. 36).[32]

[29] 'powiedzenia, że jeżeli wszystkie założenia, przyjęte w dowodzie twierdzenia T, są zdaniami słusznymi, to twierdzenie T jest także zdaniem słusznym.'

[30] 'wpływu nauczyciela na rozwój swojego ucznia, gdyby tenże poprzestawał niemal wyłącznie na stawianiu uczniowi rozumnie obmyślanych pytań przy podawaniu niekiedy niektórych wskazówek w odniesieniu do odpowiedzi.'

[31] 'L'étude approfondie de la nature est la source la plus féconde des découvertes mathématiques. Non seulement cette étude, en offrant aux recherches un but déterminé, a l'avantage d'exclure des questions vagues et les calculs sans issue: elle est encore un moyen assuré de former l'Analyse elle-même, et d'un découvrir les éléments qu'il nous importe le plus de connaître, et que cette science doit toujours conserver: ces éléments fondamentaux sont ceux qui se reproduisent dans tous les effets naturels' (Fourier 1888, vol. 1, p. XXIII).

[32] 'Rola matematyki w rozwoju fizyki określona być może w niewielu wyrazach: przyjąwszy za postulaty hipotezy, nastręczane przez obserwację i doświadczenie w odniesieniu do pewnej klasy zjawisk, zwracamy się do matematyki, żeby poznać następstwa logiczne powyższych postulatów; jeżeli fakty potwierdzą (z dostatecznym stopniem dokładności) powyższe wyniki, to teoria, która została rozwinięta koordynuje zjawiska rozważanej klasy; w razie przeciwnym powyższa teoria stanowi dowód na to, że układ postulatów, z którego się wyszło, winien być zmodyfikowany; ta ostatnia okoliczność może się objawić i bez zwracania się do faktów; żeby się to wydarzyło potrzeba i wystarcza, ażeby, przy rozwijaniu następstw przyjętych postulatów, okazało się, że rzeczone postulaty są sprzeczne ze sobą.'

Therefore, according to Zaremba mathematics plays an ancillary role to physics—it gives physics formal tools, and on the other hand, it develops itself thanks to solving the problems given by physics.

We have already mentioned Zaremba's interest in mathematical logic. His views on the place and role of logic were parallel to those of French mathematicians. Zaremba treated logic as an ancillary discipline towards the classical branches of mathematics, and first of all, as a tool of mathematical didactics. Consequently, he was not interested in the theoretical problems of logic itself. He wrote about himself:

> My deep conviction that in mathematical sciences one should aim at the highest precision and exactness made me deal with logic (Szarski 1962, p. 26).[33]

In the dissertation *La logique des mathématiques* [The Logic of Mathematics] (1926) Zaremba expresses his conviction emphatically, writing in the introduction:

> Let us add that theoretical logic is also called to precious service to the work of co-ordination and simplification of mathematical theories, work that makes the excellent development of these theories even more necessary. [34]

Moreover, he presented clearly and simply these branches of mathematical logic that in his opinion gave indispensible background for the studies of various domains of science. He also explained several logical paradoxes.

With strong conviction, one can say that Zaremba represented the view that logic should be 'in mathematics' (*en mathématique*) and be *ancilla mathematicae*—which to some extent is reflected in the title of his work *La logique des mathé matiques*. Such a conception of the place and role of logic was expressed in the polemic that began with respect to his book *Arytmetyka teoretyczna* [Theoretical Arithmetic] (1912) and the methodological analysis, which Jan Łukasiewicz conducted to examine this work. The discussion has been described in Sect. 3.2 of Chap. 3. Let us only mention that the polemic did not concern—as one could guess from the title of Łukasiewicz's paper 'O pojęciu wielkości. (Z powodu dzieła Stanisława Zaremby)' [About the Concept of Quantity. (Because of Stanisław Zaremba's Work)] (1916)—the definition of the concept of quantity but was of a more profound nature. It concerns the programme. The polemic showed perfectly the differences in understanding the role of logic in mathematics and the question of the relationship between these two sciences. The differences in the standpoints represented by Zaremba and Łukasiewicz were perceived well by the Russian mathematician Nikolai N. Luzin in his letter of 30 September 1926 to Arnaud Denjoy, writing:

[33] 'Głębokie przekonanie, że w naukach matematycznych należy dążyć do maksimum precyzji i ścisłości skłoniło mnie do zajęcia się logiką.'

[34] 'Ajoutons que la logique théorique est appelée encore à rendre de précieux services dans l'œuvre de coordination et de simplification des théories mathématiques, œuvre que le développement considérable des ces théories rend de plus en plus nécessaire.'

> It seems to me that mathematical life in Poland has two quite different sides: one aims at the classical branches of mathematics and the other—set theory (function). In Poland these tendencies are mutually exclusive; they are very hostile, and are waging a fierce battle. Both parties are very energetic but as it seems to me their forces are not equal. [. . .] The classical party is represented only by the old [. . .] Cracow university. [. . .] Among the Polish mathematicians the most inflexible follower of this way is Professor Zaremba. The other adherents of this trend followed Mr Zaremba closely. [. . .] However, the classical tendency has been abandoned in many cities [. . .] where it has been replaced by the tendency held by the school of Mr Sierpiński's (Luzin 1983, p. 66).[35]

In fact, Zaremba was rather alone in his views,[36] which is suggested by the course of the discussion in *Przegląd Filozoficzny*, presented in Sect. 3.2 of Chap. 3. The Warsaw mathematical environment accepted Łukasiewicz's arguments concerning the place and role of logic towards mathematics, i.e. mathematical logic is not a peripheral discipline in relation to mathematics (as claimed by Zaremba), but on the contrary, it belongs to the centre of mathematics and additionally, it should be treated as an autonomous discipline. This view corresponded to the programme of the Warsaw School of Mathematics, which stressed set theory, the foundations of mathematics and mathematical logic (cf. Sect. 2.1 of Chap. 2).

5.3 Witold Wilkosz

Being influenced by Jan Sleszyński several young Cracow's mathematicians, including Witold Wilkosz,[37] became interested in logic. Although Wilkosz did not write any work or article on the philosophy of mathematics, he made numerous remarks of philosophical nature. His ideas are not revolutionary and do not constitute a cohesive system, but they show the prevailing philosophical tendencies and

[35] 'Wydaje mi się, że życie matematyczne w Polsce toczy się dwiema całkiem różnymi drogami: jedna z nich ciąży ku klasycznym działom matematyki, druga zaś ku teorii mnogości (funkcji). Tendencje te w Polsce wykluczają się nawzajem, są sobie bardzo wrogie i obecnie trwa między nimi zacięta walka. Obydwie strony są bardzo energiczne, lecz jak mi się wydaje, siły ich są nierówne. [. . .] Stronę klasyczną reprezentuje obecnie tylko stary [. . .] uniwersytet krakowski. [. . .] Spośród matematyków polskich najbardziej nieugiętym zwolennikiem tej drogi jest p. profesor Zaremba. Inni zwolennicy tej drogi trzymają się blisko p. Zaremby. [. . .] Jednakże tendencja klasyczna zakończyła się w wielu miastach [. . .], gdzie zastąpiła ją tendencja szkoły p. Sierpińskiego.'

[36] Let us add that the authority, Zaremba enjoyed in the Cracow's environment (although not only there—he was a mathematician of international renown and unquestioned authority all over Poland), caused that his views concerning the role and place of logic with reference to mathematics had consequences on the institutional level and influenced the situation of logic in Cracow. Although around the year 1918, Cracow was a much more developed centre of mathematical logic than Warsaw, it was Warsaw that created a school of logic. The power of tradition is also testified by the fact that until today there has been no chair and department of logic at the Faculty of Mathematics of the Jagiellonian University, which is rather an exception at Polish universities.

[37] In this context one should also mention Antoni Maria Hoborski (1879–1940) and Otto Nikodym (1887–1974).

sympathies in Poland of those days. They also testify to the Polish mathematicians' interests in philosophical questions. We are going to present some of them.

Let us begin with the booklet *Liczę i myślę. Jak powstała liczba* [I Count and Think. How the Number Originated] (1938a). Discussing the origin of the concept of number, Wilkosz opts for the logical-set theoretic conception (contradictory to the intuitionist conception referring to the primary intuition of number), according to which number and arithmetic, which is related to it, come from more general intuitions connected with equipollency. He gives numerous examples of psychological-ethnological nature that are to support and illustrate this conception. According to Wilkosz one can distinguish three stages in the process of the formation of the concept of number: (1) the ability of confirming practically the equipollency of sets of object, (2) the idea of a concrete, physical 'substitute' set and finally, (3) the mentalisation or interiorisation of this substitutive set. This led to the creation of the idea of a chain used to counting—an example of such a chain is the sequence of natural numbers (0), 1, 2, 3, etc.[38]

Wilkosz focuses on the problems important to the methodology of mathematics and generally, to formal sciences, in his article 'Znaczenie logiki matematycznej dla matematyki i innych nauk ścisłych' [The Significance of Mathematical Logic for Mathematics and Other Exact Sciences] (1936a).[39] Recognising logic as 'an indispensible element of every deductive system' he states that 'deductive logic, i.e. mathematical logic, is *only a tool* [emphasis is mine] to develop the primary concepts of the system through definitions and its primary propositions through argumentation' (1936a, pp. 343–344).[40] Logic is connected with deductive systems and axiomatic method. The method itself was introduced by the ancient Greeks—Plato was the first person who invented it whereas in *The Elements* Euclid showed how it could and should be applied in mathematics. However, the logical element of this method was ignored since the commonsense methods of deduction (Aristotle's syllogistic was not especially helpful here) were used instead. Only the nineteenth century brought about some change and development of investigations on logic itself. As Wilkosz writes 'in practice we encounter almost exclusively deductive systems in mathematics and for the needs of mathematics' (1936a, p. 344).[41] Consequently, logic itself assumed the form of a deductive system in which—using Wilkosz's words—'"logic" is replaced by the system of "directives".'[42] Wilkosz also stresses that:

[38] '0' was written in brackets since it played a specific role among the natural numbers, evolving from the symbol of an empty place in the positional notation to the status of a rightful number.

[39] It is the author's abstract from his talk.

[40] 'nieodzowny składnik każdego systemu dedukcyjnego'; 'logika dedukcyjna, a więc matematyczna, to *tylko narzędzie* [podkr. moje—R.M.] rozbudowy pierwotnych pojęć systemu drogą określeń i pierwotnych jego zdań drogą dowodzeń.'

[41] 'w praktyce spotykamy się niemal wyłącznie z systemami dedukcyjnymi w matematyce i dla potrzeb matematyki.'

[42] '"logikę" zastępuje system "dyrektyw".'

> Methodologically, formal logic assumed a certain slightly one-sided form. The elimination of the language of general names for the benefit of classes or propositional conditions, the neglect of the technique of epicherematic concluding, the eradication of the difference between the aspects of conditionals, known from grammar, are theoretically quite permissible. But are they beneficial in practice? 1936a, p. 346).[43]

The successes of mathematics, thanks to the axiomatic method, led to many attempts of its application in other sciences. According to Wilkosz the evaluation of these attempts is 'very critical':

> The axiomatic method has its natural place where the major hypotheses of a given, concrete science are worth being regarded, even for some time, as not requiring any changes. Then translating them into a language that can undergo logical processes and yielding them to the deductive methodology can result in benefits, and the theory will have a sufficient amount of time to apply its laborious argumentation with absolute precision (1936a, p. 345).[44]

Nonetheless, very few sciences follow this way. Even theoretical physics does not apply it completely. One of the reasons may be that 'many a time axiomatisation would force us to the excessive simplification of the assumptions and narrowing the topic' (1936a, p. 345).[45] On the other hand, Wilkosz thought that 'it is time to apply today's precision in such sciences as economics, sociology, not mentioning philology[46] or history' (1936a, p. 345).[47] Wilkosz also mentioned the possibility of its application in theology.[48]

The aforementioned deficiencies of formal logic, i.e. its certain incompatibility with the 'natural' ways of inference and the form of the deductive system as well as the fact that 'its present shape and the convenience of procedures according to its requirements do not look great,' cause that formal logic 'very much frightens, for

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[43] 'Metodologicznie logika formalna przybrała pewną postać nieco jednostronną. Wyrugowanie języka imion ogólnych na rzecz języka klas lub warunków zdaniowych, zarzucenie techniki konkludowania epicherematycznego, zatracenie różnicy między znanymi z gramatyki odcieniami zdań warunkowych jest teoretycznie najzupełniej dopuszczalne. Ale czy korzystne w praktyce?'

[44] 'Metoda aksjomatyczna ma swe naturalne miejsce tam, gdzie główne hipotezy danej konkretnej nauki opłaca się uważać, choćby na jakiś czas, za niewymagające zmian. Wtedy przełożenie ich na język poddający się procesom logicznym i wtłoczenie ich w metodykę dedukcyjną może przynieść korzyści, a teoria dysponować będzie dostateczną ilością czasu, by swe żmudne dowodzenie przeprowadzić z całą precyzją.'

[45] 'aksjomatyzacja zmuszałaby niejednokrotnie do nadmiernej symplifikacji założeń i zwężeń tematu.'

[46] It is worth mentioning Wilkosz's interests in the Middle Eastern philology and languages. After his graduation Wilkosz received a grant and studied at the University of Beirut for several months. After having returned to Poland he enrolled in the programme of classical philology and Eastern languages at the Jagiellonian University, giving it up after 2 years and enrolling in mathematics [the footnote is mine].

[47] 'przyszedł już czas na stosowanie dzisiejszej precyzji w naukach takich jak ekonomia, socjologia—nie mówiąc już o filologii czy historii.'

[48] Wilkosz might have thought of the works undertaken by the so-called Cracow Circle (Bocheński, Drewnowski, Salamucha), which tried to use logical-axiomatic methods in philosophical and theological problems (cf. Sect. 3.9, Chap. 3).

example mathematicians, who do not deal with logic professionally, out of using it at full length' (1936a, p. 346).[49]

At this point, Wilkosz's accurate remarks are worth our attention. In fact, mathematical logic and the axiomatic method rather seem to be able to reconstruct the truly developed mathematics, but they do not give an adequate picture of how mathematics is really developed. Hence, today it is popular to develop intensively those trends of the philosophy of mathematics that stress mathematicians' research practice than its reconstruction as a scientific system in order to show its consistency (which was the fundamental aim of the classical trends, e.g. logicism, intuitionism or formalism).

At this point, we should discuss yet another work of Wilkosz containing philosophical issues, namely his paper 'O definicji przez abstrakcję' [On the Definition through Abstraction] (1938b). It is dedicated to the procedure, frequently used in science (particularly in mathematics), of introducing new objects (abstract ones) through abstracting from certain special characteristics and idealising those that are of special interest to us. Wilkosz aims at formulating a popular, and at the same time, exact conception of this method through the set-theoretic principle of abstraction and equivalence relation, simultaneously stressing the problems of ontological nature related to that.

We do not intend to discuss the principle of abstraction, which all those listening to lectures concerning logic and set theory know well, but we want to focus on philosophical questions. Wilkosz begins by accepting the non-existence of defining through abstraction (Wilkosz proposes the name 'defining abstracts' as a more adequate one) formulated by Giuseppe Peano in his work *Notions de Logique Mathématique* [Notation of Mathematical Logic] (Turin 1894). Peano wrote that 'new objects (logical)' in the form of φx, can be introduced in the case of an equivalence relation R, demanding that proposition aRb was always equivalent to $\varphi a = \varphi b$. It was Russell who asked what it meant that we introduced new objects, how we did it and by what virtue. He was not satisfied with Georg Cantor's answer that we do it 'thanks to our active ability of thinking.' He proposed to identify the abstract of an object a with the set of these elements x of the field of relation R, for which there is aRx. The way, although formally satisfactory, cannot be fully accepted in everyday language in which we create and use abstracts. Therefore, Wilkosz proposes other ways of solving these difficulties (of ontological nature). He suggests 'calling the abstract of an object the *class term* (common name) of all and only those objects that enter into a given relation with it' (1938b, p. 10).[50] One can also assume as the abstract of object a from the field of relation R 'a certain defined and distinguished, according to a separate principle, object from field R,

[49] 'postać jej dzisiejsza i poręczność procederów wedle jej wymogów nie przedstawia się świetnie'; 'niezmiernie odstrasza np. matematyków, nie zajmujących się zawodowo logiką, od posługiwania się nią w całej rozciągłości.'

[50] 'nazwać abstraktem przedmiotu *termin klasowy* (imię wspólne) wszystkich i tylko tych przedmiotów, które z nim wchodzą w dany stosunek.'

which is R-equivalent to element a (it may be the very a) under the condition that if aRb, then a and b will be ascribed the same object' (1938b, p. 11).[51] Wilkosz speaks here about the method through representation. Another possibility is not to stress what the abstract of $Abs_R a$ of the given element a with respect to the relation R is. We simply use it—as Russell says '*in use*.' Speaking more precisely: instead of defining what $Abs_R a$ is 'we define what the sentences spoken about $Abs_R a$ mean' (1938b, p. 11).[52]

It is worth paying attention to the fact that in the discussed paper Wilkosz, considering identity (especially the identity of abstracts), refers to the classical definition of this concept formulated by Leibniz: $a = b$ if and only if 'every predicate about a is simultaneously the predicate about object b, at the same time true or false with it' (1938b, p. 7).[53] Additionally, Wilkosz finds the source of this definition in St Thomas Aquinas who in *Summa Theologica* (questio XL, art. 1,3) wrote, 'Quaecumque sunt idem, ita se habent, quod quidquid praedicatur de uno, praedicatur et de alio.'

[51] 'pewien określony i oznaczony wedle osobnego przepisu *przedmiot z pola R*, który jest R-równoważny elementowi a (być może jest nim właśnie a) pod warunkiem, że o ile aRb, to a i b przypiszemy ten sam przedmiot.'

[52] 'określamy, co oznaczają zdania wypowiedziane o $Abs_R a$.'

[53] 'każde orzeczenie o przedmiocie a jest równocześnie orzeczeniem o przedmiocie b, jednocześnie z nim prawdziwym lub fałszywym.'

Conclusion

The analyses conducted in the previous chapters show that the Polish mathematicians and logicians of the pre-war period were interested in philosophical issues related to mathematics and logic. They knew very well the tendencies prevailing in the international science of those days; they were familiar with the literature and formulated various commentaries on logicism, intuitionalism or formalism. Furthermore, they conducted historical investigations on various problems of the philosophy of mathematics and logic—in this context the important and still valid work of Zbigniew Jordan *O matematycznych podstawach systemu Platona. Z historii racjonalizmu* [On the Mathematical Foundations of Plato's System. From the History of Rationalism] (1937) should be mentioned.[1]

Additionally, the Polish mathematicians and logicians of the pre-war period formulated their own philosophical conceptions concerning logic and mathematics. On the other hand—and this may be the characteristic of these Polish logicians and mathematicians—they thought that mathematical and logical investigations should not be limited by any *a priori* presumptions of philosophical nature. Mathematics and logic should be autonomous and neutral in relation to philosophy. Consequently, their statements pertaining to the philosophical aspects of mathematics or logic were rather fragmentary and incomplete, and they concern mainly the detailed questions concerning the problems under investigation. They did not aim at formulating a comprehensive conception of the philosophy of mathematics and logic. Their remarks were often just commentaries on concrete technical results from the foundations of mathematics or logic. Moreover, they did not always care for their consequences and the coherence of the formulated views with the research

[1] Jordan argued that 'Plato was the first philosopher of mathematics' and 'discoverer of the axiomatic method' as well as 'the first methodologist of the deductive system' (1937, p. 14). He also stressed that Plato was the first who 'paid attention to the importance of logical investigations on mathematics, the necessity of analysing the methods, which have been already applied, and of formulating new methods' (ibid., p. 14).

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6

practice. Consider for instance Tarski,[2] a follower of nominalism, who in his research work used freely and unrestrictedly the infinistic methods, which were far from being accepted by nominalism. The philosophical views represented by the Polish logicians and mathematicians were allegedly their private matters and should be suspended when they were working on concrete mathematical or logical problems. When they formulated certain theses of philosophical nature, they investigated them from various possible perspectives, avoiding opting for any of them and giving any definite and ultimate answers.

This attitude can be illustrated by the attitude of the Polish mathematicians and logicians towards the controversial axioms of set theory or the continuum hypothesis. They thought that solving the philosophical questions of the validity of the aforementioned conceptions should be obtained on the basis of the technical, strictly mathematical results indicating the consequences of such assumptions.

Therefore, no ideologies and strictly defined philosophical conceptions lay at the sources and foundations of the excellent development of logic and mathematics in interwar Poland. Although the Polish school of mathematics created a set-theoretic direction, it had a methodological character and not a strictly philosophical one (in the sense of *a priori* ontological or epistemological solutions as it was, for example in intuitionism). In the Warsaw School of Logic, philosophy was important; particularly concrete logical investigations were often philosophically motivated (e.g. Łukasiewicz's many-valued logics or Tarski's semantic definition of truth). However, when problems of logical nature were formulated their philosophical motivation stopped playing any role. The only important thing was technical logical investigations and the results obtained through the methods of mathematical logic. Only Leon Chwistek and Stanisław Leśniewski who were interested in nothing except logical problems resulting from their own philosophical views on the foundations of logic and mathematics did not follow this trend. Their philosophical views generated their research interests in concrete logical problems and their logical investigations were motivated by their philosophical views.

What were the sources of the described attitudes of the Polish logicians and mathematicians of the pre-war period towards the philosophy of logic and mathematics? It seems (cf. Woleński 1996) that on the one hand, they should be sought in

[2] Besides Tarski other Polish logicians and mathematicians, for example Chwistek, Leśniewski or Kotarbiński (cf. Sect. 2.2 of Chap. 2, Sects. 3.4 and 3.5 in Chap. 3) sympathised with nominalism. They played an important role in reviving interests in nominalism in the world. Leon Henkin writes about it in his paper 'Nominalistic analysis of mathematical language.' Among other things we can read, 'While the nominalistic tradition in philosophy is of course very ancient, a specific concentration of interest in this viewpoint, as applied especially to the analysis of mathematical language, can be clearly discerned in the work of the Polish school of logicians of the early decades of this century. The names of Lesniewski, T. Kotarbinski, Chwistek and his student Hetper, and Tarski are all associated with this activity. [...] Aside from some writings of Russell, this interest of the Polish logicians did not seem to be reflected outside of their own country. But with the transplantation of Tarski to the United States in 1938 [Tarski left in August 1939—remark is mine] the concern with nominalism made itself evident in this country, and subsequently in countries of western Europe' (1962, pp. 187–188).

the distinction between research practice and philosophical disputes concerning the foundations of mathematics, which the Polish school of mathematics clearly suggested, and which was greatly expressed in the research stand taken in the dispute about the axiom of choice, and on the other hand, in the principle of distinguishing between science and worldview postulated by Kazimierz Twardowski and his school of philosophy. According to the last postulate, when we deal with a concrete scientific discipline the philosophical questions related to it become a kind of worldview. Whereas philosophical questions should be analysed by scientific methods and then the programme of scientific philosophy would be treated seriously.

One of the fruits of the described principle of distinguishing and not mixing philosophical questions and strictly scientific problems connected with mathematics and logic was the excellent development of the Polish mathematics and mathematical logic in those 20 interwar years (1919–1939).

Biographical Notes

AJDUKIEWICZ KAZIMIERZ was born in Tarnopol on 12 December 1890. He studied philosophy, physics and mathematics at the Jan Kazimierz University in Lvov. In 1912, he obtained his doctor's degree—the supervisor of his doctoral dissertation was Kazimierz Twardowski. In 1913, he passed the secondary school mathematics teacher state exam. In the years 1913–1914, he continued his studies at the University of Göttingen where he listened to the lectures given by Edmund Husserl, Leonard Nelson and David Hilbert. The views of the latter exerted a considerable influence on Ajdukiewicz, which was revealed in his *Habilitationsschrift*. At the outbreak of World War I, he was drafted into the Austrian army, and sent to the Italian front in 1915. In October 1918, he was appointed as the commander of a battery of the Polish Army in Cracow and then he commanded an armoured train. Till September 1919, he fought near Lvov. In 1920, he participated in the Polish-Bolshevik war. In the years 1919–1922, he worked as a teacher in a gymnasium [secondary school comparable to British grammar school] in Lvov and at the same time he conducted research. In 1921, he completed his habilitation at the Philosophical Faculty of the University of Warsaw. In the years 1922–1925, he lectured as a private docent at the University of Lvov and taught in secondary schools in Lvov. In 1925, he became professor of the University of Warsaw, and from 1928 he was professor of the University of Lvov. In 1940–1941, he lectured on psychology at the Lvov State Medical Institute. During the Nazi occupation he worked as an accountant and at the same time, he was involved in the underground education. In 1944–1945, he held the Chair of Physics at the Ivan Franko University in Lvov [the Ukrainian: Lviv]. In 1945, he was given the Chair of Theory and Methodology of Sciences at the University of Poznań, where he was also elected rector for the term 1948–1952. In the year 1954, he moved to the University of Warsaw. He died in Warsaw on 12 April 1963.

Ajdukiewicz dealt mainly with semiotics, epistemology and general methodology of sciences. He always followed anti-irrationalism and empiricism. He wrote many important and valued textbooks.

R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6

BANACH STEFAN was born in Cracow on 30 March 1892. His father was Stefan Greczek, a young highlander serving in the Austrian army (later he became a clerk in Cracow), and his mother was probably Katarzyna Banach, a highlander. He was brought up in a foster family (the owner of a laundry Franciszka Płowa and her daughter Maria Puchalska). He learnt mathematics himself. In 1916, Hugo Steinhaus met him accidentally in Planty Park in Cracow and took interest in him. Although Banach did not complete his studies he received his doctorate at the Jan Kazimierz University in Lvov in 1922—his doctoral dissertation contained the fundamental theorems of the new mathematical discipline: functional analysis. In 1922, he presented his *Habilitationsschrift* at the University of Lvov and was appointed as a professor. After the seizure of Lvov by the Russian forces in September 1939, he was a professor at the University of Lvov and the Dean of the Mathematical-Physical Faculty. During the Nazi occupation (1941–1944) he was a feeder of lice at the Rudolf Weigel Research Institute of Typhus and Viruses. After the Red Army had seized Lvov in 1944 he continued his work at the University as the head of the chair of mathematics. He also lectured at the Lviv Technical Institute. He died in Lvov on 31 August 1945.

He was the co-author of the new mathematical domain—functional analysis. He formulated its fundamental theorems and introduced terminology that was accepted all over the world. He was one of the chief figures of the Lvov School of Mathematics.

BIEGAŃSKI WŁADYSŁAW was born in Grabów on 28 April 1857. He studied medicine in Warsaw in 1875–1880. After having completed his residency in Russia (1881–1882) he continued studies in Berlin and Prague. Then he settled in Częstochowa where he practiced medicine for over 30 years. At the same time, he conducted research and was involved in social activities. He died in Częstochowa on 29 January 1917.

He practiced medicine (internal diseases, neurology) and dealt with the history of medicine, hygiene and social medicine as well as philosophy, in particular methodology, logic, epistemology and ethics.

BOCHEŃSKI JÓZEF (INNOCENTY) MARIA was born in Czuszów (District of Miechów) on 30 August 1902. He began the studies of law in Lvov in 1920. Then he moved to Poznań (in 1922) where he studied economy. However, he did not finish his studies. In 1926, he entered the major theological seminary in Poznań, and in 1927, he joined the Dominicans. In the years 1928–1931, he studied philosophy in Fribourg (Switzerland), where he received his doctor's degree. In the years 1931–1934, he studied theology at the Angelicum in Rome—he also obtained his doctorate there. From 1934, he lectured on logic at the Angelicum. In 1938, he presented his *Habilitationsschrift* to the Theological Faculty of the Jagiellonian University in Cracow. Together with Jan Drewnowski, Rev. Jan Salamucha and Bolesław Sobociński, he founded the so-called Cracow Circle. During World War II he served as chaplain to the Polish Army and the Polish II Corps in Italy. In the years 1945–1972, he was a professor at the University of Fribourg. In 1948, he received the Chair of the History of Modern Philosophy there. In 1958, he founded the Institute of Eastern Europe at the University of Fribourg. He died in Fribourg on 8 February 1995.

Bocheński dealt with philosophy and logic as well as the history of logic. He changed his philosophical views several times. First, he was a follower of Kant, then of Thomism, which he combined with analytic philosophy. In the early 1960s he refuted Thomism, revised his views on the development of European philosophy and created his own system of analytic Christian philosophy.

BORNSTEIN BENEDYKT was born in Warsaw on 31 January 1880. He studied in Warsaw and Berlin. In 1907, he received his doctor's degree at the University of Lvov under the supervision of Kazimierz Twardowski. From 1915 he lectured on logic, epistemology and ontology within the framework of the Warsaw Society of Science Courses and from 1918 in the Free Polish University (Polish: Wolna Wszechnica Polska). From 1928 he also worked in the Łódź branch of the Free Polish University. After World War II he held the Chair of Logic and Ontology at the University of Łódź. He died suddenly after an operation in Łódź on 11 November 1948.

Bornstein's scientific interests were on the border of philosophy and mathematics. At first, he dealt with Kant's philosophy, translated his works into Polish and developed his thought. Then he focused on the philosophy of mathematics and finally, he worked on metaphysical problems.

CHWISTEK LEON[3] was born in Cracow on 13 June 1884. He studied philosophy and mathematics at the Jagiellonian University and as an unenrolled student at the Academy of Fine Arts in Cracow under the supervision of Józef Mehoffer. In 1906, he obtained his doctorate at the Jagiellonian University, on the basis of his doctoral dissertation 'O aksjomatach' [On Axioms]. From 1906, he taught mathematics in the Jan Sobieski Gymnasium (which he had completed) on and off for 20 years. In the years 1908–1909, he continued philosophical studies in Göttingen (where he listened to Hilbert's lectures), in 1910, he stayed in Vienna, being fascinated with Venetian Renaissance painting. In 1913–1914, he studied drawing in Paris. In 1914–1916, he fought in the First Brigade of the Polish Legions. From 1922, he lectured on mathematics to students in natural sciences at the Jagiellonian University. He presented his *Habilitationsschrift* in 1928 to this University, and in 1930 received the Chair of Mathematical Logic at the Mathematical-Natural Sciences Faculty, the Jan Kazimierz University of Lvov. In June 1941, he left Lvov together with the Russian Army. He settled in Tiflis (Tbilisi), where he taught mathematical analysis. From 1943, he lived in Moscow. He was active in the Association of Polish Patriots. He died in Moscow on 21 August 1944 (some sources give the date of 27 August 1944 in Barwicha near Moscow).

[3] Details concerning Chwistek's life can be found in K. Estreicher's book *Leon Chwistek. Biografia artysty (1884–1944)* [Leon Chwistek. The Artist's Biography (1884–1944)] (1971).

Chwistek dealt with many disciplines: formal logic, theory of art; he was also a productive painter. He wrote two novels: *Kardynał Paniflet* [Cardinal Paniflet] (written in 1906, but its script was destroyed in 1917) and *Pałace Boga* [Palaces of God] (written in 1932–1933, partially printed in episodes in 1934 and 1939; after the war the State Publishing Institute [Polish: Państwowy Instytut Wydawniczy] published its reconstruction in 1968 and 1979).

DICKSTEIN SAMUEL was born in Warsaw on 12 May 1851. After having attended gymnasium in 1866–1869 he studied in the Main School [Polish: Szkoła Główna Warszawska]. When the School was closed and changed into a tsarist university, he studied at the Imperial University of Warsaw (1869–1870), where he received his Master's degree in mathematics in 1876. From 1870, he worked as a teacher of mathematics in various gymnasiums and the Leopold Kronenberg School of Economics. In the years 1878–1888, he ran his own *Realschule* [a type of secondary school]. After the opening of the University of Warsaw as a Polish institution in 1915, he was appointed as a professor of mathematics at this university. In 1919, he was appointed as an honorary professor of mathematics and history of science. He lectured mainly on algebra and history of science till 1937. He died in Warsaw on 29 September 1939.

DREWNOWSKI JAN FRANCISZEK was born in Moscow on 2 December 1896. He lived in Warsaw from 1903. In 1914, he attended technical courses in Warsaw and in 1915 he studied at the Mathematical-Physical Faculty in Petersburg. In 1916, he completed a course of engineering at the Military School in Petersburg and served as officer in the Russian army. In 1918, he was drafted into the Polish army and in 1920 he was an officer of the general headquarters. At the same time, he attended lectures at the Faculty of Finance and Economics of the School of Political Sciences. In 1921–1927, he studied philosophy, mathematical logic and mathematics at the University of Warsaw under the supervision of Stanisław Leśniewski, Jan Łukasiewicz and Tadeusz Kotarbiński. In 1927, he obtained his doctor's degree under Kotarbiński's supervision. His dissertation concerned Bolzano's theses on logic. During the defence of Warsaw in 1939 he was an aide-de-camp of the commander of sappers. After the capitulation he was in a German prisoner-of-war camp until 1945. Then he was in the Polish Forces in Rome and England. He returned to Poland in 1947. He was an advisor to the minister in the Central Planning Office and a scientific consultant in the Institute of Economics and Organisation of Industry. He was also an editor of technical dictionaries. He died in Warsaw on 6 July 1978.

Drewnowski dealt with problems of logic, philosophy and the philosophy of technology. Together with Józef Maria Bocheński, Bolesław Sobociński and Rev. Jan Salamucha he was part of the Cracow Circle. Under Salamucha's influence he was converted to Roman Catholicism.

HOENE–WROŃSKI JÓZEF MARIA was born (as Józef Stefan Hoene) in Wolsztyn on 23 August 1776.[4] In the years 1786–1790, he attended *Szkoła Wydziałowa* in Poznań. In 1792, he joined the army, changing his name to 'Wroński.' Taken as a prisoner of war in the battle of Maciejowice he joined the Russian army. In 1797, he gave up his military career and began studying at the University of Halle and the University of Göttingen. In 1800, he left for England, and then France, where he intended to join the Polish Legions of General Dąbrowski. On 15 August 1803, he experienced—as he claimed—illumination. To commemorate this event he chose the name of Maria. Throughout all his life he was a private scientist. He intended to create a new philosophy—'achrematic philosophy' (from the Greek *chrema*—thing) and make a profound reconstruction of the system of science. He was also a constructor of mathematical instruments and reformer of transport. Moreover, he was involved, through his journalistic activities, in political life. He followed the trend of the Messianic philosophy. Moreover, he wrote several hundred works concerning various disciplines but many of his books remained hand-written (cf. Dickstein 1896b).

He died on 9 August 1853. On his gravestone the following words were engraved: 'L'acte de chercher la Vérité accuse le pouvoir de la trouver' (The act of seeking the Truth testifies to the possibility of finding it).

JANISZEWSKI ZYGMUNT was born in Warsaw on 12 July 1888. He completed a gymnasium in Lvov in 1907 and then studied mathematics and philosophy in Zurich, Munich, Göttingen and Paris. In 1911, he took his doctor's degree in the area of topology in Paris, under the supervision of Henri Lebesgue (the examination commission included Henri Poincaré and Maurice Fréchet). From 1911, he taught students of higher grades in the Society of Science Courses in Warsaw (it was a kind of Polish university since under the Russian partition there was only the Russian University in Warsaw). In 1913, he presented his *Habilitationsschrift* to the University of Lvov. Until the outbreak of World War I he lectured on the theory of analytic functions and functional calculus. He was one of the first who enlisted in the Polish legions and took part—as a private in the artillery—in the winter campaign of 1914/1915 in the Carpathian Mountains. In 1916—refusing to take an oath of loyalty to the Austrian government—he found shelter under the false name of Zygmunt Wicherkiewicz in the District of Radom (in Boiska near Zwoleń). From there he moved to Ewin near Włoszczowa, where he cared for homeless children in the orphanage which he created and ran till the end of the war. In 1918, he was called to work at the Polish University of Warsaw, which was reopened in 1915. He undertook intense scientific, didactic and publishing activities. He died from the Spanish flu (after a 3 day illness) on 3 January 1920.

Janiszewski's works concern mainly topology and thus he is regarded as one of the founders of the Warsaw School of Topology. He was the author of the

[4] This is the most probable date. In his biographical note for the police in 1801, Wroński himself gave the date of his birth as 20 August 1776 whereas on the gravestone there is 24 August 1776.

programme of the development of Polish mathematics (which was the basis of the creation of the Polish School of Mathematics). He helped set up the first international specialist mathematical journal *Fundamenta Mathematicae*.

KOKOSZYŃSKA MARIA was born in Bóbrka on 6 December 1905. She studied at the Jana Kazimierz University of Lvov (she was a disciple of Kazimierz Twardowski and Kazimierz Ajdukiewicz). After having completed her studies in 1930 she was an assistant at the Faculty of Philosophy of this university. In 1938, she obtained her doctor's degree in philosophy. She habilitated at the University of Poznań in 1947. From 1948 until her retirement in 1976 she worked at the University of Wrocław. She died in Wrocław on 30 June 1981. She dealt with logic, semantics, methodology and epistemology.

KOTARBIŃSKI TADEUSZ was born in Warsaw on 31 March 1886. He attended the Fifth Government Gymnasium (with Russian as the language of instruction). Since he took part in a school strike in 1905 he was relegated just before his final examination. He left for Cracow where for a year he studied mathematics and physics as an unenrolled student. He took his final graduation examination in the private Gymnasium of Chrzanów in 1906 (with Polish as the language of instruction). Then he went to Lvov and Darmstadt to study architecture. However, his graduation certificate from the private gymnasium was not recognised by the authorities. Therefore, he went to Pärnu (Estonia), where he obtained a state graduation certificate in 1907. Afterwards he studied philosophy at Lvov under the guidance of Kazimierz Twardowski. In 1912, he obtained his doctor's degree. He returned to Warsaw where he took the qualifying examination and took up a career as a teacher of Greek and Latin in the Mikołaj Rej Gymnasium. In 1918, he began lecturing on philosophy at the University of Warsaw. In 1919, he was appointed as an extraordinary professor and in 1929—a full professor. In the 1930s he opposed anti-Semitism and the so-called bench ghetto. During the Nazi occupation he was involved in the clandestine courses. After the war he worked at the University of Warsaw and the University of Łódź. In fact, he organised the latter and became its first rector. At the same time, he helped reactivate the University of Warsaw. He held the Chair of Philosophy, and from 1951—the Chair of Logic at this university. In the years 1957–1962, he was President of the Polish Academy of Sciences. He dealt with philosophy, logic, ethics and praxeology. He died in Warsaw on 3 October 1981.

KOZŁOWSKI WŁADYSŁAW MIECZYSŁAW was born in Kiev on 17 November 1858. After passing his final secondary school examination in 1876 he enrolled in the Faculty of Medicine of the St Vladimir University in Kiev. Arrested in 1880 for his revolutionary activities he was sent to Siberia. In 1883, the Irkutsk court sentenced him to a stay without any time limit in Siberia, depriving him of any rights. In 1889, under the amnesty act he returned to Europe. In 1890, he received a Candidate of Sciences in botany (equivalent to a doctorate) at the Faculty of Natural Sciences of the University of Dorpat (today's Tartu in Estonia). After having moved to Warsaw he was involved in editorial work for several years. In 1896–1898, he worked as a teacher of Polish among Polish immigrants in North America. In 1899, he obtained his doctor's degree in philosophy at the Jagiellonian University. In 1900, he presented his habilitation thesis to the Jan Kazimierz University of Lvov—however, he did not get *veniam legendi* since under political pressure the ministry refused to accept his habilitation. From 1901, he lectured at Université Libre in Brussels and from 1902, he was a docent of the University of Geneva. In 1905, he settled in Warsaw where he taught philosophy in the Society of Science Courses. In 1919–1928, he was a professor at the Faculty of Philosophy of the University of Poznań. He died in Konstancin near Warsaw on 25 April 1935. He dealt with philosophy, sociology, history and botany.

LEŚNIEWSKI STANISŁAW was born in Sierpuchów, east of Moscow, on 30 March 1886 (18 March 1886 according to the Julian calendar) After having graduated from a gymnasium in Irkutsk he studied in Germany (among other places in Munich; certain sources speak about his studies in Zurich, Heidelberg and Leipzig). In 1912, he obtained his doctor's degree written under the supervision of Kazimierz Twardowski at the Jan Kazimierz University of Lvov. Then he stayed in France, Italy and in Saint Petersburg. In the years 1913–1915, he lived in Warsaw. He was a member of the Social Democracy of the Kingdom of Poland and Lithuania (SDKPiL) and it was Feliks Dzierżyński who accepted his candidature to the party. In 1915–1918, he taught mathematics in Polish secondary schools in Moscow and lectured in the so-called Higher Course. In Moscow he got to know Wacław Sierpiński, who introduced him to the group of Nikolai N. Luzin who dealt with set theory. In 1918, he returned to Warsaw where he was given the Chair of Philosophy of Mathematics at the University of Warsaw in 1919. He changed his social-political views, became conservative and anti-Semitic (but it should be stressed that he was not involved in any anti-Semitic disturbances of the 1930s). Together with Jan Łukasiewicz he co-founded the Warsaw School of Logic. He dealt with logic and the foundations of mathematics. Attempting to strengthen mathematics and eliminate antinomies, he created his own logical systems: propositional calculus (protothetic), calculus of names (ontology) and set theory in the collective sense (mereology). He died in Warsaw on 13 May 1939.

ŁUKASIEWICZ JAN was born in Lvov on 21 December 1878. He studied philosophy under the guidance of Kazimierz Twardowski at the University of Lvov. He obtained his doctor's degree in 1902, and then he continued studies, mainly in Germany and Belgium. In 1906, he presented his *Habilitationsschrift* to the University of Lvov and became a docent of this University. In 1911, he was promoted to an extraordinary professor ('professor extraordinarius'). In 1915, he was invited to hold one of the chairs of the newly restored University of Warsaw. He was a professor of this university till 1944. In 1920, he was elected to the Chair of Philosophy at the Faculty of Mathematics and Natural Sciences. In the years 1918–1920, he was head of the Department of Higher Schools in the Ministry of Religious Denominations and Public Enlightenment, and in 1919 he directed this ministry in Cabinet of Ignacy Jan Paderewski. He was twice elected the rector of the University of Warsaw (1922–1923 and 1931–1932). During World War II he worked in the municipality and taught in clandestine courses. In 1944, with the help of Heinrich Scholz, a professor of the University of Münster, he left Poland. His destination was Switzerland. However, the bombardments of Germany forced him to change his plans. After staying in Münster, he went to Belgium. In 1946, he was appointed to the Chair of Logic at the Royal Irish Academy in Dublin, where he spent the last decade of his life. He died on 13 February 1956.

Łukasiewicz dealt with philosophy (especially, in the first period of his scientific activities, i.e. between 1902 and 1918) and logic (mainly in the years 1918–1956). He was one of the first Polish scientists who dealt with mathematical logic in a professional way. He was also the first Polish lecturer on mathematical logic as a separate subject. With Stanisław Leśniewski he co-founded the Warsaw School of Logic.

MAZURKIEWICZ STEFAN was born in Warsaw on 25 September 1888. After completing high school in 1906, he studied in Cracow, Munich, Göttingen and Lvov. In 1913, he was awarded his doctorate, under the supervision of Wacław Sierpiński, by the University of Lvov. After the University of Warsaw had been restored in 1915, he was a lecturer there. In 1919, he presented his habilitation thesis to the Jagiellonian University. In the same year he was appointed as a professor of the University of Warsaw. He held the post of vice-rector and for a few times he was the dean of the Faculty of Mathematics and Natural Sciences. He is one of the co-founders of the Polish School of Mathematics. Together with Sierpiński and Janiszewski he founded the journal *Fundamenta Mathematicae*. He dealt with topology, mathematical analysis and probability calculus as well as cryptology. In the years 1920–1939, he collaborated with the Cipher Bureau. He broke the cipher used by the Soviet Army, which enabled the Polish General Staff to read the orders of Tukhachevski's staff and contributed to Polish victory at the Battle of Warsaw.[5] He died in Grodzisk Mazowiecki on 19 June 1945.

MEHLBERG HENRYK was born in Kopyczyńce (today's Kopychintsy) near Lvov on 7 October 1904. He studied at the Jan Kazimierz University in Lvov, then in Fribourg as well as at the Sorbonne and Collège de France. As a grant-holder he did his internship with Moritz Schlick in Vienna. He belonged to the second generation of the Lvov-Warsaw School. During the Nazi occupation he used—because of his Jewish background—the name of Suchodolski. After World War II he worked at the University of Warsaw and the University of Łódź. In 1949 he immigrated to Canada and was a professor at the University of Toronto. After having moved to the USA, he lectured at Princeton University, the University of Chicago and the University of Florida, Gainesville. He dealt with the philosophy of

[5] Cf. Nowik (2004), pp. 25, 232 and 925. It should be added that Sierpiński and Leśniewski also collaborated with the Cipher Bureau.

formal and non-formal sciences as well as problem concerning the relationships between science and philosophy. He died in Gainesville (Florida, USA) on 10 December 1979.

MOSTOWSKI ANDRZEJ was born in Lvov on 1 November 1913. In the years 1923–1931, he attended the Stefan Batory Gymnasium in Warsaw. In 1931, he began mathematical studies at the University of Warsaw. There Alfred Tarski and Adolf Lindenbaum exerted the greatest influence on him. He completed his studies in 1936. In the academic year 1936/1937, he stayed in Vienna and in the year 1937/1938 in Zurich where he listened to the lectures by Kurt Gödel, Hermann Weyl and Wolfgang Pauli. After returning to Warsaw he defended his doctoral dissertation, written under the supervision of Kazimierz Kuratowski (Tarski, who was in fact Mostowski's supervisor, could not fulfil this function since he was not a professor then) in February 1939. Earlier in January 1939, he had begun working for the State Meteorological Institute in Warsaw. During the war he worked as an accountant. In the years 1942–1944, he was engaged in underground teaching at the University of Warsaw. He presented his habilitation thesis to the Jagiellonian University in 1945. From 1946 until his death, he worked at the University of Warsaw: from 1947 as an extraordinary professor and from 1951—a full professor. He died in Vancouver (Canada) on 22 August 1975.

Mostowski dealt with mathematical logic and the foundations of mathematics, in particular set theory, recursion theory, model theory, logical calculi and proof theory.

SALAMUCHA JAN was born in Warsaw on 10 June 1903. In 1919, he entered the major seminary in Warsaw, and in 1925 he received the Sacrament of Holy Orders. In 1920, he was a medical orderly in the Polish-Soviet war. In the years 1923–1927, he studied philosophy, mathematics and mathematical logic at the University of Warsaw where he listened to the lectures by Jan Łukasiewicz, Stanisław Leśniewski, Tadeusz Kotarbiński, Władysław Tatarkiewicz and Stefan Mazurkiewicz. He obtained his doctor's degree in 1927. From 1927 till 1929, he studied at the Pontifical Gregorian University in Rome. Till 1933, he lectured on philosophy at the major seminary in Warsaw. In 1933, he completed his habilitation but it was recognised by the ministry only in 1936. In the year 1934, he also began lecturing at the Jagiellonian University. With Józef Maria Bocheński, Jan Drewnowski and Bolesław Sobociński he founded the so-called Cracow Circle. In November 1939, he was arrested, together with other professors of the Jagiellonian University, and transported to the concentration camp in Sachsenhausen and from there to Dachau. He was released in January 1941 and forced his way to Warsaw where he was active in the underground movement. During the Warsaw Uprising he was a chaplain. He was killed on 11 August 1944.

Salamucha's investigations concerned mainly the possibilities of applying mathematical logic to formalise reasonings in Medieval and modern philosophy. Being under Łukasiewicz's influence he tried to use logical methods in metaphysics.

SIERPIŃSKI WACŁAW was born in Warsaw on 14 March 1882. After attending a classical gymnasium in the years 1900–1904, he studied at the Faculty of Physics and Mathematics of the Imperial University of Warsaw. Upon graduating he received the degree of candidate of sciences (and a gold medal for his work concerning the theory of numbers). Then he took up a career as a secondary school teacher of mathematics and physics in a girls' school. In the years 1905–1906, he studied at the Faculty of Philosophy of the Jagiellonian University where he obtained his doctor's degree in 1906. After returning to Warsaw he taught in Polish private schools. In 1907, he took a several month programme at the University of Göttingen where he met Constantin Caratheodory. In 1908, he presented his habilitation thesis to the University of Lvov. In 1909, Sierpiński began lecturing, also on set theory,[6] at the University of Lvov. In 1910, he was appointed as an extraordinary professor. He held one of the two chairs of mathematics (the other was given to Józef Puzyna). At the beginning of World War I he was interned—as an Austrian subject—in Wiatka by the Russians.[7] Thanks to the attempts of his Russian colleagues he was moved to Moscow. There he collaborated with Nikolai N. Luzin who worked on the theory of analytic sets. In the future Sierpiński would turn out to be one of the most important figures in the development of this branch of set theory, i.e. descriptive set theory. In 1918, he returned to Poland. At first, he lectured (for a semester) at the Jan Kazimierz University in Lvov and from autumn 1918, he was a professor of the University of Warsaw (from 1919 he was a full professor). During World War II he was active in clandestine teaching. In 1945, he found himself in Cracow where he lectured at the Jagiellonian University for a semester. In the autumn of 1945, he began working at the University of Warsaw. From 1948, he also worked in the State Mathematical Institute, which later became the Mathematical Institute of the Polish Academy of Sciences. He retired in 1960. He died on 21 October 1969.

Sierpiński dealt with the theory of numbers, set theory, mathematical analysis, general and descriptive set theory, set-theoretic topology, measure theory and category theory as well as the theory of functions of a real variable.

SLESZYŃSKI JAN was born in the town of Lisianka (Lysyanka) in the region of Kiev on 23 (or 11 according to the old style) July 1854. In the years 1871–1875, he studied at the Faculty of Mathematics of the University of Odessa. In 1880, he obtained his Master's in pure mathematics and having been granted a scholarship he went to Berlin for 2 years. There he listened to lectures by Ernst Kummer, Leopold Kronecker and Karl Weierstrass. After his return to Odessa, he wrote his *Habilitationsschrift* in 1882 and was appointed as a private docent at the University of Odessa. He also worked as a teacher of mathematics at the major seminary and in

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[6] The rare opinion that these were the first lectures on this new domain in the world is false. Earlier lectures on set theory were delivered by Ernst Zermelo (Göttingen, 1900–1901), Felix Hausdorff (Leipzig, 1901) and Edmund Landau (Berlin, 1902–1903, 1904–1905).

[7] When the war broke, Sierpiński was on holiday in Russia.

secondary schools. In 1893, he obtained his doctor's degree (Russian equivalent of habilitation) in pure mathematics and was appointed as an extraordinary professor. He became a full professor in 1898 and emeritus professor in 1908. In 1909, he retired. In 1911, he moved to Cracow where he became a private docent (with the title of full professor) in the newly created chair at the Faculty of Philosophy of the Jagiellonian University. He was appointed head of this chair in 1919 (after Kazimierz Żorawski had left for Warsaw) and was also appointed as a full professor of mathematics and logic. Sleszyński's Chair was the first chair of mathematical logic at the Jagiellonian University, and more, the first in the world. In the year 1924, he retired at his own request, and in 1925, he received the title of emeritus professor of the Jagiellonian University. Sleszyński died on 9 March 1931.

ŚNIADECKI JAN was born in Żnin on 29 August 1756. He attended the gymnasium in Trzemeszno. He studied in the Lubrański Academy in Poznań and the Cracow Academy where in 1775 he obtained the degree *magister scientiarum* (substitute for habilitation) in philosophy. In 1778, he continued his studies abroad: in Göttingen, Leiden and Paris. In 1781, he was appointed as a professor of mathematics and astronomy in Cracow. In 1807, he moved to Vilnius where he received the post of professor of astronomy and was elected as Rector of the University of Vilnius. He held this post till 1815. He spent the last period of his life (from 1825) in Jaszuny (Lithuanian: Jašiūnai) near Vilnius. In 1812, he became a member of the Provisional Government Commission of the Grand Duchy of Lithuania, created by Napoleon I. He died in Jaszuny on 9 November 1830.

He dealt with mathematics, astronomy, philosophy, geography, problems of pedagogy and linguistics. He was a follower of empiricism and opponent of metaphysics and Romanticism. In 1782, he came out with a project of building an astronomical observatory in Cracow. In 1784, he constructed (with Jan Jaśkiewicz) the first hot air balloon in Poland. He did a lot for the organisation of science and education. He contributed to spreading the Polish mathematical terminology. One of the planetoids was named 1262 Sniadeckia in his honour and so was Śniadecki—a lunar crater.

STAMM EDWARD was born in Tarnów on 10 March 1886. He attended schools in Tarnów, Nowy Targ and Cracow. He studied in Zurich, Innsbruck and Vienna, where in 1909 he received a diploma at the Faculty of Philosophy. In 1914, he was called up for military service and served in the Austrian infantry and radiotelegraph units. After the war till his demobilisation he was the commander of the radiotelegraph station in Cracow. Throughout his life he taught in secondary schools in small towns, including Surochów near Jarosław, Ciechanów, Lubowidz (Żuromin County), Toruń, Przemyśl, Strzyżów on the Wisłoka River and Wieliczka. He died from exhaustion and emaciation in Wieliczka on 21 November 1940 after he had returned from the September campaign.

STEINHAUS HUGO was born in Jasło on 14 January 1887. After completing a classical gymnasium in Jasło in 1905, he studied mathematics and philosophy at the Jan Kazimierz University of Lvov. In the years 1906–1911, he studied at the University of Göttingen under the guidance of David Hilbert and Felix Klein. In 1911, he was awarded his doctorate in philosophy there. In the years 1911–1914, he lived in Jasło, and in 1915, he took part in World War I. In 1917, he completed his habilitation in Lvov. In 1920, he was promoted to extraordinary professor at the Jan Kazimierz University, and in 1923—full professor. He gathered around himself (with Stefan Banach) a group of outstanding mathematicians, creating a strong scientific centre called the Lvov School of Mathematics. They dealt mainly with functional analysis. During World War II he hid himself under the name of Grzegorz Krochmalny. After the war he co-organised the Wrocław scientific environment. He was a professor of the University of Wrocław and the Mathematical Institute of the Polish Academy of Sciences. He died in Wrocław on 25 February 1972.

He focused on trigonometric and orthogonal series as well as the problem of summability. Many of his works turned out to be essential to the exact formulation of the foundations of probability calculus based on measure theory and set theory. He analysed the applications of mathematics in various domains, especially in biology, medicine and statistics. He also popularised mathematics. Moreover, he was known for his aphorisms.

STRUVE HENRYK (alias Florian Gąsiorowski) was born in Gąsiorów near Koło on 27 June 1840. He studied theology and then philosophy at the University of Tübingen. In 1862, he obtained his doctorate at the University of Jena. In the years 1863–1903, he was a professor of the Main School in Warsaw, then the (Russian) Imperial University of Warsaw. In order to maintain this post he had to defend his doctorate again in Moscow (1870). In 1903, he decided to stay with his daughter in England. He died in Eltham (Kent) on 16 May 1912.

He dealt with philosophy and the history of philosophy. He called his philosophical system 'ideal realism'—it was an attempt at reconciling realism with idealism. He popularised the history of Polish philosophy in Germany and England. He wrote many logic textbooks.

TARSKI ALFRED was born Alfred Tajtelbaum in Warsaw on 14 January 1901. In 1923, he changed his surname to 'Tarski.' In 1918, he began studying biology at the University of Warsaw. Influenced by Leśniewski he abandoned biology and enrolled in mathematics and philosophy. His teachers included Kotarbiński, Leśniewski, Łukasiewicz, Mazurkiewicz and Sierpiński. In 1924, he completed his doctorate under the supervision of Leśniewski; his *Habilitationsschrift* was entitled 'O wyrazie pierwotnym logistyki' [On the Primitive Term of Logistic].[8] He was habilitated a year later. In 1925–1939, he taught in the Stefan Żeromski

[8] The English translation can be found in Tarski (1956), pp. 1–23.

Grammar School in Warsaw and at the same time, he held a temporary position of an associate professor at the University of Warsaw. In August 1939, he left for the USA to participate in the 5th International Congress for the Unity of Science. The outbreak of the war made him stay there. He lived in the USA till the end of his life (in 1946 the University of Warsaw wanted him to accept the post of associate professor but he refused the offer). He lectured at Harvard University (1939–1941), was a visiting professor at City College of New York (1940–1941), a member of the Institute for Advanced Study in Princeton (1941–1942). He also lectured at the University of California at Berkeley, where he was appointed as a professor in 1946. There he created a strong research centre for logic and the foundations of mathematics. He died in Berkeley, California, on 26 October 1983.

Tarski dealt with numerous domains of mathematics, in particular set theory, algebra, metamathematics and logic. He was one of the most outstanding logicians of the twentieth century. One of his greatest achievements was the definition of the concept of truth. He initiated a new area of the foundations of mathematics—model theory. He conducted research on set theory and universal algebra. He launched the programme of algebraisation and topologisation of mathematical logic. Tarski's works exerted a decisive influence on the development of mathematical logic and the foundations of mathematics. In 2000, the International Astronomical Union Commission named planetoid 13672, discovered in 1997, after Alfred Tarski.

TWARDOWSKI KAZIMIERZ was born to Polish parents in Vienna on 20 October 1866. He attended the Theresianum in Vienna. He studied philosophy (under the guidance of Franz Brentano) at the Faculty of Philosophy of the University of Vienna as well as classical philology, mathematics and physics. In 1891, he obtained his doctor's degree and in 1894—habilitation. He lectured on logic at the University of Vienna. In 1895, he was appointed as a professor of philosophy at the University of Lvov, where he began intensive teaching and organisational activities. He collaborated with Władysław Weryho to create *Przegląd Filozoficzny*. In 1904, he founded the Polish Philosophical Society, and in 1911—*Ruch Filozoficzny*. He contributed to the origin of modern philosophy in Poland. He established the Lvov-Warsaw School of Philosophy. He educated numerous students, out of who over 30 became professors of various universities. Among his pupils were Tadeusz Czeżowski, Kazimierz Ajdukiewicz, Roman Ingarden, Izydora Dąmbska, Tadeusz Kotarbiński, Władysław Witwicki, Jan Łukasiewicz and Leopold Blaustein. He died in Milanówek on 11 February 1938.

Twardowski was one of the first Polish philosophers who paid attention to Bolzano's works. He was a representative of the so-called older Brentanism. His achievements include the distinction between the content and object of presentations as well as actions and products, which became part of philosophy. His anti-relativist views influenced investigations on the theory of truth to a great extent.

WILKOSZ WITOLD was born in Cracow on 14 August 1891. After completing the gymnasium (his classmate was Stefan Banach) he began mathematical studies at the Faculty of Philosophy of the Jagiellonian University. In 1912, he studied in

Turin under the guidance of Giuseppe Peano. In the years 1914–1915, he served in the Legions and then he continued his studies at the Jagiellonian University. He graduated in 1917, and in 1918, he obtained his doctor's degree in philosophy on the basis of his dissertation dedicated to the theory of absolutely continuous functions and the Lebesgue integral. In 1917–1920, he taught in private gymnasiums in Zawiercie and Częstochowa, and listened to lectures on canon law and history of law in Cracow. In 1920, he presented his habilitation thesis concerning strictly measured functions to the Jagiellonian University. He lectured as a private docent. In 1922, he became extraordinary professor and in 1935—full professor of this University. In November 1939, he was arrested together with other Cracow professors. He was soon released because of his bad health condition. In 1940–1941, he worked in the Municipal School of Trade in Cracow. He died there on 31 March 1941.

Besides strictly scientific works he published six textbooks on set theory, arithmetic, algebra and theory of numbers (cf. 1932, 1933, 1934). His original axiomatics of the arithmetic of natural numbers (1932), equivalent to Peano's axiomatics (1889), is worth mentioning. He wrote popular books—one of the most important ones is the booklet *Liczę i myślę. Jak powstała liczba* [I Count and Think. How the Number Originated] (1938), which had several editions. Many of his works have not been published. It should be added that Wilkosz popularised knowledge through many radio talks, including talks on mathematics, and in the courses organised by the Ministry of Religious Denominations and Public Enlightenment. He also contributed to the development of radio technology and radiophony.

ZAREMBA STANISŁAW was born in Romanówka (Romanovka) in Ukraine on 3 October 1863. After completing the *Realschule* in Saint Petersburg in 1881 he began technical studies in the Saint-Petersburg Institute of Technology. After obtaining a diploma of engineer technologist in 1886 he went to Paris, where he studied mathematics. In 1889, he obtained his doctor's degree in mathematical science at the Sorbonne. In 1892–1900, he taught in grammar schools in Digne, Nîmes and Cahors. In 1900, he was appointed as an extraordinary professor of mathematics of the Jagiellonian University. In 1905 he became a full professor. After retiring in 1935 he was appointed as an honorary professor of the Jagiellonian University. He died in Cracow on 22 November 1942.

He was one of the pioneers of modern mathematics in Poland. He dealt mainly with mathematical analysis and applications of mathematics, especially in physics—mathematical physics. Consequently, he focused on the second order partial differential equations of all three types constituting a basic mathematical tool of physics and technology. He was also interested in logic as well as philosophy and the methodology of mathematics.

ZAWIRSKI ZYGMUNT was born in Berezowica Mała near Zbaraż (Eastern Galicia) on 29 September 1882. He studied mathematics, physics and philosophy at the universities in Lvov (1901–1906), Berlin (1909) and Paris (1910).

He completed his doctorate under Kazimierz Twardowski's supervision in 1910. Then he taught mathematics and the propedeutics to philosophy in various Lvov gymnasiums. He was habilitated in 1924 at the Jagiellonian University in Cracow on the basis of his thesis on the axiomatic method in natural science. In 1924–1928, he lectured on philosophy at the Faculty of General Studies of the Lvov Polytechnic. In 1928, he was elected to the Chair of Theory and Methodology of Sciences at the University of Poznań, and in 1937 he moved to the Jagiellonian University. During World War II he taught in clandestine courses. He died in Końskie on 2 April 1948.

Zawirski dealt mainly with the methodology of sciences as well as the theory of cognition and ontology, especially the problems related to the development of physics—relativity theory and quantum theory. He was then the most outstanding Polish specialist in problems concerning the border line of physics and philosophy. He was also interested in applications of mathematical logic.

ŻYLIŃSKI EUSTACHY was born in Kuna on 1 October (19 September according to the Julian calendar) 1889. In 1907–1911, he studied at the Mathematical-Physical Faculty of the St Vladimir Imperial University in Kiev. In 1912–1913, he studied at the University of Göttingen (under the guidance of Edmund Landau), at the University of Marburg (under the guidance of Kurt Hensel) and the University of Cambridge (studying with Godfrey H. Hardy). In 1914, he was awarded his Master's Degree by the University of Kiev (equivalent to a PhD). In 1917–1919, he worked as a private docent of mathematics at the Polish College of the University in Kiev. Refuting the proposal to hold the Chair of Mathematics at the Ukrainian State University in Kamieniec Podolski (Kamyanets-Podilsky), he became an extraordinary professor at the Jan Kazimierz University in Lvov where he was appointed head (after the late Józef Puzyna) of the Chair of Mathematics at the Faculty of Philosophy. In 1922, he became a full professor. In 1939–1941, he was a professor of the Ivan Franko State University in Lvov. In 1941–1944, he worked as a statistician in the private transport office in Lvov and at the same time, he taught at the underground university. In 1946 he, together with his wife and children, left Lvov and settled in Łódź. In 1946–1947, he worked for the Ministry of Foreign Affairs. From 1946 (in fact from 1947) till 1951, he was a full professor at the Faculty of Mathematics, Engineering and Construction of the Silesian Technical University in Gliwice. He died in Łódź on 4 July 1954.

Żyliński dealt mainly with the theory of numbers. After 1919 he took up algebra, logic and the foundations of mathematics. He also focused on the problems of teaching mathematics. He wrote many textbooks.

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R. Murawski, *The Philosophy of Mathematics and Logic in the 1920s and 1930s in Poland*, Science Networks. Historical Studies 48, DOI 10.1007/978-3-0348-0831-6