Logique & Analyse **214** (2011), 161–172

WHITEHEAD'S EARLY PHILOSOPHY OF MATHEMATICS AND THE DEVELOPMENT OF FORMALISM*

ROSEN LUTSKANOV

The design of this article is to provide clues for adequate appreciation of the role that Whitehead's *Treatise on Universal Algebra with applications* (henceforth "TUA") played in the development of the philosophical conceptions on the nature of mathematical knowledge. The elaboration of this topic seems justified in view of the fact that the philosophical and mathematical achievements of this work were largely destined to oblivion[1] but must be taken into account by any attempted interpretation of his later philosophy. My principal claim is that one of the possible ways to evaluate properly the impact of this work on the philosophy of mathematics is to render it as intermediary between the early formalism of Peacock and the mature formalism of Hilbert. The strategy pursued here is (§1) to motivate the introduction of Whitehead's TUA as part of the history of formalism, (§2) to sketch Peacock's approach to Symbolic Algebra and expose the problems that haunted it since its very conception, (§3) to present Hilbert's alternative approach that allegedly solves the same problem though in rather different setting, (§4) to

*I would like to express my deep gratitude to the organizers of the workshop dealing with Whitehead's philosophy of logic and mathematics that was held in September 25 (2008) at the University of Liège. Thanks to Sebastien Gandon, Bruno Leclerq and Vesselin Petrov for comments on a previous draft of this paper and to Ronny Desmet, who brought to my attention Whitehead's book review of Berkeley's "Mysticism in Modern Mathematics" (1910). Last but not least, I would like to acknowledge my debt to the anonymous referees of *Logique et Analyse* whose substantial criticisms helped me to clarify some points and add some necessary ramifications. I admit that it was not possible for me, at least at this point, to remove some of the reasons for their discontent.

[1] As Henry and Valenza have noted, in spite of its initial success, Whitehead's *Treatise on Universal Algebra* "has found little audience in twentieth century mathematics" (Henry & Valenza, 1993, p. 157). We can attribute this fact to many different circumstances: Whitehead's repudiation of his own formalistic philosophy, the stunted development of the field of universal algebra before Noether and Birkhoff's contributions, the advent of logicism effected by the overwhelming *Principia Mathematica*, and the rise of the Gibbs–Heaviside system of vector analysis that overshadowed Whitehead's alternative algebraic notation introduced in TUA (Dawson, 2008, p. 77).

explain why Whitehead's Universal Algebra can be viewed as the unnoticed missing link in the development of formalism.

***

(§1) Recently the range of significance of the term "formalism" — one of the well-established brand labels in philosophy of mathematics — was somewhat blurred. The ranks of formalists were populated by new figures and the history of formalism was prolonged backwards. This is mainly due to Michael Detlefsen who in his recently published exquisite survey acknowledged that "Formalism is not a single viewpoint concerning the nature of mathematics. Rather, it is a family of related viewpoints sharing a common framework. The basic component of this framework is ... the recognition of the nonrepresentational role of language in mathematical reasoning" (Detlefsen, 2005, pp. 236–237). The "nonrepresentational" character of mathematics is guaranteed by two methodological assumptions dubbed "creativism" (the idea that the essence of mathematics is its freedom to create new symbolic forms disregarding the problem of the "evaluation of content") and "symbolism" (the liberal admittance of "nonsemantical uses of signs")[2] (Detlefsen 2005, p. 263). Furthermore, he identified the honorable bishop Berkeley as the first exponent of this view and distinguished Peacock and Hilbert as his most influential disciples. What strikes the eye here is the fact that Detlefsen made no attempt to explore the development of formalism in the considerable period of time which separates Peacock from Hilbert. Were there any proponents of formalism in the interval between them? No doubt, if we put aside the German pre-formalistic predecessors of Hilbert (some of whom are mentioned in the concluding remarks below), one of the primary suspects is Whitehead, who developed his universal algebra in Cambridge as direct descendant of the algebraic school of Peacock, Gregory, Boole and

[2] The creativist doctrine is probably the chief reason for the general appeal of formalism because it completely removes the vexatious epistemological scruples that haunted mathematics throughout its history. But it must not blind us to the fact that although the meaning of mathematical signs does not come into play in formal derivations, it still is crucially important for the construction of formal axiomatic systems which are usually devised with particular interpretation in mind. Thanks to the anonymous referee who brought forth this point.

Hamilton and shared with Hilbert a tutor[3] and common pejorative appellation.[4] But to see how he fits in here, we first need to examine briefly the views of the formalist top-liners: Peacock and Hilbert.

(§2) In his *Treatise on Algebra* (1830), Peacock introduced Symbolic Algebra as "the science which treats of the combinations of arbitrary signs and symbols by means of defined though arbitrary laws" (Peacocke, 1830), §78. Moreover, as far as the "signs" and "symbols" employed in algebraic calculation have no provisory intended interpretation but the results of calculation are supposed to be numerically interpretable in principle, he added in the introduction that "[i]nterpretation is necessary for the final result of reasoning but need not be applied to the intermediate parts of the process which leads to that result" (Peacocke, 1830, p. xiv). This means that the laws of the science of algebra are subject to two general requirements: they have to be "consistent" (as far as inconsistency implies non-interpretability) and "useful" (which means that they have to obey certain arithmetic regularities[5]). The satisfaction of the last requirement was seen to be guaranteed by the so-called *Principle of Permanence of Equivalent Forms* (PPEF) according to which "... if we discover an equivalent form in Arithmetical Algebra or any other subordinate science, when the symbols are general in form though

[3] Felix Klein: since 1885 Klein was Hilbert's mentor and even directed his work on algebraic geometry (Rowe, 2003, p. 49) and at the same time was frequent guest of Whitehead's friend and colleague Andrew Forsyth at Trinity College and inspired his own work on non-Euclidean geometry (Dawson, 2008, p. 69). This fact has crucial importance in the present context, as far as Klein's main methodological concern (codified in the so-called "Erlanger Programm") was to secure the unity of the constantly proliferating mathematical theories while both Hilbert's and Whitehead's mathematical work may be seen as driven by the same desire (Stump, 1997, p. 390). Formalism is a natural strategy in this setting because it seems possible to extract the common core of mathematical theories through examination of the invariant features of their formal presentation.

[4] "Formalist": It is commonly believed that Brouwer introduced the term "formalism" in 1912 (in his inaugural lecture at the University of Amsterdam) just to ridicule Hilbert's philosophical views (Brouwer, 1913, p. 82), but as a matter of fact Whitehead rejected his own "formalist" position two years before, in his first and only book review (Whitehead, The Philosophy of Mathematics, 1910, p. 239).

[5] The latter condition seems justified in view of the fact that symbolic algebra was viewed as generalization of ordinary arithmetic that is not "useless and barren speculation" but assumes the applicability of its results in the narrow domain of natural numbers. As Detlefsen himself noted, this can be spelled out in today's mathematical parlance as the statement that symbolic algebra has to be conservative extension of ordinary arithmetic (Detlefsen, 2005, p. 273). This seems reasonable because in the absence of general model theory "interpretable" boils down to "arithmetically interpretable" which does not hold for people like Hilbert who worked in a pretty different context.

specific in their nature, the same must be an equivalent form, when the symbols are general in their nature as well as in their form" (Peacocke, 1830), §132. What has crucial importance here is that according to Peacock the fact that a system of Symbolic Algebra conforms to PPEF does not imply that its laws are justified by or founded on the laws of ordinary arithmetic. On the contrary, Symbolic Algebra is a "science of suggestion", its laws are "arbitrary conventions", its postulates — "arbitrary assumptions" (Peacocke, 1830, pp. vii–viii). The reason is evident: conformance to PPEF is necessary but not sufficient condition for applicability (i.e. validity in the restricted domain of natural numbers) as far as there is an indefinite number of non-equivalent systems of Symbolic Algebra obeying the principle of permanence but ostensibly devoid of numerical content.

The admission of non-interpretable subsidiary expressions was vital for the newly born science of Symbolic Algebra and was later adopted by Boole, who affirmed in his *Laws of Thought* (1854) that "the validity of a conclusion arrived at by symbolical process of reasoning, does not depend upon our ability to interpret the formal results which have presented themselves in the different stages of the investigation" (Boole, 1854, pp. 67–68). However, the second generation of Cambridge algebraists that applied Peacock's mathematical techniques to the study of logic was equally dissatisfied of the impossibility to justify mathematically the use of non-interpretable elements. It was not Boole, but De Morgan that expressed their common discontent: "We have shown the symbol $\sqrt{-a}$ to be void of meaning, or rather self-contradictory and absurd. Nonetheless, by means of such symbols a part of algebra is established which is of great utility. It depends upon the fact, which must be verified by experience, that the common rules of algebra may be applied to these expressions without leading to any false results. An appeal to experience of this nature appears to be contrary to the first principles laid down at the beginning of this work. We cannot deny that it is so in reality, but it must be recollected that it is but a small and isolated part of an immense subject" (De Morgan, 1910, p. 51). So, both Peacock and his followers admitted that even if we conform to the principle of permanence we can get no proper epistemological ground for the use of Symbolic Algebra employing numerically non-interpretable elements. That is why, the succeeding generations of mathematicians sought to justify mathematical speculation through philosophical reflection (predominantly in a neo-Kantian fashion). Hamilton, for example, conceived Algebra as a "Science of Pure Time" and noted that it is to be grounded on a priori intuitions,[6]

[6] "The argument to the conclusion that the notion of time can be unfolded into an independent Pure Science or that a Science of Pure Time is possible, rests chiefly on the existence of certain a priori intuitions, connected with that notion of time" (Hamilton, 1837).

while Cayley invited his fellow thinkers to "put the doctrine of Imaginaries upon a sound philosophical basis" just like Kant did for the negative ones in his *Über die negativen Grössen in der Weltweisheit*.[7] Later the solutions proposed by Hamilton and Cayley were severely criticized; in particular, the latter was condemned for being "in search of a philosophy": "Cayley overtly asks the question, 'What is the meaning of an imaginary point?' after having carefully explained to us how mathematicians are led analytically, i.e. by algebraic symbolism, to the notion of imaginary points. If the explanation is a real one, what more can philosophy do for us in the matter?" (Berkeley, 1910, p. 69). In sum, the whole development starting with Peacock's Treatise and culminating in the works of Boole, De Morgan, Hamilton and Cayley seemed flawed because they (overtly or not) recognized the impossibility to justify intra-mathematically the mathematician's recourse to non-interpretable algebraic forms (paradigmatically, imaginary numbers). This obstacle was removed by Hilbert and his pupils who wrestled with the same problem, provoked by the use of finitely non-interpretable set-theoretic entities (the so-called 'transfinite elements').

(§3) Following Detlefsen's juxtaposition, it is easy to detect the similarities between Peacock's Symbolic Algebra and Hilbert's Proof Theory: (a) like Peacock, Hilbert made a distinction between real [finite] and ideal [transfinite] elements in mathematics; (b) also like Peacock, he saw this distinction as being at least partially a distinction between those parts of mathematics that purport to express an independently given reality, and those parts of mathematics that are pure creations of the mind whose purpose is to preserve mathematical reasoning in a simple and inferentially efficient form; (c) like Peacock again, Hilbert subscribed to an essentially Berkeleyan conception of language, a conception according to which the cognitive significance of some parts or uses of language lies in their capacity to guide reasoning or inference without essential adversion to some particular interpretation; (d) finally, just like Peacock, Hilbert accepted the existence of two basic constraints on the use of symbolical methods in mathematics — namely, that they have to be consistent and "useful", i.e. interpretable in the contentually meaningful domain of mathematical discourse (Detlefsen, 2005, p. 291). Furthermore, the comparison between the two approaches can be based on the following features of Hilbert's metamathematical program that qualify it as paradigmatically formalistic: (a) Hilbert conceived mathematical proof as "purely formal manipulation of definite signs according to fixed rules"

[7] This queer suggestion can be found in his Presidential Address to the British Association read in September 1883, which was published later in the eleventh volume of his *Collected Mathematical Works* (Cayley, 1896, p. 434).

which does depend on their contentual interpretation;[8] (b) Hilbert strived to detach mathematical reasoning from its intuitive origins by viewing the basic notions of mathematical theories not as referring to some independently existing entities but as implicitly defined by the axioms of the theory in question; (c) Hilbert differentiated between "real" (intuitively interpretable) and "ideal" (intuitively non-interpretable) elements of the theory squarely in line with the traditional algebraic discrimination between "real" and "imaginary" numbers.

The most important difference between Peacock and Hilbert, however, stems from the fact that the latter emphasized the importance of one particular kind of consistency proofs (dubbed "finitistic") and suggested that the use of expanded mathematical systems containing ideal elements can be justified by such proof of consistency (a claim that Peacock would never accept as plausible). In his famous *Mathematical Problems* lecture Hilbert maintained that "if it can be proved that the attributes assigned to the concept can never lead to a contradiction by the application of finite number of logical processes, I say that the mathematical existence of the concept is thereby proved"[9] (Hilbert, Mathematical problems, 1902). In fact, Hilbert claimed that the finitistic consistency proof establishes "reliability" (or "usefulness"): "such a finitistic consistency proof would entail that the infinistic mathematics could never prove a meaningful real statement that would be refutable in finitistic mathematics, and hence that infinistic mathematics is reliable" (Raatikainen, 2003, p. 160). In other words, the consistency proof actually establishes the conservativity of transfinite (ideal) mathematics over its finite (real) fragment, i.e. the fact that whenever a 'real' proposition can be proved by 'ideal' means, it can also be proved by 'real', finitary means (Zach, 2003, pp. 88–92). That is why, in his Münster lecture *Über das Unendliche* Hilbert affirmed that "there is a condition, a single but absolutely necessary one, to which the use of the method of ideal elements is subject, and that is the proof of consistency; for, extension by the addition of ideals is legitimate only if no contradiction is thereby brought about in the old, narrower domain, that is, if the relations that result for the old objects whenever the ideal objects

[8] Let us recollect here that Peacock used for the same purpose the nearly logically equivalent expression "science which treats of the combinations of arbitrary signs and symbols by means of defined though arbitrary laws".

[9] As is well known, the idea that the existence of some set of mathematical objects can be derived from the consistency of the formal axiomatic system which characterizes them, and that their existence in turn bestows meaning or intended interpretation to the "formula game of pure mathematics" was severely criticized by Frege in his correspondence with Hilbert (Bochenski, 1970, pp. 292–293).

are eliminated are valid in the old domain" (Hilbert, 1926). In other words, Hilbert realized that (pace Peacock) there is specific kind of proof of consistency (finitary proof of consistency) establishing simultaneously the consistency and the "usefulness" (in other words, conservativity) of the expanded systems of mathematics. Such a proof of consistency serves the task of justifying intra-mathematically the use of 'ideal objects' thus making their extra-mathematical (that is to say, philosophical) justification superfluous. So it occurs that the final outcome of the development of Hilbert's Program (in its canonic Bernaysean presentation) was intended to be the complete separation of mathematics from philosophy effected by the internal methodological justification of mathematical knowledge: "We must make the concept of mathematical proof itself the object of an [mathematical] investigation just as ... philosopher criticizes reason itself" (Hilbert, 1918) because "The task falling to metamathematics vis-à-vis the system of mathematics is analogous to the task which Kant ascribed to the critique of reason vis-à-vis the system of philosophy"[10] (Bernays, 1930). Thus the gravest difficulty haunting pure mathematics was solved, the philosophical ratification of mathematical theories was conceived as superfluous, and accordingly nothing was standing in the way of the (purportedly) metaphysically neutral system-building of pure mathematics.

(§4) What was the role that played Whitehead in the whole story? In a nutshell, I am prepared to argue that he provided the historically first attempt, predating the development of Hilbert's program with more than two decades,[11] to render consistency proofs as providing epistemological sanction for the systems of pure mathematics. First of all, while writing TUA he was arch-formalist, just like Peacock before him and Hilbert after him. Moreover, he was part of Cambridge's algebraic lineage and was aware of the difficulties it presented.[12] So it is not strange at all, that he himself propounded a solution, and even a revolutionary one. First of all, in the opening chapter of TUA (bearing the name *On the nature of a calculus*) he demarcated the subject of pure mathematics in broadly formalist fashion arguing that (a) mathematics is formal ("the meaning of propositions forms no part of

[10] Translated in English by Paolo Mancosu and Ian Mueller for *The Bernays Project*.

[11] It is true that Hilbert stressed the importance of consistency proofs in his 1900 Paris address, but the rigorous development of his program implementing a model-theoretical rendering of consistency and precisely delimited finite standpoint had to wait for more than two decades (Raatikainen, 2003, pp. 157–158).

[12] Whitehead's friend Andrew Forsyth (the same who invited Klein to Cambridge) served as editor of Cayley's mathematical papers and wrote the foreword of the eleventh volume where the now awkward sounding presidential address was published.

the investigation"); because (b) it deals with substitutive signs ("a substitutive sign is a means of not thinking about the meaning which it symbolizes"); hence (c) it is a calculus ("the art of manipulation with substitutive signs") and can be studied "apart from any attention to the meaning to be assigned to the sign"; finally (d) it is evident that, conceived in such a way it "avoids the necessity of inference and replaces it by an external demonstration"[13] (Whitehead, 1898, pp. 3–10). In this way it becomes clear that Whitehead, just like Peacock before him and Hilbert after him, conceived mathematical demonstration not as intellectual operation on mental contents captured by interpreted signs but as physical manipulation with (generally not interpretable) marks on paper. Because of this fact his approach became vulnerable to the difficulties indicated by De Morgan more than half a century ago: "The difficulty is this: the symbol $(-1)^{1/2}$ is absolutely without meaning when it is endeavored to interpret it as a number; but algebraic transformations which involve the use of complex quantities of the form $a + bi$ where a and b are numbers and i stands for the above symbol, yield propositions which do relate purely to number ... The difficulty was solved by observing that ... the laws of Algebra ... depend entirely on the convention by which it is stated that certain modes of grouping the symbols are to be considered as identical" (Whitehead, 1898, p. 10). In other words, Whitehead's way to tackle with imaginaries was based on the discrimination between 'pure mathematics' ("referring to the world of ideas created by convention") and 'applied mathematics' ("referring to the world of existing things by the mediation of an act of abstraction").[14] The importance of this division lies in the fact that the notions of pure mathematics have no existential import (because of its conventional character) while the notions of applied mathematics

[13] The discrimination between "inference" and "external demonstration" was borrowed from Bradley who mentioned that "When in ordinary fact some result can be seen and is pointed out, perhaps no one would wish to call this 'demonstration'. It is mere perceiving or observation. It is called demonstration when, to see the result, it is necessary for us first to manipulate the facts; when you show within and by virtue of a preparation you are said to demonstrate. But if the preparation experiments outwardly, if it alters and arranges the external facts, then the demonstration is not an inference. It is inference when the preparation is ideal, where the rearrangement which displays the unknown fact is an operation in our heads" (Bradley, 1883, p. 225).

[14] This crucial distinction was stressed again by Whitehead even a decade later, when his past allegiance with formalism was over: "In any consideration of the principles of mathematics two distinct subjects should be kept separate — namely (1) the nature of mathematical propositions considered in themselves apart from any admixture of particular application and (2) the discussion of the groups of particular facts which are special cases of mathematical truth. These two subjects are respectively the problem of the nature of mathematics, and that of the applications of mathematics" (Whitehead, The Philosophy of Mathematics, 1910, p. 235).

have existential import (which is trivial consequence of the circumstance that they are conceived as "referring to the world of existing things"). Furthermore, the lack of existential import of the axiomatically defined concepts of pure mathematics implies that "they require for their verification no more than a mere test of self-consistency" while the abstracted from experience concepts of applied mathematics "require for their verification something more than self-consistency, namely, truth" (Whitehead, 1898, p. vii). In effect, the epistemological innovation of Whitehead's formalism (which, as we mentioned earlier, was a predecessor of Hilbert's variety of formalism) was concerned with the realization that "Mathematical reasoning is deductive in the sense that it is based upon definitions which, as far as the validity of reasoning is concerned (apart from any existential import), need only the test of self-consistency. Thus no external verification of definitions is required in mathematics, as long as it is considered merely as mathematics" (Whitehead, 1898, p. vi). Precisely here lies the motivation to view Whitehead's position as a formalist one: by discriminating rigorously between pure and applied mathematics he rendered the first as abstract calculus whose locutions are to be seen "nonsemantically", i.e. as having no relation to meaning or truth whatsoever. What is new here is that "p is consistent" is seen to imply not "p is possibly true" (as was recognized at least from Leibniz onwards) but "p is (simply) true" (in the just examined sense of abstract validity in the world of ideas).

Consequently, no conceptual elucidation was conceived as necessary for the properly defined 'imaginary' entities of abstract algebra: "Not only the practical men, but also men of letters and philosophers have expressed their bewilderment at the devotion of mathematicians to mysterious entities which by their very name are confessed to be imaginary ... Are the incommensurable numbers properly called numbers? Are the positive and negative numbers really numbers? Are the imaginary numbers imaginary, and are they numbers? — are types of futile questions" (Whitehead, 1911, p. 87). In this context it becomes important to note that in the beginning of XX century the idea of the philosophical neutrality of pure mathematics was attributed explicitly to Whitehead: "Mr. Whitehead's eminence as a mathematician, especially as a philosophical mathematician, made it necessary to examine his views on this particular question [the justification of imaginary numbers] ... because these views have evidently to some extent imposed themselves on mathematicians. This influence is very marked in the article Algebra of vol. XXV of the *Encyclopaedia Britannica* [where it is said that] the progress of analytical geometry led to a geometrical interpretation both of negative and of imaginary quantities [and subsequently] it was at last realized that the laws of algebra do not depend for their validity upon any particular interpretation ... the only question is whether these laws do or do not involve any logical contradiction"[15] (Berkeley, 1910, p. 83). So it seems obvious that Whitehead was the first "philosophical mathematician" to claim that consistency proofs provide epistemological justification for the implementation of numerically non-interpretable signs in the formal systems of pure mathematics. What Hilbert did was to add the 'finiteness' requirement motivated by his own epistemological position expressly qualified as 'finite standpoint' (finite Einstellung).

***

I hope that what was said up to this point at least makes plausible the claim that Whitehead's role in the history of formalism is grossly underestimated; that he was the first to suggest a possible way to free mathematics from the reign of philosophy. When we become aware of this fact, many new and equally important questions start to pop up, for example: (a) To what extent his early ("formalist") views on the nature of mathematics were preserved in his later work?; (b) To what extent Whitehead's formalism took part in the shaping of Hilbert's metamathematical program? It is strictly impossible to answer these questions here, the most I can do is to provide some sketchy and inconclusive suggestions. As far as the first question is concerned, there are two things to be said. On the one hand, Whitehead expressly rejected his earlier views on mathematics affirming that "the formalist position [adopted in TUA], whilst it has the merit of recognizing an important problem, does not give the true solution" (Whitehead, 1910, p. 239). On the other hand, even in his Lowell lectures Whitehead stressed the importance of pure mathematics as general ontology which is in perfect accordance with his earlier formalism.[16] As far as the second question is concerned, no trustworthy answers are available. It is certain that Whitehead met Hilbert at the International Congress of Mathematicians that was held in Paris in 1900 but no

[15] Whitehead wrote a crushing review of "Mysticism in modern mathematics" but nevertheless accepted almost completely Berkeley's assessment of TUA; moreover, Berkeley's book is important here only as an indication of the way TUA was understood (or misunderstood) in the first years of the XXth century.

[16] According to the Lowell lectures, "The point of mathematics is that in it we have always got rid of the particular instance, and even of any particular sorts of entities" (Whitehead, Science and the Modern World, 1925, p. 31). But to say that mathematics does not study "any particular sort of entity" is the same as to say that mathematics disregards the particular meanings of the signs it manipulates with, which is the essence of formalism. Moreover, to say that it does not study anything in particular is the same as to say that it studies everything in general, which qualifies mathematics as some sort of general ontology. This interpretation is perfectly in line with the conception of "speculative philosophy" as "generalized mathematics", that was presented in Whitehead's major opus *Process and Reality* (Desmet, 2008).

additional information about their encounter is readily available.[17] What is sure is that no history of formalism in particular (or of philosophy of mathematics in general) could be regarded as complete without better appreciation of Whitehead's *Treatise on Universal Algebra*. It played important part in the formation of XX century abstract mathematics and the general philosophical outlook which shapes up and rounds off its theoretical practice.

Institute for the Study of Societies and Knowledge
Bulgarian Academy of Sciences
Moskovska 13A Str., Sofia, Bulgaria
E-mail: rosen.lutskanov@gmail.com

REFERENCES

Berkeley, H. (1910). *Mysticism in modern mathematics*. Oxford: Oxford University Press.

Bernays, P. (1930). Die Philosophie der Mathematik und die Hilbertsche Beweistheorie. *Blatter fur deutsche Philosophie*, 4, 326–367.

Bochenski, I. (1970). *History of Formal Logic*. Amsterdam: North-Holland Publishing Company.

Boole, G. (1854). *The Laws of Thought*. London: Macmillan & Co.

Bradley, F.H. (1883). *The Principles of Logic*. London: Kegan Paul, Trench and Co.

Brouwer, L.E. (1913). Intuitionism and Formalism. *Bulletin of the American Mathematical Society*, 20, 81–96.

Cayley, A. (1896). Presidential Address to the British Association. In *Collected Mathematical Works* (Vol. xi, pp. 429–459). Cambridge: Cambridge University Press.

Dawson, A. (2008). Whitehead's Universal Algebra. In M. Weber, & W. Desmond (Eds.), *Handbook of Whiteheadian Process Thought* (Vol. II, pp. 67–86). Frankfurt: Ontos Verlag.

De Morgan, A. (1910). *On the study and difficulties of mathematics*. Chicago: Open Court Publishing Company.

[17] According to one of the anonymous referees, the actual connection between the Cambridge School and Hilbert's work is probably established not by Whitehead, but by Hermann Hankel. I agree on this point with the sole remark that the work of Heinrich Heine ("Die Elemente der Funktionenlehre", 1872) and Carl Thomae ("Elementare Theorie der Analytischen Funktionen einer komplexen Veränderlichen", 1898) is equally important in this context (Simons, 2009, pp. 293–294). Even if this is so, my claim remains intact, because I did not suppose any sort of "causal influence" of Whitehead on Hilbert. My concern was the history of ideas, not of men and their dealings.

Desmet, R. (2008). Speculative philosophy as a generalized mathematics. In M. Weber, & P. Basile (Eds.), *Chromatikon* (Vol. IV, pp. 37–50). Louvain: Presses universitaires de Louvain.

Detlefsen, M. (2005). Formalism. In S. Shapiro (Ed.), *Oxford handbook of philosophy of logic and mathematics* (pp. 236–317). Oxford: Oxford University Press.

Grattan-Guinness, I. (2002). Algebras, Projective Geometry, Mathematical Logic, and constructing the world: intersections in the philosophy of A.N. Whitehead. *Historia Mathematica*, 29, 427–462.

Hamilton, W.R. (1837). Theory of conjugate functions, or algebraic couples; with a preliminary and elementary essay on algebra as the science of pure time. *Transactions of the Royal Irish Academy*, 17 (1), 293–422.

Henry, G.C. & Valenza, R.J. (1993). Whitehead's early philosophy of mathematics. *Process Studies*, 22 (1), 157–172.

Hilbert, D. (1902). Mathematical problems. *Bulletin of the AMS*, 8, 437–479.

Hilbert, D. (1918). Über axiomatisches Denken. *Matematische Annalen*, 78, 405–415.

Hilbert, D. (1926). Über das Unendliche. *Mathematische Annalen*, 95, 161–190.

Peacocke, G. (1830). *A Treatise on Algebra*. Cambridge: Deighton.

Raatikainen, P. (2003). Hilbert's program revisited. *Synthese*, 137, 157–177.

Rowe, D. (2003). From Königsberg to Göttingen: a sketch of Hilbert's early career. *Mathematical Intelligencer*, 25 (2), 44–50.

Sauer, T. (1999). The relativity of discovery: Hilbert's first note on the foundations of physics. *Archive for History of Exact Sciences*, 53, 529–575.

Simons, P. (2009). Formalism. In D. Gabbay, P. Thagard & J. Woods, *Philosophy of Mathematics* (pp. 291–310). Amsterdam: Elsevier.

Stump, D. (1997). Reconstructing the Unity of Mathematics circa 1900. *Perspectives on Science*, 5 (3), 383–417.

Whitehead, A.N. (1911). *Introduction to Mathematics*. London: Williams & Norgate.

Whitehead, A.N. (1925). *Science and the Modern World*. New York: The Macmillan Company.

Whitehead, A.N. (1910). The Philosophy of Mathematics. *Science Progress*, 5, 234–239.

Whitehead, A.N. (1898). *Treatise on Universal Algebra with applications* (Vol. I). Cambridge: Cambridge University Press.

Zach, R. (2003). Hilbert's "Verunglückter Beweis", the first epsilon theorem and consistency proofs. *History and philosophy of logic*, 25, 79–94.