On The Scientific Works Of Tadeusz Batóg[1]

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Abstract

This paper discusses all the academic works of Tadeusz Batóg. After a short biographical note we present the major areas of Professor Batóg's scientific activity: logic and history of logic, theoretical phonology, philosophy of mathematics and, finally, methodology of sciences. A reference like TB n denotes the n-th position on the list of Tadeusz Batóg's publications included at the end of this paper.

Tadeusz Batóg was born on January 22, 1934 in the small village of Miernów in South central Poland (now in Kielce voivodship). He went to primary school in the village. During the years 1946–1951 he was a student of a State High School (lycée) in Busko-Zdrój. As he mentions in his autobiographical sketch (published in 1984 in *Ruch Filozoficzny* [*Philosophical Movement*], vol. XLI, No. 1, 1984, pp. 76–78), it was at the lycée that he first became interested in philosophy and logic. He had to study the former on his own, helped in his studies by two classics on the subject, that is, by Władyslaw Heinrich's *Zarys Historii Filozofii* [*An Outline of the History of Philosophy*] and Władysław Tatarkiewicz's *Historia Filozofii* [*The History of Philosophy*]. As for logic, it was at the time an obligatory subject in Polish secondary schools. Batóg described his logic instructor — Dr. J.B. Daniewski, a classical philologist — as a competent teacher, though not easy to follow.

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In 1951, Tadeusz Batóg first came into contact with Adam Mickiewicz University in Poznań. In the years 1951–1955 he was a student of Polish Philology at the Philological Faculty of the University. It may come as a surprise to those who know his scientific works that his M.A. thesis dealt with — poetry. The title was *Erazm Otwinowski's Pre-Arian Poetry* and the work was supervised by Professor Jerzy Ziomek. In 1955 Tadeusz Batóg passed his final M.A. exam which basically consisted in the defence of the thesis.

Tadeusz Batóg's scientific interests, however, seem to have been strongly influenced by Adam Wiegner's lectures on logic and Tadeusz Szczurkiewicz's lectures on the history of philosophy. On September 1, 1954 — still as a student of Polish Philology — Tadeusz Batóg was employed by the University as a junior assistant at the Department of Fundamentals of Marxism-Leninism (Katedra Podstaw Marksizmu-Leninizmu). He had been teaching for two years there, before he joined the Department of Logic where he worked for one year. Both Departments formed — at the time — parts of the Philosophical and Historical Faculty, and Adam Wiegner was the director of the Department of Logic. Tadeusz Batóg was to devote three of his later works (see: TB 13, 14, 16) to discussing and analysing Wiegner's logical works.

Readers interested in the history of logical and methodological research conducted at Poznań University should consult Seweryna Łuszczewska-Romahnowa's *Logika* [*Logic*], an excellent and exhaustive paper on the subject included in a volume devoted to the development of scientific investigation (up to the early seventies) in the Wielkopolska Region (see: Łuszczewska-Rohmanowa 1973, pp. 190-198). There they can find exhaustive information on the scientists and institutions involved in logical and methodological research in Poznań. It is interesting to note that at that point logic in Poznań had been shaped by, among others, Władysław Mieczysław Kozłowski and Zygmunt Zawirski, and that at the time when Tadeusz Batóg was beginning his scientific career, the community of local logicians could boast of such famous names as Seweryna Łuszczewska-Romahnowa, Kazimierz Ajdukiewicz, Roman Suszko and Jerzy Giedymin who together with Adam Wiegner were active in the field. It goes without saying, that we are not able to define here the role any of the above mentioned scholars played in the development of logic and methodology in Poznań during the last 50 years. We think, however, that it would be an extremely interesting task for a scientific historian to analyse the influence logic may have had on the development of other domains of science in the region.

On September 1, 1957, promoted to the position of senior assistant, Tadeusz Batóg joined the staff of the Department of Logic at the Faculty of Mathematics, Physics and Chemistry of Adam Mickiewicz University in Poznań. Seweryna Łuszczewska-Romahnowa headed the Department at that time.

Batóg's first paper was published in *Tygodnik Zachodni* (*Western Weekly*; a magazine published in the years 1956–58). It was entitled *Philosophy is practical study* [*Filozofia jest nauką praktyczną*], and dealt with an analysis of Tadeusz Kotarbiński's philosophical works.

In the sixties, Tadeusz Batóg published two important purely logical papers dealing with the generalized theory of classification (TB 9, 10; both written together with Seweryna Łuszczewska-Romahnowa), and several papers on logical attempts at the reconstruction of the notion of the phoneme, and on the axiomatic foundations of theoretical phonology. The latter papers (TB 3, 4, 6, and 12, in particular) became very popular and were regarded as fundamentally important for phonology (especially segmental phonology).

Having defended his Ph.D. dissertation, entitled *A Logical Reconstruction of the Notion of the Phoneme* [*Logiczna rekonstrukcja pojęcia fonemu*, TB 3], Tadeusz Batóg was granted (on May 14, 1962) the degree of doctor of humanistic studies. The dissertation was supervised by Professor Seweryna Łuszczewska-Romahnowa, and its defence took place at the Philosophical-Historical Faculty of the University. Consequently, on September 1, Tadeusz Batóg was promoted at his Department to the rank of senior lecturer (adiunkt).

His monograph, *The Axiomatic Method in Phonology* (TB 12) was a part of Tadeusz Batóg's habilitation qualifying procedure (which is obligatory in Poland before a scholar can be promoted to assistant professor or "docent" in Polish). On October 15, 1968, Tadeusz Batóg was granted the degree of "habilitated docent" (with a specialization in mathematical logic) by the Council of the Faculty of Mathematics, Physics and Chemistry (and the degree was confirmed by the Minister of Education and Higher Learning on May 31, 1969). On May 1, 1971, Tadeusz Batóg was finally nominated to the position of docent (assistant professor) at the Institute of Mathematics. It is difficult to resist the temptation to quote at this point two sentences from the autobiographical sketch mentioned above:

> As a result of the 1969 reform of Adam Mickiewicz University, the Department of Logic was renamed Department of Mathematical Logic and as such, it was included into the Institute of Mathematics. That was the way Tadeusz Batóg became a member of the staff at the Institute.

In the seventies, Tadeusz Batóg was trying to develop his axiomatic phonological theory. He enlarged on his original logical reconstruction of various structuralist approaches (based on distributional dependences) by including into his system some aspects pertaining to semantic relations (TB 19, 26). At the same time he published his first handbook of mathematical logic: *Zasady logiki* [*Principles of Logic*] (TB 31), as well as several short papers. Of all his works published at that time, particularly interesting (and vividly discussed at the time of their publication) were his methodological studies (TB 23, 27, 29) as well as two papers written together with Maria Steffen-Batogowa; namely, TB 30 (which presents an algorithm of converting phonetic texts in Polish into their orthographic equivalents; the algorithm was meant to be a converse to the one presented earlier in MSB 16), and TB 36 (Batóg makes use here of his own methods of the generalized classification theory to define a natural notion of "phonetic distance between units of a sound system").

Several years later, Tadeusz Batóg decided to return to his studies on the mathematical foundations of theoretical phonology. His recent works include TB 46, 48, and 58. They all deal with problems pertaining to the feasibility of describing the phonemic inventory (of a single language) by automatization. It seems obvious that a solution to the problem would largely depend on technology (e.g., the processing power of electronic calculating machines). On the other hand, it is equally manifest that no computer will ever be clever enough to be able to cope with sloppily organized linguistic data. It seems appropriate to quote at this point the sentences concluding Tadeusz Batóg's Ph.D. dissertation (which went to press as early as June 13, 1960):

> It is only natural that we may still go on arguing if linguistics, or any other non-mathematical branch of studies for that matter, needs the degree of precision that the use of mathematical logic assumes. Today, however, there seems to appear more and more evidence pointing clearly to the fact that they do. I shall not repeat here all the arguments so frequently put forward by both the linguists favouring scientific precision and by logicians. I would only like to stress one aspect of the problem which in my opinion is momentous. Thus, it is of primary importance for contemporary linguistics to make its results accessible to several branches of technology and even to medicine (and I am well aware of how strange that suggestion might seem to an uninitiated reader). I am not so much interested here in the quite urgent at the moment issue of machine translation (undisputedly assuming the need of making grammar more formal and "precise"), but rather in a thing which is apparently very simple and easy; that is in the indispensability of making use of various linguistic tests in communication, telecommunication and audiometry, for instance. Practice has shown that in all these areas linguists will have to cooperate very closely with engineers. The cooperation has so

> far been made very difficult by the simple fact that the engineer can hardly understand the imprecise, often vague explanations usually offered by the linguist.
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> On the other hand, linguistics itself could profit enormously if it were to accept logistic precision. This acceptance would facilitate cooperation between linguists and mathematicians within so called linguistic statistics or statistical linguistic, as well as make linguistics better prepared to use modern technology in linguistic research itself (for we cannot exclude at the moment that a machine will be designed soon which — on the basis of a precisely formulated system of theoretical phonology — would be able to characterize a phonological system of any human language by analyzing a chosen idiolect of that system). (TB 3, pp. 179–180)

For several recent years Tadeusz Batóg has been publishing more and more articles dealing with philosophy of mathematics, history of logic and fundamentals of mathematics. His latest publication in this domain is the monograph *Dwa paradygmaty matematyki. Studium z dziejów i filozofii matematyki* [*Two Paradigms of Mathematics. An Essay on the History and Philosophy of Mathematics*] (TB 60).

From what he has published on the subjects so far, I would also like to draw the reader's attention to his two encyclopedic entries (TB 40 — on the philosophy of mathematics; and TB 41 — on set theory), and to several articles analyzing the contribution of Polish logicians and philosophers to the development of formal studies (in particular: TB 37, 38 and 49 — on the influence that Kazimierz Ajdukiewicz's early works had on the discovery of the deduction theorem, as well as on the formulation of the semantic definition of entailment, the rule of infinite induction and the calculus of syntactic types; TB 45 — where he suggested a way of solving the Locke-Berkeley problem by means of certain tools offered by contemporary formal logic; TB 47 — where Batóg points to the fact that certain pioneer ideas of formal logic, commonly ascribed to Leibniz could already be found in Locke's writings; and TB 51, 61, 55, 56, 60 — other encyclopedic entries discussing some monumental works of the twentieth century philosophy and fundations of mathematics).

I should add here that Tadeusz Batóg has long been interested in the history of logic. Though there have always been many historians of logic in Poland (and many of their works are really excellent), it was Batóg who finally managed to fix the date marking the beginnings of modern mathematical logic in Poland. In 1973, he published two papers (TB 20, 22), in which he discussed the life and works of Stanisław Piątkiewicz. It seems that Piątkiewicz's work entitled *Algebra w logice*

[*Algebra in Logic*] — which was published in 1888 — marked the beginnings of modern mathematical logic in Poland. Thus, Batóg's findings move that date back more than a decade earlier than had been commonly (after Ajdukiewicz, Ingarden and Jordan) assumed. The beginnings were usually associated with either the first lectures on the algebra of logic (given by Twardowski in 1899), or sometimes with Twardowski's coming to Lwów (in 1895), or with the publication (in 1910) of Łukasiewicz's monograph *O zasadzie sprzeczności u Arystotelesa* [*On the Principle of Contradiction in Aristotle*].

On October 1, 1974 Tadeusz Batóg became Director of the Department of Mathematical Logic (when the former Head, Seweryna Łuszczewska-Romahnowa retired). Since then he has headed the Department, which until 1993 was part of the Institute of Mathematics. Now it functions as an independent unit of the Faculty of Mathematics and Computer Sciences. In the years 1975–1981 Tadeusz Batóg was Vice-Director of the Institute of Mathematics (responsible for research work — in the years 1975-1978; and for teaching — in 1978–1981).

Despite his many other duties, Tadeusz Batóg had been for eleven years (in the years 1962-1973) assistant to the editor of "Studia Logica", the most prestigious journal of logic in Poland.

Legions of students of mathematics and philosophy at UAM remember Tadeusz Batóg's lectures in mathematical logic. He has always been a rigorous, though intelligible and approachable instructor. He has also supervised over 170 M.A. theses. In 1961, his didactic accomplishments were honoured with the Minister's award. In 1978, his second logic handbook — *Podstawy logiki* [*Fundamentals of Logic*] — won him another ministerial award. Incidentally, the handbook also had extremely favourable reviews in such prestigious professional journals as *Wiadomości Matematyczne* [*Mathematical News*] and *Ruch Filozoficzny* [*Philosophical Movement*].

Three assistants working in his Department have written their Ph.D. dissertations under Professor Batóg's supervision. They were Wojciech Buszkowski, Wojciech Zielonka and Maciej Kandulski. All the dissertations dealt with some aspects of the theory of categorial grammars. The first of the three doctors mentioned above has been a full professor for some time now. Thus, thanks to Tadeusz Batóg's efforts it seems justifiable to talk of the "Poznań school of categorial grammars" at the moment. Many publications of his students have been quoted in world literature on the subject, so it would be no exaggeration to call the achievements of "the school" truly imposing. Ajdukiewicz's pioneering ideas on categorial grammars have been elaborated on and developed here in Poznań.

A short list of subjects in which the research workers of the Department have been interested so far includes:

— logical reconstruction of linguistic theories,

— applications of the theory of algorithms,

— theory of categorial grammars,

— various problems pertaining to decidability, completeness and axiomatizability of theories,

— models of Peano's arithmetic,

— history and philosophy of mathematics,

— algebraic semantics for various types of modal logic, and

— applications of information system theories.

Dr Jerzy Czajsner worked in the Department for many years, before he retired. He was primarily interested in the logical foundations of physics (see: Czajsner, 1978). His lectures, and the didactic mastery he showed while giving them, are remembered by everyone who attended them.

Professor Roman Murawski has been interested in Peano's arithmetic and also in some aspects of philosophy of mathematics and history of logic (Murawski 1986, 1988, 1990, 1995; Marciszewski, Murawski 1995). In 1996 Professor Murawski became the Head of the Department of Mathematical Logic.

Professor Wojciech Buszkowski has been working on the theory of categorial grammars (Buszkowski 1989, Buszkowski, Marciszewski, van Benthem 1988). Since December 1993 he has headed his own Department of Computation Theory (Maciej Kandulski is a staff member in the Department).

Dr Wojciech Zielonka has dealt with problems pertaining to decidability and axiomatizability of various versions of the syntactic types calculus (Zielonka 1978, 1981).

Dr Maciej Kandulski's works have either dealt with theories of categorial grammars, or they have discussed history of mathematics and applications of information systems (Kandulski 1983, Kandulski, Marciniec, Tukałło 1992).

Dr Kazimierz Świrydowicz used to work on the logical foundations of legislative systems (Świrydowicz 1981, 1995); for some years now he has been primarily interested in semantic problems pertaining to non-classical logics.

Professor Zygmunt Vetulani formerly worked with set theory. Recently he has been working on computational linguistics (Vetulani 1989). In 1993, he left the Department of Mathematical Logic and set up his own Department of Computational Linguistics and Artificial Intelligence.

The author of the present paper used to work in Professor Batóg's Department in the seventies (at present, he is a staff member of the Institute of Linguistics).

Later, I shall have something to say on the influence Tadeusz Batóg's works have had on scholars from outside his Department.

Basically, all his works deal with — broadly taken — problems of logic. We have shown this in what was said above when they were published. To present their contents, however, it will be convenient to subdivide them — arbitrarily — into three sub-sections, namely:

1. Logic and the history of logic
2. Logical foundations of phonology, and
3. Methodology and philosophy of mathematics.

The subdivision is not quite artificial, though — Batóg's texts themselves define quite clearly the reader they are meant for.

Naturally, we shall disregard here the situations, in which some texts addressed, for instance, to linguists might turn out too difficult for them to read on account of their lack of logical training. We should not blame poor uneducated linguists alone for that. Those who prepare teaching programmmes in the departments educating future linguists are primarily responsible for such situations.

1 Logic and the History of Logic

This section will deal with the following works of Tadeusz Batóg: TB 9, 10, 13, 14, 16, 17, 21, 22, 28, 31, 32, 35, 38, 39, 41, 43, 49, 51, 52, 53, 54, 56, 57, and TB 60.

Two of these are handbooks: *Zasady logiki* [*Principles of Logic*] (TB 31), and *Podstawy logiki* [*Fundamentals of Logic*] (TB 39). The former presents an axiomatic approach to classical sentential calculus (including a discussion of such meta-logical problems as consistency, completeness and axiom independence), and also an approach to the classical predicate calculus (axiomatized too) with a brief chapter on definitions. Batóg's *Fundamentals...* clearly aims at broadening the scope of the first handbook (in terms of sheer size it is twice as long), as well as at introducing certain new ideas left out of the *Principles...* The chapters on some syntactic aspects of the predicate calculus, on the deductive systems and on semantics are completely new. The chapter on definitions has been enlarged as well. In my opinion, the most essential feature of the two handbooks is the author's methodological consistency in presenting logic as simultaneously a branch of science and a tool to be used in other domains of research. The main stress was laid on the problems of inference, so that the reader is to see LOGIC as a set of methods for establishing the validity of statements on the basis of the axioms accepted and the rules of inference.

Tadeusz Batóg's handbooks have been used by students of mathematics, philosophy, linguistics and computer studies at our University as part of the obligatory literature accompanying their lectures in mathematical logic. The author writes in the Introduction to his *Fundamentals of Logic*:

> I think that this book is rather intelligible. To make it intelligible however, I have not even once tried to make any of its subject matter shallow, as is sometimes the case. Instead, I have taken great pains to present all the subjects in it as precisely and exactly as I could.

The Author has done just that. To get through the handbook both the student and the lecturer (who wants to use it in his lectures) have to work slowly and carefully. Yet the effort — after some years - pays with interest.

Neither of Batóg's handbooks have any tests or exercises accompanying the texts. Yet, as I have been using the books in my lectures for many years now, I find this lack to be a merit rather than a drawback. It forces the lecturer to follow the text more closely and, in a way, to cooperate "creatively" with the author: to invent new examples and exercises and to show to the students the various relationships between other methods of reasoning and the axiomatic one.

I could add here, as an anecdote that I happened to write some time ago several short and extremely simple computer programmes which aimed at helping the student learn some basic principles of logic. One of them was based on the rules presented in Batóg's *Fundamentals* and it dealt with transforming the abstract formulas of the sentential (or propositional) calculus into their conjunctive normal forms. It was interesting to note that the structures generated by the programme were at every stage identical with the examples given in the *Fundamentals*, though they often differed from the examples found in many other handbooks of logic. I take this as proof of that the author of *Fundamentals* was in fact as precise and exact in his handbook as he had promised in the Introduction.

Batóg's *Fundamentals of Logic* had two favourable reviews in *Ruch Filozoficzny* ([*Philosophical Movement*], vol. XLVI, No. 2, 1989; by Krystyna Piróg-Rzepecka) and in *Wiadomości Matematyczne* ([*Mathematical News*], XXVIII, No. 2, 1990; by Wiktor Bartol). Both reviewers presented a thorough analysis of the contents of the individual chapters of the handbook, and both stressed the didactic merits of *Fundamentals*. Krystyna Piróg-Rzepecka says:

> T. Batóg's handbook shows the meticulous care its author took to make the exposition both understandable and interesting. The author's numerous comments also evidence his intention to make the book as clear as possible.

> Especially useful will — in my opinion — be the author's comments on the generalizing and particularizing interpretations of the notion "free variable" (which he attached to his theorem 4.1), as well as his remarks on the neutrality of logic with respect to extra-logical constants, and his notes on the notions of "consistency" and "completeness" and on the methods of proving the completeness of axiomatic systems (appended to his analysis of the elementary inequality theory). His comments on certain semantic notions (e.g., on interpretation, semantic models and truth) seem to be equally valuable. Furthermore, in no other Polish book on logic could I find a more lucid presentation of a proof of the Gödel's model existence theorem (which undoubtedly is one of the most important metalogical theorems) than in Batóg's "Fundamentals". I also liked the allegorical interludes which serve as illustrations to, for instance, the conclusions resulting from Gödel's theorem. Finally, I think that the theory of definitions — presented in the last chapter — is bound to remove the many doubts students might have on the subject of formulating logically well-formed definitions.
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> Summing up, I would only like to repeat some general comments on T.Batóg's handbook. I am convinced that — as a handbook - *Fundamentals of Logic* is flawless both formally and substantially. Its terminology and notional apparatus are precise and, though "only" a handbook, the book is clearly innovative. Its numerous inductive proofs (in which not a single step in the reasoning process has been omitted) make it easier for the reader to "get the feel" of logical reasoning. As do the comments attached to the "dry logical formulas". Finally, the handbook is written in simple, clear and stylistically beautiful Polish.

I would also like to quote here two short fragments from Wiktor Bartol's review:

> As the author states in the Introduction to his *Fundamentals*, the book is primarily aimed at students of mathematics and philosophy, though no previous knowledge of either mathematics or logic on the reader's part is assumed. ...
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> The author of a logical book conceived of in this way has to be very careful in guiding the readers so as to have them avoid the many traps of the field. And it must be stressed at the very beginning of the review that Tadeusz Batóg outstandingly succeeded in doing just that. At the same time he seems to have accepted the principle of avoiding all simplifications. Instead, he tries to explain and present all the links which are necessary for the reader to understand the notion or theorem discussed. The method seems to be

particularly effective with inductive proofs, where the author is especially careful in formulating the inductive statements.

Another important way of making his exposition easier to follow is the inclusion of well chosen examples to illustrate particular notions or methods analyzed. Thus, the reader is expected to understand how the axiomatic system functions thanks to the huge number of proved statements of both predicate calculus and sentential calculus presented in the handbook whereas his proof of completeness of the theory of dense linear orderings without the first and last elements illustrates the essence of the method of quantifier eliminations and non-triviality completeness proofs.

The UAM (University) Scientific Publishers published in 1994 a new (corrected and enlarged) edition of the handbook. The new sections discuss, among others, such subjects as: relativization of quantifiers, axiomatic foundations of metamathematics, duality, inductive definitions, and a method of introducing definitions in sentential calculus by means of appropriately choosing the valid rules of inference (TB 54).

In 1965 Tadeusz Batóg published (together with Seweryna Łuszczewska-Romahnowa) two papers dealing with generalized classification theory (TB 9, 10). They may be viewed as a sequel to some earlier works published by Łuszczewska-Romahnowa which analyzed multilevel classifications and generally aimed at formalizing the notion of "natural classification". Here is what Professor Łuszczewska-Romahnowa has to say on the contents of the two 1965 papers:

> The authors of the papers having first introduced the notion of a generalized classification of a transfinite type try to show the relationships between transfinite classifications and the so-called Boolean metric spaces, between some wide family of classifications and common metric spaces, and between so-called "classification types" and certain arithmetical classes (in the sense Tarski used the term). (Łuszczewska-Romahnowa 1973, p. 194)

All these notions have turned out to be useful in defining the notion of the distance between individual units of a sound system of a natural language and were used by Maria Steffen-Batogowa in her papers (which we have mentioned earlier). The central definition of the two 1965 papers is as follows:

We shall call the sequence $\boldsymbol{F} = \langle F_\alpha \rangle_{\alpha<\nu}$ a *ν-type classification* (or a ν-level classification; α and ν stand here for ordinal numbers) of the set S, if and only if $\boldsymbol{F}$ is a sequence of families of non-empty sets that conform to the following conditions:

(i) $F_0 = \{S\}$,

(ii) for any $\alpha < \nu$, the family F_α consists of pairwise disjoint sets,

(iii) for any $\alpha < \nu : \bigcup F_\alpha = S$,

(iv) for any α such that $\alpha + 1 < \nu$: $F_\alpha \neq F_{\alpha+1}$,

(v) for all α, β such that $\alpha < \beta < \nu$ and for any $X \in F_\beta$ there is an $Y \in F_\alpha$ such that $X \subseteq Y$.

Theorem 5.1 in TB 9 presents a generalization of the standard abstraction principle, which defines the relationship between a partition of a set and the corresponding equivalence relation.

TB 41 is an encyclopedic entry, in which Tadeusz Batóg discusses the general theory of sets. The author presents here Zermelo's axiomatic system (with all the corrections introduced later on by others). Then, he includes a brief history presenting the development of set theory as an independent mathematical discipline, followed by a discussion of the terms and theorems which are fundamental for the discipline. The entry ends with some notes on the relationship between set theory and the foundations of mathematics.

On September 27, 1977 Tadeusz Batóg gave an inaugural lecture to the students of the Institute of Mathematics during the ceremony which began the 1977/78 academic year at Adam Mickiewicz University in Poznań. Later on, the lecture was published as TB 32. The paper shows Batóg's didactic mastery: he managed to present — in a brief speech devoid of any formal machinery — all the problems considered to be fundamental for contemporary mathematics. Additionally, he included into the speech his original views on the relationship between mathematics and logic. I think it most appropriate to quote the closing section of the text here:

> Many years have passed since the various systems of the general theory of sets were born and since Gödel or even Cohen published the results of their studies. And yet the troubles at the foundations of mathematics with which we all have to cope have not become any less serious. However, from the upper floors of the tower of mathematics, we can at the same time hear rejoicing over both glorious theoretical triumphs and the constantly widening scope of the applications of mathematics. Thus the picture of mathematics implied by what was said above seems to be disturbing; a huge tower supported by weak and insecure foundations. Personally, I do believe that this is simply a wrong picture. And because the picture could have only resulted from a common acceptance of a popular misbelief — whose origins go back to the beginnings of the present century — namely, a belief which identifies mathematics with set theory and set theoretical analyses of structures. Thus, we probably have no other way out but to reject this mistaken belief itself.

All the theoretical problems we have to cope with today at the foundations of mathematics will — in my opinion — have to disappear as soon as we have stopped identifying mathematics with set theory and start looking at it as logic, taken in the narrowest sense of the term, that is as logic viewed as classical logical calculus. Furthermore, it is not difficult to explain why an identification like that is justifiable. The identification finds its justification in the so called deduction theorem. The theorem states that if $A_1, A_2, \ldots, A_n$ are statements of any kind, and if T is a consequence of these statements, then the implication:

(1) $(A_1 \wedge A_2 \wedge \ldots \wedge A_n) \rightarrow T$

is a law of logic. Thus if $A_1, A_2, \ldots, A_n$ are axioms of any theory, set theory, for example, and if T is a theorem of the theory, then the implication (1) is also a logical law. Therefore, proving theorems within any axiomatic theory must always mean proving certain logical laws. In exactly this sense the whole of mathematics is nothing but a huge treasure box full of the laws of logic.

It should also be stressed at this point that — given the approach to mathematics like the one presented above — it is theoretically irrelevant whether the $A_1, A_2, \ldots, A_n$ axiom system is contradictory or not. Neither does it matter if the system is or is not complete. What does matter is that the logic itself, which underlies the axiomatic systems, is non-contradictory and that we have an absolute, finitistic proof of its consistency. It is also important that the system of logic which we know at the moment is complete, in the sense that its rules let us derive all the laws of logic.

The nineteenth century — having created non-Euclidean geometries — put an end to the belief that Euclidean geometry was the only a priori science able to account for our spatial reality. Thus, to save the unity of mathematics, it then became indispensable to submerge all the geometries in set theory and view them as definitional constructs accounting for certain structural wholes. Today, when we can see more and more competing set theories appear, it again seems indispensable to deprive set theory of its status of the foundation on which the whole of mathematics rests. And to maintain the unity of mathematics, we should submerge all its theories in logic in the way shown above.

Two short logical papers published by Batóg in the seventies (TB 21, TB 28) are again very good examples of his meticulousness. TB 21 is a thorough analysis of the "proof" of an alleged contradiction of the theory of types discovered by

A. Dumitriu (on account of its highly technical nature we shall not go into the contents of TB 21 in this paper).

In TB 28 Batóg notes that nowhere in the literature on the subject has the operation of replacing functor variables (in sentential calculi with quantifiers which can bind sentence variables and functor variables) been clearly defined. The paper introduces a definition of that kind and it also provides an appropriate rule of inference. Batóg's approach presented in TB 28 can be adapted (with some obvious modifications) to richer languages (e.g., to languages based on the predicate calculus in particular).

Concerning Tadeusz Batóg's works on history of logic, one finds among them primarily biographical notes on the lives and works of logicians he either worked with or met at Poznań University. These include: TB 35, 43 — on Seweryna Łuszczewska-Romahnowa, TB 14 on Adam Wiegner, and TB 37 devoted to Kazimierz Ajdukiewicz. There are also some longer papers which discuss the theoretical views and contributions to logic of such logicians as: Wiegner (TB 13, 16), Ajdukiewicz (TB 38, 49) and Stanisław Piątkiewicz (TB 20, 22, 50) [whose works were "re-discovered" by Batóg]. We should also mention here an occasional paper published in the cultural magazine *Nurt* [*Current* — a popular weekly, prestigious at the time; TB 17] and devoted to Bertrand Russell. The paper appeared soon after the philosopher's death. In the paper, Tadeusz Batóg analysed the so called "Russellian logicism" and presented two possible interpretations of the phenomenon. One of the approaches, called by Batóg "the strong thesis of logicism" could be reduced to the following two statements:

1. All mathematical notions are definable with the help of purely logical notions; and

2. All theorems of mathematics are derivable from the principles of logic.

The other approach, termed by Batóg "the weak thesis of logicism", is expressed by the deduction theorem itself:

> Basically, it can be understood in a slightly different way, let us say, that: Every mathematical theory is a set of logical laws, whose antecedents consist of nothing but the conjunctions of axioms of a particular theory, and whose consequents include statements commonly known as the theorems of the theory in question. (TB 17, p. 41)

What Russell seemed to have aimed at was to justify the strong thesis of logicism. The task, however, cannot be achieved, as there are certain theorems of mathematics which are simply impossible to prove without having first accepted, for instance, the (existential!) axiom of infinity which can by no means be said to have the status of a logical axiom. The weak thesis of logicism, on the other hand, has the status of a meta-theoretical theorem which is obligatory for all "reasonably normal" logical calculi. In Batóg's opinion, Russell was not clear enough in differentiating the two theses of logicism as they were presented above. Furthermore, he had never presented in any of his writings a clear general formulation of the deduction theorem, though he was both aware of its existence and referred to it in practice (in *Pricipia Mathematica*, for instance, all the theorems assuming the infinity axiom or the axiom of choice have the form of implications whose antecedents consist exactly of one of those axioms).

It would not be — in my opinion — improper to conclude that Tadeusz Batóg has shown in his writings a strong feeling of admiration for the deduction theorem. In his *Fundamentals of logic*, he stresses the importance of the law (p. 123). He also tries — painstakingly and conscientiously — to find the real author of the theorem. On October 24, 1983 he gave a lecture at the meeting of the Poznań Section of the Polish Philosophical Society [Polskie Towarzystwo Filozoficzne] in which he analysed the early logical works of Ajdukiewicz (TB 38). As we know, Tarski — who was the first to announce in print the discovery of the deduction theorem (in 1930) — did admit (though only in 1956) that his discovery of the law (in fact as early as in 1921) was influenced by Ajdukiewicz's works. Batóg noted in his lecture that if we analysed the definition of logical implication which Ajdukiewicz had formulated in 1921, we must come to the conclusion that its nature is not semantic. Here is the definition (Batóg quotes it after Ajdukiewicz's *Logical Foundations of Teaching*):

> Sentence B follows logically from sentence A if and only if the implication which has sentence A as its antecedent and sentence B as its consequent is only a special case of a certain general law of formal logic having the shape of a formal implication.

If L stands for a set of all laws of logic, and if $Cn(X)$ stands for the set of all the sentences which logically follow (in the sense of Ajdukiewicz's definition) from the sentences of some set X, then the definition itself could be given a following symbolic form:

(*) $B \in Cn(\{A\}) \leftrightarrow (A \rightarrow B) \in L$

Then, if we assume — as did Tarski in his 1930 work — that Cn is an operation of the logical consequence determined jointly by the axioms of logic and the rules of inference, then what we get is the metalogical theorem we are interested in; namely, the deduction theorem. Thus, it seems that Ajdukiewicz makes use of the deduction theorem in exactly the same way that the authors of *Principia Mathematica* did (for instance, in his paper from 1926 "Assumptions of traditional logic," when Ajdukiewicz comments on the ways of avoiding the axiom of name non-emptiness).

Summing up his views on Ajdukiewicz's role in the discovery of the deduction theorem, Batóg states:

> Therefore, we can have no doubts that Ajdukiewicz was aware of the validity of the deduction theorem, though he clearly underrated its importance. That is why he did not formulate it in the form of a general principle, but rather used the formula (*) as a definition of logical consequence. So Tarski deserves the credit not so much for the discovery of the theorem, as for his proper appreciation of its importance and for publishing it as an important metalogical theorem.

In the same paper (TB 38), Batóg draws the reader's attention to the fact that the rule of infinite induction, also known as Carnap's rule (Carnap did use it in one of his works in 1935, but so did Tarski in 1933 where he attributed the rule to Hilbert's paper of 1931), can be found in Ajdukiewicz's handbook "Basic Principles of Methodology of Science and Formal Logic" (Główne zasady metodologii nauk i logiki formalnej) which was published in 1928. The rule was called "the directive of quasi-complete induction" in the handbook. In contemporary literature the rule is usually referred to as the ω-rule and its present-day notation has the following form:

$$\frac{A(0),\quad A(1),\quad A(2),\ldots}{\forall x A(x)}$$

(A stands here for any sentential formula of a language in which we can talk about natural numbers).

Thus it is a rule according to which a general conclusion is inferred from an infinite number of premisses. This rule has important applications not only in the arithmetic of natural numbers. In Ajdukiewicz's handbook the rule was formulated in the following way:

> Every general proper statement can be considered valid if all the statements which come within the scope of that general statement are validated by one of the remaining directives. (pp. 207–208)

Batóg shows (having first explained the meaning of the terms used by Ajdukiewicz; we shall omit here that part of his exposition) that the two formulations of the rule are identical. Then, he adds that Ajdukiewicz was clearly aware that the two discussed rules differed from other "standard" rules (such as for instance the detachment or modus ponens rule). "Ajdukiewicz noted — writes Batóg — that difference in a brief comment where he stated that the ω-rule 'does not conform in its formulation to the structuralness condition' (Ajdukiewicz 1928, p. 208)". (TB 38, p. 141). Incidentally, as I just happen to have the venerable text by Ajdukiewicz in front of me, I would like to draw the reader's attention to the sentence which directly follows Ajdukiewicz's statement quoted by Batóg: "The directive of quasi-complete induction could be reformulated so that it would become a structural directive. That new formulation would however be much more complicated."

Commenting on Ajdukiewicz's writings, Batóg naturally does not omit his *Die Syntaktische Konnexität*, the paper which won its author the title of the father of mathematical linguistics. In connection with that I have no doubts that Professor Batóg has been genuinely pleased to be able to refer to the many publications of his students (Wojciech Buszkowski, Maciej Kandulski and Wojciech Zielonka), who have been working on problems pertaining to the completeness, and axiomatizability of the systems which have been termed Ajdukiewicz-Lambek systems or the calculi of syntactic types.

In his work describing Adam Wiegner's logical attainments (TB 13), Tadeusz Batóg draws the reader's attention to an original system of axioms invented by Wiegner to account for the two-valued propositional calculus (Batóg stresses its didactic values), and to the task in which Wiegner had been primarily interested throughout his professional life, namely, a reconstruction of traditional logic by means of tools offered by modern logic. Then, Batóg discusses one of Wiegner's last papers, devoted to semantic analyses of such notions as abstraction, idealization, generalization and concretization, and so on. Some years after TB 13 had been published, Tadeusz Batóg offered his own version of logical analyses of the same set of notions in his polemic with methodological conceptions propagated by Professor Leszek Nowak (see the section discussing Batóg's methodological studies). The controversy became famous in the community of Poznań logicians.

In my opinion, however, Tadeusz Batóg's most interesting accomplishment in the field of history of logic (so far) has been his "discovery" of Stanisław Piątkiewicz, who should be considered a pioneer of mathematical logic in Poland (TB 20, 22, 57). It became customary for those writing on the subject to associate the beginnings of mathematical logic in Poland with either Kazimierz Twardowski or Jan Łukasiewicz. Thus, Ajdukiewicz, for instance, assumed that the beginnings went back to the academic year 1899/1900 when Twardowski mentioned in his lectures the algebra of logic for the first time. Ingarden and Kotarbiński seem to agree with Ajdukiewicz. Twardowski's and Łukasiewicz's remarkable contribution to the field notwithstanding, the information would imply that mathematical logic had been totally unknown in Poland before 1899. And that is not the case. As early as in 1878 a Polish translation of Bain's *Logic* was published in Warsaw in which both De Morgan's and Boole's theories were dealt with at length (and we cannot be certain that no other translations appeared at around the same time). It was just that translation that interested Stanisław Piątkiewicz in algebraic logic.

Piątkiewicz was born on September 21, 1849 in Dębowiec, near Jasło. He graduated from Lwów University where he had studied mathematics and physics in the years 1867–1871. Then, he taught both subjects first at a lycée in Przemyśl (1872–1879), then in a grammar school (Gymnasium No IV) in Lwów (1879–1890; since 1883 he also taught logic there), and then again in Przemyśl (1890–1906) as the Headmaster of the lycée. While teaching in Lwów, Piątkiewicz became familiar with Bain's book and in 1888 published a lengthy paper entitled *Algebra w logice* [*Algebra in Logic*; Lwów 1888, Nakładem Funduszu Narodowego]. Unless an earlier work on the subject is found, we will have to accept Piątkiewicz's monograph as the first original Polish contribution to mathematical logic. The author is surprisingly well-read in the contemporary literature of the discipline (Stamm, Łukasiewicz and Chwistek will show a comparable level of logical competence only 20 years later). Piątkiewicz presented in his *Algebra in Logic* a number of arguments both to show that it is possible to widen the scope of contemporary formal logic by approaching it in the "algebraic way" and to prove that the language of algebra is more suitable for logic than the everyday language used by logicians. The monograph discussed among others: the calculus of classes (including such operations as addition and multiplication of classes, as well as the complement of a given class), and a detailed presentation of Schröder's method of logical equations. Piątkiewicz also tries to apply the calculus of classes to syllogisms (in which he allows for sentences with negated subjects and predicates). Generally, *Algebra of Logic* is proof of its author's logical competence and his practical mastery in making use of formalizations. Some minor inaccuracies (of a mainly methodological nature) were noted by Batóg.

In TB 57 Tadeusz Batóg together with Roman Murawski aim at presenting Piątkiewicz's life and works in detail.

2 Logical Foundations of Phonology

Tadeusz Batóg's works devoted to a logical reconstruction of the theoretical foundations of phonology have undisputedly won the scholar his greatest renown so far. Consequently, they must be considered his most important contribution to the world of science.

This section will deal with the following: TB 3, 4, 5, 6, 12, 15, 18, 19, 26, 30, 33, 34, 36, 44, 46, 48, 55, and TB 58.

Most of these works deal either with a logical reconstruction of the notion of the phoneme, or with the forming of axiomatic foundations of segmental phonology. The reader can find a more detailed presentation of Tadeusz Batóg's approach to phonological systems in another paper by the same author included in this volume. Here, I would like to present a rather general view of Batóg's theoretical approach to phonology and (unlike in the other paper) draw the reader's attention to his recent papers on algorithmic processes for determining phonemic bases. The reader who feels that the latter subject has been treated too sketchily here is kindly referred to Włodzimierz Lapis' paper (also included in this volume) which contains a detailed analysis of TB 48[2]

At the turn of the fifties and sixties in some countries of Central and Eastern Europe several important works on mathematical models of language appeared. On account of their notional and terminological uniformity (their authors made use of either algebraic or set-theoretical apparatus), the works were included into the so called analytical (or algebraic) school of linguistics. Among the most influential representatives of the trend were: Kułagina, Marcus, Revzin, Gladkiy, Uspienskiy and Dobrushin. Several of their publications dealt with phonological problems (e.g.: works by Marcus, Revzin, and Uspienskiy). Naturally, works also appeared, which discussed mathematical foundations of phonology in other parts of the world (e.g. Greenberg 1959; Kanger, and others).

The above digression is meant to draw the reader's attention to the fact that at the time we are referring to, all problems pertaining to working out the mathematical foundations of phonology were considered urgent and important. Having become interested in the subject, Tadeusz Batóg managed to come out with a number of valuable results considerably earlier than did many other scholars working in the field. What is more, his works and solutions offered therein reached — within a relatively short period of time (the dissertation TB 3 was sent to the

[2]In 1994 UAM Scientific Publishers [Wydawnictwo Naukowe UAM] published in English in its *Linguistics Series* [*Językoznawstwo*] Batóg's *Studies in Axiomatic Foundations of Phonology*. The collection includes: TB 3, 4, 6, 15, 18, 19, 26, 30, 34, 36, 44, 46 and 48.)

publishers on June 13, 1960 and the monograph TB 12 was published in 1967) — a level which by far surpassed that of all other competing solutions formulated at the time.

Greenberg's first attempt at formulating the axiomatic foundations of phonology was published as early as 1959 (Greenberg 1959). The paper, however, had many mistakes and inadequacies which Batóg pointed out in TB 4 with the meticulousness so characteristic of all his works.

It seems that one of the most important reasons which made even Batóg's first work on phonology surpass other approaches was the methodology developed by the scholar. Batóg's method consists of outlining a logical reconstruction of segmental phonology. This approach assumes that it is indispensable (1) to enumerate explicitly all the primitive terms of a given phonological theory, (2) to reconstruct all the implicit assumptions (assumed without any proof), and (3) to determine the status of all the other terms and statements of the system in such a way that each of the former will be given a clear definition, whereas the latter will be accompanied by formal proofs. Thus, it seems that the author (unlike many others) wanted from the very beginning to develop a complete system of formalized theoretical phonology rather than simply to make another contribution to current phonological discussions.

Batóg found his linguistic starting point in structural linguistics — first, in the works of American distributional linguists (Harris and his followers). Then, in more elaborate versions of his theory, he began to pay more and more attention to European linguistic structuralism, especially to the works of N.S. Trubetzkoy.

Concerning the formal logical apparatus which Batóg makes use of in his systems, it includes — in addition to the standard set of notions belonging to the predicate calculus, the calculus of classes and of relations — a set of concepts of the Leśniewski-Tarski system known as the extended mereology. Mereological concepts seem to be particularly useful in describing language phenomena at the parole level. For mutual relations between concrete utterances and their fragments are based exactly on the part-whole relationship and on the temporal ordering of individual elements of a given whole which extended mereology aims to account for. Tadeusz Batóg deserves credit for popularizing that kind of formal device in linguistics. The extended mereological system was earlier applied in biology by J.H. Woodger in *The Axiomatic Method in Biology* (Woodger 1937). In his appendix to the book, A. Tarski suggested a way of extending Leśniewski's original mereology so that it could also account for certain temporal relations. It seems that Batóg intentionally referred in the title of his TB 12 monograph to Woodger's work.

The aims that Batóg seems to have set himself in all the works in question is to characterize in precise terms the notion "phoneme" and to establish the unequivocal axiomatic foundations on which theoretical phonology could be based. In my opinion, it is possible to find three distinct stages in the development of Batóg's phonological theory, namely, the stage of:

1. logical reconstruction of the notion of the phoneme, which at that stage would be based on exclusively distributional criteria (TB 3, TB 6);

2. accounting for — in his approach to phonemes — the fact that each speech sound can be assigned a set of phonetic features (TB 12); and the stage of

3. extending his TB 12 phonological system by adding certain semantic aspects to it (TB 19, TB 26).

In the paper by the same author which was mentioned earlier, the reader will find a comparison of the three stages, as well as the crucial axioms and definitions of Batóg's system (as it is presented in TB 12, which seems central within the series of Batóg's works on the subject). As for what follows below in this section, I would like to discuss here one of the most recent logical reconstructions of the notion of the phoneme as it was presented in TB 26 (and in its English version, i.e. TB 34). I must point to the fact that both TB 26 and TB 34 are written in a very simple and straightforward way and that they can be easily understood by linguists having no special training in mathematics. Thus, the list of the primitive (or primary) terms of the system presented in TB 26 includes:

$\boldsymbol{I}$ the set of all idiolects;
$\boldsymbol{O}$ the set of all pauses;
$\boldsymbol{K}$ the family of all kinds of phonetic features;
$\boldsymbol{M}$ the relation of synonymy.

I shall start with the so called intuitional analysis of these notions (and "intuitional" means here "referring to what most professional linguists say on a given subject").

The term "*idiolect*" is used here to refer to a (relatively extensive) set of spoken texts (concrete, individual utterances) which are linguistically homogeneous. Some examples of idiolects conceived of in this way would include, for instance:

— the set of all the utterances produced in the Polish language during some period of time (the period should not be too long, or one would have to account for diachronic changes — let us say, 10 years would be an adequate period) by all the speakers of Polish who speak the same variety of modern, standard Polish; or

— all the texts of all the conversations which could be carried on in one and the same variety of Polish in Poznań on the night of New Year's Eve in 1999 (assuming the set of texts is not empty).

We could also say that such an idiolect is a material representation of some language. Idiolects form a set of direct data for phonetics and phonology. Depending on what purpose or purposes any given analysis is to serve, the analyst may analyse either one particular idiolect or a given set of idiolects.

Then, idiolects consist of *texts* (spoken texts or utterances), understood as elements of a given idiolect and texts will in turn consist of *unit-length segments*. Batóg understands by the latter notion what D. Jones called a *concrete speech sound*, or Z. Harris — a *unit-length segment*, and H. Pilch a *phonematisches Segment*. Batóg's segments are thus the minimal units (or objects) of a sound system. Yet, the notion of a unit-length segment is not a primitive term of his system, as it will be defined in terms of phonetic features (the notion of a phonetic feature will be understood here in its extensional meaning).

Some of Batóg's unit-length segments are *pauses*. The set $\boldsymbol{O}$ is the set of all pauses (which are also called zero segments). Pauses are simply moments of silence (of saying nothing), and they are easily detectable in utterances. The unit-length segments which are not pauses are called *proper segments*.

The elements of the $\boldsymbol{K}$-family are called by Batóg *kinds of phonetic features*. They correspond to the articulatory, acoustic and auditive dimensions or parameters which linguists make use of while describing sound systems of various languages. Every kind of phonetic features consists of a finite number of elements — i.e., some number of features of a particular kind. Thus, for example, such features as "stopness", "affricateness", "fricativeness", "half-openness" and "openness" would all belong to one and the same phonetic parameter (or dimension), which accounts for the degree of supra-glottal opening.

The set $\bigcup \boldsymbol{K}$ is identical with the totality of phonetic features. Phonetic features themselves are understood extensionally as classes of unit-length segments to which the features can be attributed. Thus, the feature of "voicedness", for instance, will be identical to a set of all the voiced unit-length segments. Such an extensional approach lets one define the set of all unit-length segments simply as the set $\bigcup \bigcup \boldsymbol{K}$.

Each text in every idiolect is treated as a finite chain of unit-length segments ordered linearly by means of the relation of temporal succession (and some of the segments may be pauses).

Within a text, *phrases* and *words* are distinguished. Phrases in a given idiolect are those fragments of texts of the idiolect which are contained between any two consecutive pauses (and thus contain no internal pauses). Thus, a phrase is a chain consisting exclusively of proper segments. A definition of the term "word" is slightly more complicated and as such it will be disregarded here (but see the article by the same author in this volume for this definition). Suffice it to say here, that every phrase is taken to be also a chain of words — which are physical concrete entities of which the phrase consists.

Individual texts (phrases, words, and unit-length segments) can be juxtaposed and grouped together in various ways in order to find similarities and differences between them. It is only natural that a phonetician will be primarily interested in the relationships between the units of a sound system which are based on the phonetic features ascribed to those units, as well as on their distribution (i.e., based on the comparison of the contexts in which the units appear). Of particular importance among all these relations is the relation of *homophony* (or phonetic equivalence). Homophonous segments are segments which do not differ from one another in any phonetic feature (out of the predetermined $\bigcup \boldsymbol{K}$ set of features). The system of axioms with which Batóg has equipped his approach makes it possible to define the relation of homophony for any text or set of texts. Homophony is obviously an example of the relation of logical equivalence. Additionally, it has the following two important properties:

1. if x is a chain (a phrase or a word) consisting of the unit-length segments $x_1, \dots, x_n$ following each other in this order, and if y is a chain which consists of the unit-length segments $y_1, \dots, y_n$ following each other in this order, and x_i is homophonous with y_i (for $1 \leqslant i \leqslant n$), then x and y are homophonous; and

2. if a chain x is homophonous with a chain y, then it is possible to break the chain x into consecutive unit-length segments $x_1, \dots, x_n$, and the chain y into the segments $y_1, \dots, y_n$ respectively, in such a way that x_i is homophonous with y_i (for $1 \leqslant i \leqslant n$).

Speech sounds of a given idiolect are abstraction classes of the relation of homophony on the set of unit-length segments. Thus, each speech sound is a maximum class of homophonous unit-length segments.

As each phrase is a linearly ordered chain of unit-length segments and each unit-length segment belongs to exactly one speech sound, then, as a result, each phrase can be assigned exactly one sequence consisting of the speech sounds which include consecutive unit-length segments of the phrase in question. If, for instance, some phrase x consists of the $x_1, \dots, x_n$ unit-length segments (in that temporal order), and if X_i is a speech sound which includes the x_i segment, then the sequence $(X_1, \dots, X_n)$ is called a *phonetic structure* of the x phrase (in a given idiolect). Phonetic transcriptions of texts consist in producing their phonetic structures.

The last notion to be used here is the relation of *synonimity* (or meaning equivalence) M. The relation may hold between phrases, words and texts — all understood as concrete physical objects. By treating synonymy as a relation between individual utterances, Batóg adheres to the opinion that utterances acquire their meaning only in use — when they have been uttered by a concrete person and in a definite situation.

It is assumed that synonymity is a logical equivalence relation. The *actual meaning* of a given word (phrase, or text) is the equivalence class of the relation including the word (phrase, or text). Thus, the actual meaning of a word corresponds to the meaning in which the word has been used.

A *potential meaning* of a word (phrase, etc.) x is any actual meaning of an expression homophonous with x. Thus, potential meanings of an expression correspond to all the meanings in which the expression may be used in a given idiolect. An expression may have more than one potential meaning. The actual meaning of an expression is naturally one of its potential meanings.

Before we start analysing the most important definition of Batóg's approach, that is the definition of a phonemic basis, it might be convenient to introduce two auxiliary terms, which will facilitate the discussion.

Let us suppose now that $\mathfrak{B}$ is a classification of the set of all speech sounds of some idiolect, and that $(X_1, \ldots, X_n)$ is the phonetic structure of some phrase in that idiolect. Then we shall use the term $\mathfrak{B}$-*structure* of the phrase (with respect to the idiolect in question) to denote the sequence of sounds $(X_1, \ldots, X_n)$ such that X_i is that element of the classification $\mathfrak{B}$ to which the speech sound X_i belongs.

It is easy to observe that $\mathfrak{B}$-structures of phrases are formed in a way analogous to the way phonetic structures are formed. It is also obvious that a $\mathfrak{B}$-structure of any phrase is always determined unambiguously.

The other auxiliary concept refers to families of sets. If $\mathfrak{A}$ and $\mathfrak{B}$ are two distinct families of sets, we shall say that $\mathfrak{A}$ is summably reducible to $\mathfrak{B}$ if and only if each set Y belonging to $\mathfrak{B}$ is the set-theoretical sum of some sets belonging to $\mathfrak{A}$. It is essential for the concept defined here that if the family $\mathfrak{A}$ is summably reducible to $\mathfrak{B}$, then $\mathfrak{A}$ cannot contain any element that would be a more extensive set (in the sense of proper inclusion) than some element of the family $\mathfrak{B}$.

Now we shall introduce the definition of a phonemic basis as it was formulated by Batóg (TB 26, p. 8):

> A family $\mathfrak{B}$ of the families of sets is a *phonemic basis* of the idiolect ι, if and only if the family satisfies the following conditions (postulates):
>
> (1) *Postulate of classification*. $\mathfrak{B}$ is a classification of the set of all sounds of

the idiolect ι, i.e. each element of the family $\mathfrak{B}$ is a non-empty set of sounds of the idiolect ι, and each sound of the idiolect ι belongs exactly to one set in the family $\mathfrak{B}$.

(2) *Postulate of free variation*. All sounds of the idiolect ι which are free variants of a given sound X (i.e. the sounds replaceable for the sound X in all phonetic structures of phrases of the idiolect ι, in the sense that the replacement always transforms one structure of a phrase into another structure of a phrase) belong to the same set in the family $\mathfrak{B}$ as the sound X.

(3) *Postulate of complementary distribution*. For each two sounds X and Y of the idiolect ι, if these sounds belong to the same set in the family $\mathfrak{B}$, then either X is a free variant of the sound Y, or the sound X is in complementary distribution with the sound Y (i.e. the replacement of the sound X for the sound Y is inadmissible in any phonetic structure of a phrase, in the sense that such a replacement always transforms a phonetic structure of a phrase into a sequence of sounds which is not a phonetic structure of any phrase).

(4) *Postulate of distinctiveness*. For each set $\boldsymbol{X}$ of sounds in the family $\mathfrak{B}$ there exists a class of features (the so-called distinctive features) such that each sound that belongs to $\boldsymbol{X}$ has all the features of this class, and each sound that does not belong to $\boldsymbol{X}$ lacks at least one of the features.

(5) *Postulate of differentiation*. $\mathfrak{B}$-structures of each two words that have different sets of potential meanings are different.

(6) *Postulate of economy*. The family $\mathfrak{B}$ is not summably reducible to any family $\mathfrak{B}'$ which would also fulfill the postulates (1)–(5).

Elements of arbitrary phonemic basis of a given idiolect are called *phonemes* of this idiolect with respect to this basis.

As evident from the passage above, the system allows for non-unique phoneme assignments, i.e. it permits the existence of different phonemic bases (different systems of phonemes) for the same idiolect.

The following sentence (H) Batóg terms THE FUNDAMENTAL HYPOTHESIS OF PHONOLOGY:

(H) Every idiolect has at least one phonemic basis.

The sentence is independent of any of the axioms found in Batóg's phonological systems. Thus it must be considered an empirical hypothesis which can be refuted. Yet all the phonological analyses that have been offered so far seem to confirm the thesis. Furthermore, from a purely formal point of view, it would seem very interesting to be able to look for the sufficient and necessary conditions for the existence (and uniqueness) of phonemic bases. In my opinion one would have to use for that certain tools offered by algebraic linguistics and the theory of information systems.

Let us now supplement the definition quoted above with some comments on its contents. Instead of trying to invent some paraphrases of my own, I will quote here again Batóg's own commentary, which on account of its precision and conciseness is inimitable (TB 26, pp. 9–11):

> Let us suppose now that a given family $\mathfrak{B}$ is a phonemic basis of the idiolect ι, and consider the question of what the phonemes with respect to the basis $\mathfrak{B}$ actually are and what properties they have. The answers, of course, are provided by the above adopted definitions.
>
> According to the postulate of classification each phoneme (in the basis $\mathfrak{B}$) is a non-empty set of sounds and no two different phonemes have common elements and each sound belongs to some phoneme. It is not excluded, however, that particular phonemes can be unit sets, i.e. sets consisting of one sound only. Thus, the conception of phonology presented in this paper excludes the so-called overlapping of phonemes, due to which it creates the possibility of describing phonemes by means of so-called distinctive features.
>
> The postulate of free variation expresses the conviction that the sounds whose language functions differ must — as a result — have different ranges of occurrence, i.e. so-called distribution. In connection with this, the postulate requires that each two sounds that have the same distribution be assigned to one phoneme (i.e. be included in one phoneme). The concept of the discussed postulate can be expressed in yet another way: each equivalence class of the relation of being a free variant (which — as can be easily noticed — is reflexive, symmetric and transitive) is a subset of a phoneme in any phonemic basis $\mathfrak{B}$. While the postulate of free variation is some sort of requirement, the postulate of complementary distribution — contrary to the belief oflinguists — is rather a prohibition. What it prohibits is the inclusion into one phoneme of any two sounds which are neither in free variation nor in complementary distribution. One could raise the following simple question at this point: is it allowed — or is it perhaps, necessary — to assign complementary sounds to one phoneme? The answer to this question is not simple and reads as follows: it is allowed but only if such an assignment does not violate any of the postulates included in the definition of phonemic basis; in such a case, however, the assignment is not only permissible but also necessary (this is determined by the postulate of economy); in all other

cases the assignment of sounds which are in complementary distribution to one phoneme is not only unnecessary but also it is not permissible. It may be worthwhile to add that the fact that some sounds X and Y are in complementary distribution in a given idiolect means that the sound X never occurs in this idiolect in the same phonetic context as the sound Y.

According to the postulate of distinctiveness, each phoneme should be associated with a certain set of distinctive features. Furthermore, features that belong to such a set should be shared by all the sounds which are the elements of the corresponding phoneme. What is more, a sound that does not belong to a phoneme cannot have all the distinctive features which correspond to this particular phoneme. It follows from the above that sets of distinctive features associated with two different phonemes (in the same basis) always differ and none of these sets is a subset of the other. Again, the postulate of distinctiveness thus characterized has the nature of a prohibition which does not permit too arbitrary a classification of sounds of a given idiolect to be considered a phonemic basis. It is, however, noteworthy that such a formulation does not settle the fact that the association of distinctive feature sets with individual phonemes must be univocally determined. It only requires that an appropriate association exist but it does not exclude the existence of various associations of this type. ...

The postulate of differentiation is characteristic of European phonology, which originated from the tradition of the classical monograph by Trubetzkoy (Trubetzkoy 1939). It allows only such an 'identification of sounds on the phonological plane' (i.e. association of sounds with one phoneme) which does not result in the identification of phonological structures of words that have different potential meanings. American phonology of Harris' type tried, in principle, to avoid adopting the concept of meaning when defining the concept of phoneme. It would be in accordance with this type of phonology if the postulate of differentiation was to be replaced by the following postulate of one-one representation:

(5') *Postulate of one-one representation*. $\mathfrak{B}$-structure of any phrase univocally (up to free variants) determines the phonetic structure of this phrase.

The last item to be discussed here concerns the postulate of economy. Roughly speaking, this postulate requires the minimalization of the number of phonemes in the framework set up by other postulates. To be more precise, it says that if a family $\mathfrak{B}$ fulfills the conditions (1)–(5), it can be accepted as a phonemic basis of the idiolect ι only if it cannot be summably reduced to any other family $\mathfrak{B}'$ which would also satisfy the conditions (1)–(5). It should be noted here, that such a formulation of the postulate of economy implies that one idiolect can have two or more different phonemic bases differing not only as to the quality but also as regards the number of elements. In this case, of course, none of them can be summably reducible to another.

Then, referring again to the postulate of distinctiveness Batóg tries to characterize the phoneme in yet another way. What I mean here is his definition of the term *phonological system*. For Batóg, a phonological system of an idiolect ι is an ordered pair $(\mathfrak{B}, \mathbb{F})$ in which $\mathfrak{B}$ is a classification of the set of speech sounds of the idiolect ι which fulfills the postulates (2), (3), (5) and (6) of the definition of a phonemic basis, and in which $\mathbb{F}$ is a function whose domain is $\mathfrak{B}$ and whose values are sets of phonetic features of sounds. The function fulfills the following condition: for each set of sounds $\boldsymbol{X} \in \mathfrak{B}$, and for any sounds X and Y, if $X \in \boldsymbol{X}$ and if $Y \notin \boldsymbol{X}$, then X has all the features in the set $\mathbb{F}(X)$ whereas Y does not have at least one feature in the set $\mathbb{F}(X)$. Here $\mathbb{F}(X)$ is the value of the function $\mathbb{F}$ for the argument X.

The function $\mathbb{F}$ is called the *distinctivizing function* of a given phonological system. The concept of a phonological system may be useful in all these cases in which the elements of the same phonemic basis would be characterized by means of different assignments of groups of distinctive features. A situation like that could arise when, for instance, two linguists agree as to the set of phonemes which characterize some idiolect, but suggest two distinct sets of phonetic features to describe the phonemes of the idiolect.

As an example of a phonemic basis, Batóg presents the 39 phonemes of the Kraków-Poznań variety of the modern standard Polish. The example is taken (with small alternations) from MSB 16, a work by Maria Steffen-Batóg. The base assumes that free variation is an identity relation in Polish (it contains no different sounds which would be in free variation). Yet in order to illustrate the phenomenon of what he calls "non-trivial free variation" Batóg considers a hypothetical situation in which the Polish uvular R is a free variant of the standard front r. In such a case the phoneme /r/ would have the following eight sounds:

[r]	[r̦]	[r']	[ŕ]
[R]	[R̦]	[R']	[Ŕ]

Each two sounds placed in the same columns would be in free variation, whereas each two sounds placed in different columns (irrespective of the line) would be in the relation of complementary distribution.

Let us now draw the reader's attention to the importance that Batóg's ideas may have for theoretical phonology in general. I would like to start with the author's opinion. Here is what Batóg has to say in the conclusions of his monograph (TB 12, p. 120):

> What are the principal results of this work? We consider the following can be listed: the submission of the set of primitive notions which suffice to define almost all other notions of phonology, the submission of axiomatic characterization of these notions, arrangement of conceptual apparatus of phonology, analysis and definition of the notion of a unit-length segment, precise formulation of the principles of distribution and above all analysis of the notion of a phoneme and formulation and discussion of fundamental hypotheses of phonology.

Then, I would like to refer to what in my opinion must be considered the most competent analysis of Tadeusz Batóg's linguistic works, that is to F.H.H. Kortlandt's monograph, *Modelling the Phoneme* (Kortlandt 1972). The whole twenty page long Chapter V of the book was devoted to Batóg's early works, and the words opening the chapter have remained valid:

> The formally most elaborate model presented in phonemic theory up to now is the one formulated by the Polish logician T. Batóg.

Kortlandt first presents all the main constructions of the 1967 system. Then a part of the chapter offers a number of detailed critical remarks concerning Batóg's approach. Batóg dealt with the objections in a review of Kortlandt's monograph (TB 25), a fragment of which I would like to quote below. The reader should be reminded, however, that it is absolutely exceptional on the part of Professor Batóg to compliment the authors of works he is reviewing (if one disregards, naturally, the works of such Masters as, for instance, Russell). Here is, however, what Batóg says on Chapter Five of Kortlandt's monograph:

> I must admit that I read the chapter with real satisfaction. For I found that the author presented my ideas accurately and competently which must have been the result of his having understood them thoroughly. And even if — on account of the complexity of some parts of my theory — the author had to simplify some of its details, he would introduce the simplifications explicitly and professionally, fully aware of what he was aiming at. Similarly his criticism of my phonological theory has impressed me favourably. For his critical remarks are clear, matter-of-fact and instructive. I obviously do not mean

> to say by that I wholly agree with all the critical arguments of my opponent, though I do acknowledge that his argumentation is valuable and worth discussing.
> (TB 25, p. 123)

Batóg closes his review of Kortlandt's book as follows (TB 25, p. 128):

> . . . as a whole Kortlandt's book is in my opinion quite valuable. I think that anyone who wants to deal with contemporary theoretical phonology at a level adequate to the methodological sophistication of our times should read the monograph. Logicians and mathematicians could learn some more respect for linguistic facts from it, whereas linguists by reading it would find out a lot on the subject of modern attempts at introducing rigour to phonology and on the indispensability of such attempts.

I fully agree with Professor Batóg's opinion on Kortlandt's book. The appeal, however, implicitly addressed both to the logician and the linguist in the above quoted conclusion of the review has unfortunately received (for over 20 years!) no *adequate* response. I have frequently had the honour of being invited to numerous conferences organized by logicians interested in the fundamentals of linguistics. My two impressions that persist after the meetings: most logicians seem to have little factual knowledge of the languages of the world, and also it is really difficult for them to understand contemporary linguistic approaches (for most of them only certain "initially pre-processed linguistic theories" — such as the works of Montague, for example — would form "raw linguistic data"!). In my own (totally arbitrary and biased) opinion among all the works published by Polish logicians who are currently interested in the formal foundations of syntax, semantics or pragmatics of natural languages only the approaches advocated by Professor Barbara Stanosz (e.g. Stanosz 1991) and Professor Marek Tokarz (e.g. Tokarz 1993) are subtle, sophisticated and valuable. On the whole, most logicians interested in linguistics seem to feel overly restricted by the methodological standards of their own discipline. Yet, I would — somewhat provocatively — claim that natural human language can be identified with free algebra to exactly the same degree as, for instance, the classical sentential calculus is identical with the layers of ink in the manuscript of *Begriffsschrift*. On the other hand, it is my professional duty to take part in the sessions of the Neo-philological Faculty Council. The few fragments of those sessions which happen to be devoted to scientific matters are enough to easily convince even the most stubborn disbeliever that many branches of linguistics are still deeply rooted in methodological standards of the previous century. The puzzles we face here would really be intriguing for the sociologist of science.

Let us now return to Kortlandt's critical remarks and Batóg's comments on them. First of all, Kortlandt was naturally right stating that Batóg's TB 12 system disregarded meaning. While working on his monograph, Kortlandt could not have been familiar with Batóg's other works which were being published at the same time. In particular, he could not have known TB 19 where the definition of phoneme was based on, among others, semantic relations, thus making TB 19 account for the role of phonemes as meaning distinguishers. It should be added here that Kortlandt believes in the existence of the one and only "true" notion of the phoneme (his own), which does not seem to be the happiest methodological standpoint (if one takes into account the fact that many theoretical approaches to the problem have co-existed in linguistics for so long; which in turn seems to further my thesis concerning methodological immaturity of linguistics).

Batóg also agrees with Kortlandt's critical comments concerning the fact that his approach does not allow for interpreting a single speech sound as a sequence of two consecutive phonemes. Yet he points to the historical (diachronic) nature of all the arguments which are usually advanced to justify such bi-phonemic interpretations of sounds. Batóg also shows that his system can account for the phenomenon of neutralization as it assumes the existence of "the same words" having two distinct forms. Similarly, Batóg finds that Kortlandt's criticism of his approach to inseparability of proper segments is justified (the approach was meant to be a logical reconstruction of Harris' ideas).

Other critical arguments voiced in Kortlandt's book were rejected by Batóg (concerning for instance Batóg's "phoneticism", i.e. attaching too much weight to the relation of homophony in his phonological analysis, or pertaining to the problem of natural segmentation and to the question of assigning certain concrete sounds belonging to individual languages to either free variants or sounds standing in the relation of complementary distribution). TB 25 naturally lists the reasons why he finds Kortlandt's argumentation unconvincing.

On July 2, 1968 Professor Roman Suszko wrote a brief but very penetrating review of *The Axiomatic Method in Phonology* (it was a review of the book as Batóg's thesis for *venia legendi*). I would like to quote here the fragment of Suszko's analysis which stresses the importance of Batóg's work for contemporary theoretical phonology:

> The need to use axiomatic methods in linguistics, and especially in phono-

logy, has been felt by many linguists for a long period of time. The attempts, however, which have been made so far to answer the need were fragmentary, immature and inexpert. The axiomatic system of distributional phonology worked out by Dr. Batóg is the first precise and logically fully mature systematization of phonology. The logical foundations on which the system is built are found in the extended mereology of Leśniewski-Tarski and in the calculus of classes and relations. The system explicitly lists its primitive terms characterized by the axioms which are also explicitly listed. It then goes on to deduce its theorems and to define its further concepts. The whole work is formally perfect. Dr. Batóg's aim is to logically reconstruct the distributional theory and that is why the monograph in addition to its formal system juxtaposes and compares the theorems and definitions of the system with the linguistic data found in the works of Harris and other linguists. And at exactly that point we can observe the incomparable double competence of a logician and a linguist shown by Dr. Batóg. ...

Chapters 12 and 13 deal with the fundamental concepts and theorems of distributional phonology: the notion of distribution, relations of complementary distribution and free variation, the concept of a phonemic basis, and some phonological hypotheses. We face the most difficult problems having found ourselves in the midst of phonological controversies. And it is exactly here that Dr. Batóg shows both his mastery of logical analysis and his phonological competence. His definition of the distribution of a speech sound is precise and clear. It must have been quite an achievement to arrive at it on the basis of vague linguistic formulations. Batóg's definition of a phonemic basis is excellent (with a reservation to be voiced below) [Suszko refers here to the form of Batóg's postulate of economy which has been corrected in his later works — J.P.] and his demonstration of the fact that both the existence of only one phonemic basis and the postulate of one-one representation are nothing more than empirical hypotheses (as they do not follow from the phonetic axioms) is in my opinion Batóg's most important discovery. ...

Dr. Batóg's scientific output has until now belonged mostly (though not exclusively) to the borderland of logic and linguistics. To ask whether his works are logical or linguistic, however, would be most improper now at the time when border lines between distinct disciplines are moving. Yet, if someone were to ask me the question, I would have to say that I expect that Dr. Batóg's monograph will turn out to be one of the most important works in phonology. But I do believe that the book cannot have been authored by any of present day phonologists. The monograph entitled *The Axiomatic Method in Phonology* equipped with the logical apparatus with which Dr. Batóg managed to provide it can have in 1968 only one author who happens to be a *prominent scientist working in the domain of logic* [emphasis added — J.P.].

Batóg's axiomatic system has been also allotted a whole chapter in Kłyczkow's book on historical linguistics (Kłyczkow 1975). Yet, I do not think that the author succeeded in understanding Batóg's works which he discussed there. Not to mention the fact that Kłyczkow makes elementary formal errors (here is an example, so that I am not accused of groundless criticism: Kłyczkow sees no difference between the letter U and the symbol denoting the union of sets $\bigcup$).

Next, Hans-Heinrich Lieb trying to logically reconstruct (in Lieb 1979) count N.S. Trubetzkoy's phonological theory refers in one of his footnotes to Batóg's works. He, however, mistakenly assumes there that Batóg's system cannot cope with the ideas pertaining to Trubetzkoy's distinctive oppositions (Batóg does account for them by means of his postulate of differentiation accompanying the definition of a phonemic basis).

In still another attempt at a formalization of N.S. Trubetzkoy's phonology undertaken by Qvarnström, Batóg's works are mentioned in the following way (the author refers to them while discussing the formalized versions of phonological theories he is familiar with):

> However, hardly any existing formal theory, with the possible exception of Batóg's theory, can be construed as a formalization of a particular phonological theory as a whole (Qvarnström 1979, p. 5).

Then, there is also C. Douglas Johnson's critical analysis of TB 12 found in his review of the monograph published in "Foundations of Language" (see: Johnson 1972). Though, in this particular case the reviewer did read Batóg's work carefully, his theoretical standpoint (Johnson is an incurable generativist) does not allow him to accept any of the solutions advocated by Batóg. Johnson's review itself is in my opinion a failure. It fails not because its author criticises in it my Master, but because the review seems to violate some standard methodological norms, thus reducing Johnson's evaluation of Batóg's monograph to a simple statement "generativists would do it differently" (that is, the reader should infer: "in the only proper way").

Batóg's works have also been quoted in, among others, such works as: Marcus (1963, 1966, 1967, 1969); Piotrowski (1966); Brainerd (1971); Rewzin (1963, 1967, 1969); Winogradow (1966); Bird and Klein (1990); and in Lieb (1976). The names speak for themselves.

Batóg's phonological works have most strongly, however, influenced the linguistic writings of Professor Jerzy Bańczerowski, a Poznań linguist who is known not only on account of his phonological works but also because of his important contributions to Finno-Ugric and Oriental studies, and to general, typological and diachronic linguistics. The influence of Batóg's studies upon the scholar is clearly visible both in Bańczerowski's monograph (Bańczerowski 1980) and in the handbook (Bańczerowski *et al.* 1982). Bańczerowski not only tries to apply Batóg's phonological theory to account for sound systems, but he also makes attempts at widening the scope of its application to include other linguistic sub-systems (morphology, the lexicon and syntax). Batóg's approach has also been referred to in Bańczerowski's numerous articles on theoretical phonology. Two of his recent papers, for instance, (i.e. Bańczerowski *et al.* 1992; Bańczerowski 1993) include a brief discussion of Batóg's theory and compare it with other phonological approaches. Here is what Bańczerowski writes on Batóg's works:

> However, the most important step towards an axiomatic phonology was taken by Batóg in his work *The Axiomatic Method in Phonology* (1967), which represents a fully adequate and explicit logical reconstruction of a specific phonological theory, namely the one developed by Harris.
> (Bańczerowski *et al.* 1992, p. 200).

> The aim of Batóg's axiomatic approach has been to attain an adequate logical reconstruction of the Harrisian phonology. For its consistency, complete explicitness, and comprehensiveness Batóg's theory surpasses all other similar attempts which have been offered thus far. The most concise way to characterize this theory would be to adduce its primitive (basic, undefined) terms as well as its axioms.

[Then follows a brief discussion of the primitive terms, axioms and some of the definitions of Batóg's phonological systems — J.P.]

> We do not intend to discuss here all the advantages and disadvantages of Batóg's axiomatic approach, because we have surveyed it in a highly abbreviated and condensed form, which does not allow us to enjoy all the charm of axiomatization. Nevertheless, we would like to emphasize the fact that the essential virtue of his theory consists in establishing a hierarchy in the sets of phonological notions and theorems in the sense that primitive notions are clearly distinguished from the defined ones, and axioms from their

consequences. Such a method demonstrates in an explicit way which primitive notions and axioms should be de facto accepted in order to reconstruct an axiomatic theory within the framework of Harrisian phonology. What is more, it has revealed the enormous possibilities of applying axiomatization to phonological theories (Bańczerowski 1993, pp. 8–11).

In his more recently published linguistic works Tadeusz Batóg tries to determine the possibility of making the process of constructing all the phonemic bases for a given idiolect automatic (TB 46, 48, 58). The problem does not seem to be purely theoretical. On the contrary, if we could come out with a computer program which — on the basis of some (relatively extensive) sample of language material prepared in the form of a phonetically transcribed text — would be able to "phonemize" the text automatically, then such a program would be of great practical significance (to dialectology, for instance).

Thus, in TB 48, Batóg presents an algorithm which could provide a basis for such a computer program. The paper has its theoretical roots in Batóg's earlier work where the notion of a phonemic basis was defined (here, however, the postulate of differentiation is disregarded).

The algorithm consists of a series of components, each of which corresponds to a postulate accompanying Batóg's definition of a phonemic basis. After the set of all sounds which are to be "phonemized" have been established, the first step of the algorithm makes us construct all the possible partitions of that set. The number of such partitions can be calculated by means of the following recursive formulas:

$$P_1 = 1$$
$$P_{k+1} = 1 + \sum_{i=1}^{k} \tfrac{k!}{i!(k-i)!} P_i.$$

The number of partitions grows very fast with the growth of the number of the elements of our initial set. Thus, for example, for the initial set consisting of 60 elements, P_{60} would considerably exceed 10^{36}. We know that the value P_k never exceeds the number $2^{k(k-1)/2}$ (which is the number of reflexive and symmetric relations in a set with k elements).

The second component of Batóg's phonemization algorithm establishes free variants of sounds. A comparison of distributions of all sounds results in their classification into classes of free variants. Thus the set of all sounds determined by the first component of the algorithm is now broken into equivalence classes of the relation of free variation.

Then, one of the following components of the algorithm (the fourth, to be exact) produces a matrix of complementary speech sounds (i.e. it establishes which sounds are mutually complementary). Having established the matrix, we can further reduce the family of classifications of sounds which are to become our potential phonemic bases; i.e. that particular reducing component helps us reject (delete) all the classifications which do not fulfill the postulate of complementary distribution. As members of each partition fulfilling the postulate of free variation are sums (unions) of classes of relation of free variation, it is enough to check whether or not the sounds of a given member of a partition in question which are not mutual free variants stand in the relation of free distribution.

Now the twice reduced family of classifications will have to undergo the test of distinctiveness (which checks whether or not it fulfills the postulate of distinctiveness). The algorithm analyses every classification of sounds separately. For every member of a given classification it constructs a list of phonetic features of all the sounds belonging to this member and then it constructs the set theoretical product of the lists. If the constructed product is an empty set, then the analysed classification is deleted. Otherwise, the product will be compared with other products constructed similarly for all other members of the classification in question. The aim of the comparison is to eliminate all the features which are present in all these products (and thus differentiate nothing!). All the classifications which have "passed the test" and remained will naturally fulfill the postulate of distinctiveness.

The last component of Batóg's phonemization algorithm compares all the classifications which have "survived" the previous three reduction procedures with each other. If for any compared pairs of classifications one (say the first) is summably reducible to the other (e.g. the second), then we delete the first classification and compare the second classification to the next (the third, and so on).

Classifications which will remain after we have gone through all the steps (procedures described in the consecutive components) of the phonemization algorithm will be exactly all the phonemic bases of the initial language material.

The algorithm presented in TB 48 is meant to be universal (suitable for any language). In the last section of his paper, however, Batóg considers the feasibility of constructing its restricted versions — mainly in order to make the algorithm more effective (e.g. it might be considerably simplified if one limited its applicability to only one pre-defined language; a change of the order of its components would influence its effectiveness as well).

Tadeusz Batóg's paper which was discussed above is the first work of its kind in literature.

TB 44 is a brief note in which Batóg shows how incontestibly useful — methodologically, terminologically (and conceptually) and also heuristically — contemporary logic is for linguistics. He illustrates his arguments by referring to Ajdukiewicz's proposals concerning categorial grammars. He also notes — and it is worth mentioning here — that Chomsky has always carefully avoided quoting Ajdukiewicz's work on syntactic connectiveness (*Die Syntaktische Konnexität*) though he is doubtlessly familiar with it. Finally, Batóg discusses W.S. Cooper's rather radical thesis which assumes that deductive logic is nothing but a specialized branch of linguistics. If the reader could stand one more personal digression, I would like to add that many years ago Professor Roman Suszko expressed some interesting ideas which were quite similar to Cooper's thesis.

3 Methodology and Philosophy of Mathematics

I shall include into this section the following works by Tadeusz Batóg: TB 1, 23, 27, 29, 40, 45, 47, 50 and TB 59.

3.1 Philosophy of mathematics

One of Tadeusz Batóg's most important works in the field is his book — which appeared only recently — *Dwa paradygmaty matematyki. Studium z dziejów i filozofii matematyki* [*Two Paradigms of Mathematics. An Inquiry into History and Philosophy of Mathematics*]. The author writes in the Preface that the monograph was meant to form a part of a more comprehensive work on the philosophy of mathematics. Yet, as it forms a complete whole, Tadeusz Batóg decided to publish it separately so as to prevent the manuscript from becoming obsolete.

It would be not modest to claim that I can summarize this book better then the Author himself. Thus, let me limit myself here to quoting the summary of Batóg's book (TB 59, p. 104):

> Two paradigms of mathematics are distinguished in this monograph: the Euclidean paradigm and the logico-set-theoretical paradigm. Within the first of these paradigms, mathematics has been constructed from ancient times until the end of the nineteenth century. This was a quasi-axiomatic mathematics. Proofs carried out within it were based only to a very small degree on assumed axioms. They frequently depended on drawings and on so-called self-evident facts. Mathematicians were also not able to adequately define some fundamental concepts.

> The mathematics developed in the logico-set-theoretical paradigm is fully axiomatized. The proofs carried out in it need not to depend on either drawings or self-evident facts. The very concept of proof is precisely defined within logic. Exact rules of defining concepts are elaborated. The language of mathematics is artificial and separated from natural language, but at the same time it is flexible and has a very great power of expression. The logico-set-theoretical paradigm has been formed mainly owing the works of M. Pasch, G. Peano, G. Cantor, G. Frege, D. Hilbert, B. Russell, A.N. Whitehead, K. Gödel, P. Bernays and A. Tarski.
>
> The distinction between the two paradigms is particularly significant for the philosophy of mathematics. It is evident that the statements of this philosophy must be different for the Euclidean paradigm and for the logico-set-theoretical paradigm. Mixing up these paradigms can lead to absurd conclusions.
>
> Kantian philosophy, with its emphasis on the synthesis of mathematical judgments in intuition (*Anschauung*), furnishes a typical example of philosophical reflection over mathematics constructed within the Euclidean paradigm. Kant's considerations are based upon a fundamentally right observation of mathematical practice, but when related to present-day mathematics they lose all their significance. The matter is quite different, for example, in the case of the weak form of logicism (briefly outlined in the appendix [i.e. the reprint of TB 32 — J.P.]) which is a common outcome of Russell's classic logicism and of Hilbert's classic formalism. This philosophy can be significantly related only to contemporary mathematics, developed within the framework of the logico-set-theoretical paradigm.
>
> A detailed study of the relations between the changes in the philosophy and the changes in mathematics itself would, however, require another paper.

From among the earlier papers in which Tadeusz Batóg dealt with the philosophy of mathematics four (TB 40, 45, 47, 50) are — in my opinion — worth presenting here. The first of them is an encyclopedic entry entitled *Filozofia matematyki* [*Philosophy of Mathematics*]. Batóg points in the paper to two sets of problems (namely, epistemological and ontological) which are of crucial importance for the field. The former class deals with such issues as, for instance, the nature of mathematical cognition, the structure of pure mathematics and the applicability of mathematical theories to empirical research. Ontological problems belong to the nature and manner of existence characteristic of the objects which mathematics is believed to analyse. The author refers in the paper to the names of those philosophers (Plato, Aristotle, Leibniz and Kant) whose thought is believed to have influenced the three main trends of the philosophy of mathematics, namely LOGICISM, INTUITIONISM and FORMALISM. Then the author points to what seems essential for each of the three currents of mathematical thought. Thus, for instance:

— logicism assumes that the whole of mathematics is reducible to logic; of particular importance are the efforts of its followers to axiomatize arithmetic and to reduce set theory to logic;

— intuitionism claims that mathematical objects are products of human thought and, consequently, their existence cannot be axiomatically assumed; also all axioms which admit non-constructive proofs should be rejected; and

— formalism maintains that investigation of axiomatic systems forms the essence of mathematics; finite methods are of fundamental importance within this trend; the formalistic programme can be successful only if it is possible to formalize mathematics.

Batóg discussed, in the paper, the contribution various logicians made to the three respective currents of mathematical thought. Tadeusz Batóg — in the opinion of the author of this review — seems to have sincere sympathy for logicism (particularly for its pluralistic version) as evidenced by the following (TB 40, p. 185):

> Thus, particular mathematical theories can be considered treasuries of logical laws. From that point of view Cantor's set-theory would be as valuable as any non-Cantorian one. It was Bertrand Russell who as early as 1903 formulated the principles of pluralistic logicism (though he changed his opinion concerning the trend later on). Furthermore, the principles are quite close to Aristotle's views that certainty and necessity belong not to individual statements but rather to the logical relations into which they enter.

In TB 45 Tadeusz Batóg shows that it is possible to solve the so-called Locke-Berkeley problem with the help of the tools offered by the theory of suppositional proofs. Let us first look at a formulation of the problem (as in TB 45, p. 3, quoted after: Lubomirski 1983):

> Firstly, why — when trying to prove this or that general mathematical theorem — must we always at some stage of our reasoning refer to individual objects (particulars)? And secondly, how is it possible that the reasoning in a situation like that still leads us anyway to a conclusion which is general?

The matter dealt with in those questions was considered not only by Locke and Berkeley. It was also examined by Kant and Poincaré. Batóg starts with an analysis of Kant's ideas concerning the philosopher's notion of constructing mathematical objects. He finds Kant's position untenable when the philosopher holds "that while proving mathematical theorems we are continuously in the process of intellectual synthesis based on intuitions (and thus allegedly contemplating concrete objects)" (TB 45, p. 8).

The first of the two questions of the Locke-Berkeley problem seems to be directly related to the common use in mathematical proofs of such everyday language expressions as "let us take any triangle", "let n be any natural number", "let us consider any given metric space", and so on. Yet the very use of such expressions points to the fact that a given proof is based on natural deduction — in other words, that the proof is in fact a suppositional proof. And the symbols which refer to a triangle, to an Abelian group, or a natural number, etc. are actually individual constants. Every first year student (here in Poznań of at least mathematics, philosophy, linguistics and information science) should be familiar with the method of proving theorems by means of suppositional proofs. The validity of the method is grounded in the following law, which Tadeusz Batóg calls a suppositional proof theorem (TB 45, p. 11):

> If x is the only free variable of the sentential formula $A(x)$, and c is an individual name absent from the formula $A(x)$ and also from all the formulas of the set X, and if
>
> $B(c) \in Cn_L(X \cup A(c))$,
>
> then
>
> $(\forall x[A(x) \rightarrow B(x)]) \in Cn_L(X)$.

(Cn_L denotes a logical consequence operator; we deal here with theories formulated in the standard predicate calculus).

Pages 11–12 of the paper (TB 45) present the author's conclusions:

> In the first place, in the proofs of general theorems we never have recourse to individual objects, as we are either unable to point to such an object at all, or we consciously try to escape all concretes by means of using names whose denotata remain unspecified.
>
> Secondly, even if we agree that the practice of making use of such unspecified names might mean "having recourse to" objects which are ("in a sense")

individual, then it is still not the case that we always have recourse to individual objects, nor (and all the more so) is it the case that we must do so; if our methods of proving conform to the principles of Hilbert-type logic, the proofs can do without the recourse at all.

Thirdly, if we agree again as we did in point two, then we should also answer the reasonable question: why do we then refer in some proofs to individual objects? The answer is trivial: because the proof is a suppositional proof.

Fourthly, a precise and well-founded answer to the second question posed by the Locke-Berkeley problem can be found in the theorem on suppositional proofs: any proof in which a recourse to individual objects is found is always an example of a suppositional proof, which can always be substituted by a normal proof assuming a necessary "level of generality".

In this way all the sensible questions posed by the Locke-Berkeley problem have found their answers. Yet — to be precise — it is not in this article of mine and it is not today that the problem was solved. It was solved half a century ago when A. Tarski published his deduction theorem and when S. Jaśkowski and G. Gentzen constructed the logical systems of natural deduction and proved their equivalence to the classical Hilbert-type predicate calculus.

Somewhat related to the problems discussed in TB 45 is Batóg's analysis of the infinite induction rule which was mentioned earlier (while discussing TB 38).

Batóg's paper TB 47 deals with two issues. First, the author points to the fact that Locke was clearly aware that the mathematical studies of his times fell short of the ideal of methodological precision (it was *mos geometricus* then). Then Batóg also gives in the paper a brief review of Leibniz's views on fundations of mathematics.

To understand Locke's approach to mathematics one has to remember his assumptions concerning the sources of human cognition and knowledge. Locke believed that "whatever it is which the mind can be employed about in thinking" comes from experience alone, either the experience of sensation, or that of reflection. Then, there seem to be three distinct types of knowledge and three degrees of certainty corresponding to these types; thus, we have:

— intuitive knowledge (the most certain knowledge of some irrefutable truths; sensory perceptions which our mind is bound to accept, e.g. that white is not black);

— demonstrative knowledge (of, for example, some aspects of mathematics; our mind perceives in this case some relations between ideas by means of other ideas — which constitute a proof — rather than indirectly, as in the case of intuitive knowledge); and

— sensory knowledge (which is the least certain of the three, and which includes existential statements — sentences in which we state that some external things exist).

According to Locke, the ultimate goal of human cognition would be the discovery of general and absolutely certain truths. Hence mathematics (which includes only intuitive and demonstrative types of knowledge) is a paragon of all science. Locke however differed from his contemporaries in his views concerning the essence of mathematics. Though he did consider axioms true (an even self-evident), he also pointed to the huge numbers of sentences which are used in mathematics daily and which — though equally self-evident as axioms — were neither axioms themselves, nor were they deduced from axioms. He was referring to primarily certain arithmetical truths (such as for instance properties of addition), and also geometrical proofs, which had to be supplemented by resorting to intuition and drawings. Batóg looks into Locke's *Essay Concerning Human Understanding* to substantiate his claims (TB 47, p. 106):

> Locke's criticism was the first firm voice raised as it were in public to show that the state of axiomatics is still unsatisfactory. His criticism was of a rather global nature and assumed a feasibility of making the whole of mathematics (and arithmetic in particular) axiomatic. Locke's words sound like a challenge when he asks: 'I think I may ask all those who wish so urgently that the whole of our knowledge — except for some general truths — were based upon some general, inborn and self-evident principles: what is the principle needed to prove that 1 and 1 make 2, 2 and 2 make 4, and 3 times 2 make 6?'.

It is necessary — states Batóg — to revise our beliefs that it was Leibniz who first suggested deducing the whole of arithmetic from a few primary elements. We should confront these beliefs with what Locke contributed to the problem. Leibniz's results must be viewed as a polemical answer to Locke's challenge.

There are differences between Locke's and Leibniz's approaches to such questions as the structure and role of ideas, concepts and terms — a subject which I do not intend to go into here. Yet, it seems important to point to Leibniz 's notion of definitions. To define a concept meant for him to iterate the operations of conjunction and negation; the iteration, however, was restricted to names only (so that predicates were excluded). Thus, for example, a complex concept: a *non-*

trivial geometrical theorem would be derived from simple concepts, such as: *trivial, geometrical, theorem* by means of the operations mentioned earlier.

All truths may in Leibniz's opinion be, on the one hand either truths of reason or truths of fact, and on the other either basic or derived truths. Truths of fact are contingent and concern the real world. Truths of reason, however, are necessary and eternally true; they concern every possible world. Out of the latter, the laws of identity are basic. Batóg notes that Leibniz's basic truths should not be simply identified with logical truths, as they also include extra-logical truths of the type "no thing is larger or smaller than itself".

Mathematical truths were included by Leibniz to truths of reason, and as such they were regarded as eternal and necessary. Every mathematical theorem was to be reducible to basic truths (by means of either the law of non-contradiction, or the law of the excluded middle, or the laws of identity). A position like that could justifiably be called that of radical logicism. And that was exactly the position Leibniz tried to defend. It is interesting in this context to note the way he attempted to validate the arithmetical truths mentioned in the quote from Locke. Thus, truths such as these were considered to be either definitions of consecutive natural numbers, or else as truths reducible to such definitions with the help of the laws of identity and the rule of substituting "the equal for the equal". For example:

$$2 = 1 + 1, \quad 3 = 2 + 1, \quad 4 = 3 + 1$$

are definitions of the numbers $2, 3, 4$ and so on. It is worth noting that in Peano's system which was born 200 years later natural numbers are defined in exactly the same way, starting from 0 and making use of the operation of adding one. Then, Leibniz would treat the arithmetical truths:

$$2 + 3 = 5, \quad 2 + 2 = 4$$

as derived truths. So that a proof of, for example, $2 + 2 = 4$ would look in the following way:

1.	2 = 1 + 1	definition
2.	3 = 2 + 1	definition
3.	4 = 3 + 1	definition
4.	2 + 2 = 2 + 2	axiom of identity
5.	2 + 2 = 2 + 1 + 1	from 4. and 1. (substitution)
6.	2 + 2 = 3 + 1	from 5. and 2. (substitution)
7.	2 + 2 = 4	from 6. and 3. (substitution)

The proof makes also use of the associative law of addition. But Leibniz could not have been aware of the fact that the law differed from his "normal" law of identity. These facts — as Batóg pointed out — were discovered later by Bolzano and Frege. Thus, the proof itself did not quite conform to what was strictly required by Leibniz's "logical programme".

Yet, despite certain inadequacies of his programme, Leibniz was certainly the first to understand the mathematical nature of logic, and the first mathematician who knew what a formal proof consists in when arrived at in conformity with some predetermined precise principles.

Let me quote again what Batóg says concluding his paper (TB 47, p. 118):

> And yet Leibniz did not reform logic. There seem to be two factors which contributed to this failure. One of them involved his outdated approach to definitions of concepts; and this approach formed the foundations of his programme of constructing a universal logical calculus. The other factor was his insistence on including the whole of logic into the narrow framework of the calculus of concepts or calculus of ideas — or as we would call it today the calculus of names. A reform of logic, and a reform in style it was, was carried out only by Gottlob Frege in 1879 and its three cornerstones were: the moving of sentential calculus to the forefront of logical analysis; differentiating between general names and individual names; splitting sentences into arguments and functors. Leibniz's logic, however, rediscovered at the turn of the 19th century by B. Russell and L. Couturat was to become an ally of modern mathematical logic in its fight for the position it rightly deserves in mathematics and philosophy.

Then, in his paper entitled *O Kantowskiej krytyce argumentu ontologicznego* [*On Kant's Criticism of the Ontological Argument*; TB 50] Batóg presented his own version of the famous ontological "proof" for the existence of God, which — presented presumably for the first time in the 11th century by Anselm, Archbishop of Canterbury — was then discussed by many philosophers including, among others, Descartes. Batóg aimed at showing the sources of its ostensible validity. The results of his analysis reveal the essence of Kant's criticism of the argument including the goals and consequences of the philosopher's undertaking. I hope that the two excerpts from the paper that I quote below will be representative of the author's approach (the first shows which of the logical means the authors considers appropriate to philosophical argumentation, while the other is a succinct conclusion of the reasoning presented in the paper — I allowed myself the liberty of leaving out the footnotes from in the original text):

During the last several decades the ontological argument was often analysed with the help of logical means. At the same time a strong tendency prevailed making use of such non-classic logical tools as modal logics, Leśniewski's ontology, a logic devoid of ontological assumptions, and so on. This tendency undoubtedly results in very interesting observations which are, however, far removed from what can be found in Anselm's and Descartes' historically given texts. Furthermore, as the concepts of modal logics is notoriously vague, the tendency also leads to incessant purely verbal controversies which usually end in concluding that this or that philosopher is thoroughly muddled with respect to modal predicates. Neither do I think that the littering of the language of science — which forces one to abandon the principles of classical logic — with such objects as Pegasus or Chimera (only to be able to say that there are such things which do not exist) is of any scientific value at all. *I do believe, however, that at the moment only classical logic is the proper tool to analyse all the old problems and philosophical arguments. It is so because it is the only kind of logic that functions within science as a real tool for proving statements (for example in classical mathematics and all the sciences grounded in it). All other kinds of logic (with a possible but extremely doubtful exception of intuitionistic logic) are for the time being extrinsic to science and still have to search for their legitimate place therein. In any case, I want to be explicit about one thing, namely that my analyses are grounded here in classical logic. That is what makes them clear and simple*. [emphasis added — J.P.]

The final conclusion of Kant's criticism of the ontological argument can be found, in my opinion, in the following clear, simple and unambiguous statement: 'If thus I imagine some being as the highest reality (without any imperfection), then there will always remain the question of whether the being exists or not'. The conclusion is perfectly justifiable. For inserting — by means of a definition — existence into a deity's essence is a completely misguided procedure. It may lead at most to a general statement to the effect that all gods exist. Naturally, a general statement like that has nothing at all to do with the real problem of the existence of a god; yet it does produce an illusion as if the proof of the existence of a god were thus easily established.

3.2 Methodology of science

In the mid-seventies Tadeusz Batóg devoted three of his works (TB 23, 27, 29) to polemical discussions with Professor Leszek Nowak's views on some methodological issues. In TB 23 Batóg carried out a critical analysis of a number of Nowak's notional constructs, such as, for instance:

— his concept of idealizational laws;

— his approach to ideal types;

— some of the methodological principles underlying Nowak's theory;

— Nowak's process of the so-called "concretization of idealizational laws" (and its result)

— the procedure of explanation in Nowak's theory.

In the same paper Batóg also presented his own suggestions concerning the notion of "an idealizational law" and that of its "concretization". At the same time, he noted that idealizational laws themselves pose certain problems which are most probably unsolvable.

The editors of *Studia Filozoficzne* [*Philosophical Studies*] published TB 23 together with Nowak's comments on the paper. Both papers were written in a sharply polemical form. Tadeusz Batóg mercilessly attacked his opponent's formal blunders, whereas for Leszek Nowak the most important were purely methodological problems.

TB 27 and the English version of the same paper (i.e. TB 29) point to the interdependence between concretizations of idealizational laws on the one hand and the notion of generalization on the other.

In what follows below, I would like to restrict myself to quoting some of the author's definitions. It is a truism to say that the problems dealt with in the paper are of fundamental importance to the methodology of empirical studies and that they still await satisfactory solutions. Tadeusz Batóg has not returned to that subject for several years.

TB 23 opens as follows (TB 23, p. 63):

> The problem of the idealizational laws is one of the most important issues in the methodology of empirical research. This results from two simple reasons: laws of that kind are — on the one hand — relatively frequent in all sciences, the stage of development of which allows for the formulation of any laws at all, and — on the other — such laws seem never to say anything about the real world. Thus, what intrigues us is the role of these laws in sciences; what is their formulation based on, whether and how can they serve to explain real facts.

What seems characteristic of Batóg's approach to defining such notions as that of an idealizational law, its concretization, etc. is his grounding of the definitions on purely syntactic properties of the formulas and on inferential dependences. Thus, for instance, as an introduction to his definition of an idealizational law one finds his definition of an idealizational implication.

If T is an empirical theory (closed with regard to the rules of inference) and A is an arbitrary formula of the (strictly codified) language of the theory T, then $\bigwedge(A)$ and $\bigvee(A)$ will denote respectively the universal and existential closure of the formula A.

We shall say that the sentence A is an *idealizational implication* within T with regard to the idealizing conditions $B_1, \ldots, B_n$ (where $B_1, \ldots, B_n$ are sentential formulas of the theory T) if and only if there exist sentential formulas C, D of the theory such that

(*) $A = (\bigwedge(C \wedge B_1 \wedge \ldots \wedge B_n \rightarrow D))$

or

(**) $A = (\bigwedge(B_1 \wedge \ldots \wedge B_n \rightarrow D))$

and also when the following conditions are satisfied:

(a) $(\bigvee(C)) \in T$;

(b) $(\neg \bigvee(D)) \in T$;

(c) for all $i \leqslant n,\ (\neg \bigvee(B_i)) \in T$;

(d) the set of conditions $B_1, \ldots, B_n$ is consistent.

All implications having the form (*) will be termed idealizational implications with the realistic condition C. The implications of the form (**) — will be called idealizational implications without any realistic condition.

The sentence A is the *idealizational implication* within the theory T if and only if A is an idealizational implication with respect to the conditions $B_1, \ldots, B_n$, for certain $B_1, \ldots, B_n$.

A simple but extremely important consequence of the above definitions is the fact that every theory includes all of its own idealizational implications. Therefore, if the validity of the theory T itself is assumed, there is no need to check the validity of such implications.

Which of the idealizational implications of the theory deserve the name of idealizational laws does not depend — concludes Batóg — on the structure itself, which the formulas may have, but rather on the role that the formulas play (through their concretizations — which are nothing more than simple generalizations) within a particular theory. A definition, however, of the notion of generalization itself poses serious difficulties (as it would have to refer to a vague concept of theorem similarity).

Methodologists believe that idealizational implications of the form presented as (***) below are of particular importance (on account of their applications):

(***) $C \wedge p_1(x) = d_1 \wedge \ldots \wedge p_n(x) = d_n \rightarrow F(x) = H(x)$

where p_i, F, and H are some real-valued functions, and d_i are concrete real numbers ($1 \leqslant i \leqslant n$). Batóg assumes that various types of concretizations corresponding to the idealizational implication of the form (***) may exist. To illustrate this approach I shall quote here an example of one of them.

Thus, we shall use the term *complete concretization* of the idealizational implication (***) with regard to the theory T to denote the general closure of the following formula:

$$C \rightarrow K(x, p_1(x), \ldots, p_n(x)) = L(x, p_1(x), \ldots, p_n(x)$$

where K and L are real functions such that the equations:

$$K(x, d_1, \ldots, d_n) = F(x) \quad L(x, d_1, \ldots, d_n) = H(x)$$

are logical consequences of some finite set T' which is included in T, but which does not have among its consequences any statements of the form:

$$\neg \bigvee\nolimits_x (p_i(x) = d_i), \text{ for all } i \leqslant n.$$

To illustrate this notion Batóg makes use of Clapeyron and van der Waals equations for states of gases and vapours. Clapeyron's equation is an example of an idealizational implication of the form:

If x is a mole of gas such that $p_w(x) = 0$ and $B(x) = 0$, then

$$p(x) \cdot V(x) = R \cdot T(x)$$

where $R =$ the gas constant which depends on the gas, $T(x) =$ temperature of some gas x on the Kelvin scale, $p(x) =$ pressure of the gas x, $p_w(x) =$ internal pressure of the gas x, $B(x) =$ the actual volume of the gas molecules of which the portion of the gas x consists, $V(x) =$ volume of the gas x.

Van der Waals equation has on the other hand the following form:

If x is a mole of gas, then

$$(p(x) + p_w(x)) \cdot (V(x) - 4 \cdot B(x)) = R \cdot T(x).$$

Van der Waals equation is obviously a complete concretization of Clapeyron's equation and — what is more — the latter is a trivial logical-mathematical consequence of the former. Batóg concludes that in practice the phenomenon is quite typical — an idealizational implication is usually but a consequence of its own concretization.

A complete concretization of a given idealizational implication is usually not a theorem of a theory T — it is simply a hypothesis which needs an empirical verification.

Yet, neither the verification nor the falsification of some concretization of an idealizational implication may have any influence on the truth of the implication itself. One might therefore come up with the idea that some idealizational implications should be set apart with the help of certain semantic criteria as idealizational laws of a given theory.

> The idealizational implication with regard to some theory T which has the form (***) is an *idealizational law* of the theory T if and only if the implication is a strictly general statement and also if there exists at least one true (in the standard model of the theory T) complete or restricted concretization of the implication with regard to the theory T (TB 23, p. 79).

[I have omitted here the definition of restricted concretizations — see page 78 of the discussed paper — J.P.]

It turns out, however, that even this definition (a seemingly reasonable one) cannot be regarded as adequate. That is so, because among other reasons, it is always possible to find for an arbitrary idealizational implication (including the most absurd one) its true complete concretization. Thus, if, for example, the formula:

(****) $C \wedge p(x) = 0 \rightarrow F(x) = H(x)$

is an arbitrary idealizational implication, and the formula:

$C \rightarrow \varphi(x) = \psi(x)$

is an arbitrary factual assertion which happens to be empirically true, then the implication below:

$$C \rightarrow (F(x) \cdot (1 - sg(p(x))) + \varphi(x) \cdot sg(p(x)) = \\ = H(x) \cdot (1 - sg(p(x))) + \psi(x) \cdot sg(p(x)))$$

is an empirically true complete concretization in respect of (****); the function sg of a real variable used in the formula above has the value 0 for the argument 0 and that of 1 for all other arguments. Thus, as we can see, the semantic criterion when used to distinguish idealizational laws loses all meaning.

Tadeusz Batóg ends his polemical review of Leszek Nowak's ideas in the following way (TB 23, pp. 80-81):

> Basically, I could end my arguments here. For they demonstrate quite conclusively that "concretizability" or the fact itself that some idealizational implication can be assigned a concretization cannot be regarded as a valid criterion to distinguish idealizational laws from absurd idealizational implications. ... One could naturally argue that concretizations could be understood in a way different from what I have suggested here and also from the way Nowak understands the notion. But how? - That is the question. In my opinion, the concretization problem cannot be solved by means of strict and precise methods. For, as a matter of fact, it is the old methodological problem pertaining to the codification of methods of inductive reasonings. Thus, when trying to think it over, one might find it worthwhile to ponder a moment over the following simple statements written by Jan Łukasiewicz the founder of hypothetism (he refers to inductive reasonings as reductive ones): 'In the natural sciences, deductive reasoning has an important role to play side by side with reductive procedures in the testing of hypotheses. Reductive procedures, however, do not keep to precise scientific criteria and they depend on the researcher's free intuition. Looking for general laws of nature could be compared to reading an encoded telegram the key to which is unavailable to us'.

Tadeusz Batóg has been held in high esteem not only by logicians, mathematicians and linguists from Poznań, but also by methodologists and philosophers. This respect and admiration are a result of his lectures on mathematical logic which he has (for over twenty years) been giving in the Institute of Philosophy at UAM.

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