Regressive Analysis*

Volker Peckhaus
Universität Gesamthochschule Essen
Fachbereich 1: Fach Philosophie, D – 45117 Essen
E-mail: volker.peckhaus@uni-essen.de

Abstract

The regressive analytical method which can be generally understood as the way backward occurs in science on different levels. In scientific practice it represent research, i. e., activities of scientists when looking for the conditions necessary to solve given problems. On the methodological level it stands for ordering a given set of statements, and, finally, on the foundational level it gives a justification for the starting points of deductions. In the first section the paper gives a historical survey of regressive analysis concentrating on three paradigmatic examples: (1) Pappus's definition of analysis and synthesis, (2) the definition of method that can be found in the so-called "Logic of Port Royal", and (3) David Hilbert's definition of the axiomatic method as a procedure for setting up axiomatic systems. In the second section the scepticism of traditional philosophy of science concerning the regressive method is reflected. The reason for this scepticism might be that regressive analysis is not completely logically determined, but has elements of contingency, creativity and intuition.

Die regressive Methode, die allgemein als rückschreitendes Verfahren charakterisiert werden kann, kommt in den Wissenschaften auf verschiedenen Ebenen vor. In der wissenschaftlichen Praxis repräsentiert sie die Forschung, also die Aktivitäten von Wissenschaftlern bei der Suche nach den Bedingungen, die notwendig erfüllt sein müssen, damit ein gegebenes Problem gelöst werden kann. Auf der methodologischen Ebene steht sie für das Ordnen einer gegebenen Menge von Sätzen und auf der Ebene der Grundlagen schließlich gibt sie die Rechtfertigung

*A first version of this paper was presented on June 5, 2000, at the 2000 Bled Conference in Philosophy on Philosophical Analysis, and on August 14, 2000, at the Department of Philosophy of the University of La Plata, Argentina. An earlier version of this paper was published in a Spanish translation (Peckhaus *2000*).

für Ausgangssätze von Deduktionen. Im ersten Teil des Beitrages wird ein Überblick über die Geschichte der regressiven Analyse gegeben. Dabei werden drei paradigmatische Beispiele besonders berücksichtigt: (1) Pappus' Definition von Analyse und Synthese, (2) die Methodendefinition in der sogenannten "Logik von Port-Royal" und (3) David Hilberts Definition der axiomatischen Methode als eines Verfahrens, mit dem axiomatische Systeme allererst aufgestellt werden. Im zweiten Teil wird auf den Skeptizismus der traditionellen Wissenschaftstheorie bezüglich der regressiven Methode eingegangen. Die Ursache für diesen Skeptizismus mag darin liegen, daß die regressive Analyse nicht vollständig logisch determiniert ist, sondern Elemente von Kontingenz, Kreativität und Intuition enthält.

1 Introduction

On 9 March 1907 Bertrand Russell gave a paper before the Cambridge Mathematical Club, entitled "The Regressive Method of Discovering the Premises of Mathematics". The object of this paper was, as he told the listeners, "to explain in what sense a comparatively obscure and difficult proposition may be said to be a premise for a comparatively obvious proposition." He wanted to consider furthermore "how premises in this sense may be discovered, and to emphasize the close analogy between the methods of pure mathematics and the methods of the science of observation" (Russell *1973*, 272). Russell's words indicate a scepticism towards this regressive method which might be due to the philosophical standpoint of naive realism he held at that time.

Russell distinguished two types of premises. An "empirical premise" is the proposition from which we are led to believe that the proposition in question is true, a "logical premise" is some logically simpler proposition from which the proposition in question can be derived deductively (ibid., 272–3). The distinction between logical and empirical premises is not a complete disjunction. For Russell, the term "empirical premise" concerns the epistemological justification of our beliefs, the term "logical premise" concerns the deductive structure between propositions. A premise, e. g. the law of contradiction, can therefore be both, logical and empirical. According to Russell, these general definitions play a specific role in mathematics (ibid., 273–4):

> [In mathematics] our propositions are too simple to be easy, and thus their consequences are generally easier than they are. Hence we tend to believe the premises because we can see that their consequences are true, instead of believing the consequences because we know the premises to be true. But the inferring of premises from consequences is the essence of induction; thus the method in investigating the principles of mathematics is really an inductive method, and is substantially

> the same method of discovering general laws in any other science.

In the paper on the regressive method Russell discusses at length the relation between propositions of elementary number theory such as $2 + 2 = 4$, and the Peano axioms, the possibility of going back further, to Frege's "more ultimate" logical premises, and to his own alternative version formulated in the light of the paradoxes in order to save logicism in an even more general form than Frege held.

I do not intend to go into the details of this paper, but only hint at some peculiarities. Peter Hylton has observed that the picture of knowledge sketched in Russell's paper (Hylton *1990*, 332)

> is tailor made for the axiom of reducibility. It is surely a description of something like the way in which Russell in fact came to formulate this axiom: not because it struck him as intrinsically obvious, but because he needed it in order to permit inferences to logical or mathematical claims which did strike him this way.

The axiom of reducibility says that for each propositional function of a certain type there is an elementary (predicative) propositional function which is extensionally equivalent to it. Hylton concludes: "The effect of the discussion is to elevate this [regressive] method not merely into a general method of discovery but also into a justification" (ibid.). I'm not empiricist enough to accept this effect. The way a principle is obtained should be distinguished from the justification of the principle. One should furthermore add that this effect is connected with a rather one-sided view of the scientific process. Russell wrote on the starting point of science (Russell *1973*, 274):

> In every science, we start with a body of propositions of which we feel fairly sure. These are our empirical premises, commonly called the facts, which are generally got by observation [...]. The general laws of a science are propositions logically simpler than the empirical premises of the science, but such that the empirical premises, or some of them, can be deduced from these laws.

This picture is a rational reconstruction of the scientific process, and one can fairly doubt whether it meets scientific practice. Against Russell, I would like to suggest that science usually starts with doubts, questions or problems, and that methods are needed that help to solve these problems. In Russell's view science as a problem solving activity is not a topic. But it is this aspect of science that gives the most important applications of the regressive analytical method. It is a problem solving method, or, to be more exact, it contains an undetermined set of problem solving devices.

I hold a very general view of the regressive analytical method. Regression occurs on different levels:

1. On the practical level the regressive analytical method stands simply for research, i. e., the activities of scientists when looking for the conditions necessary to solve given problems.

2. On the methodological level it stands for ordering a given set of statements.

3. One can finally add a foundational level which goes beyond the relative foundation given by ordered statements. On the foundational level the justification of the starting points of deductions themselves is requested.

These three levels have to be distinguished. It is possible to follow the analytic method on one level whilst refusing it on another. A famous example is Kant, the master of the critical method that is definitely regressive analytical on level (3), who denied that philosophy should be concerned with *quid facti* questions, i. e., research done in practice and therefore regressive analysis on level (1).

My aim in this paper is, firstly, to give a historical survey of regressive analysis. This survey will cover some 1700 years of reflection on method. I will, however, concentrate on only three paradigmatic examples:

1. Pappus's definition of analysis and synthesis,

2. the definition of method that can be found in the so-called "Logic of Port Royal,"

3. David Hilbert's definition of the axiomatic method as a procedure for setting up axiomatic systems.

I will secondly try to determine some features of the regressive analytical method which help to explain the scepticism of the traditional philosophy of science. Regressive analysis is not completely logically determined, but has elements of contingency, creativity and intuition. This insight will help us to widen the scope of the philosophy of science and the philosophy of mathematics in a certain respect.

2 Historical Survey

Regressive analysis is a topic dealt with in general methodology. It is one side of what has been called "the method of analysis and synthesis", i. e., the combination of methods of analytical decomposition and synthetical construction. Although the terms "decomposition" or "dissection", in German "Zergliederung" or "Zerlegung", were generally associated with regressive analysis through the ages, it doesn't consist simply in breaking complex structure into pieces, i. e., their simpler constituents. A paradigmatic formulation of the special form of regressive analytical decomposition in the focus of this paper can already be found in book 7 of the *Collection* of Pappus of Alexandria, written at the end of the third, beginning of the fourth century A. D.

2.1 Pappus of Alexandria

Let me quote the relevant sections where Pappus gives his son Hermadorus some hints for obtaining the capability of solving geometrical problems. I quote from Alexander Jones's translation (Pappus *1986*, 82).

> Now, analysis is the path from what one is seeking, as if it were established, by way of its consequences, to something that is established by synthesis. That is to say, in analysis we assume what is sought as if it has been achieved, and look for the thing from which it follows, and again what comes before that, until by regressing in this way we come upon some one of the things that are already known, or that occupy the rank of a first principle. We call this kind of method 'analysis', as if to say *anapalin lysis* (reduction backward). In synthesis, by reversal, we assume what was obtained last in the analysis to have been achieved already, and, setting now in natural order, as precedents, what before were following, and fitting them to each other, we attain the end of the construction of what was sought. This is what we call 'synthesis'.

Pappus distinguishes between two kinds of analysis, *theorematic* analysis, that can be characterized as a procedure of hypothesis assessment, and *problematic* analysis, that opens the way to existence proofs for certain geometrical constructions.

The main problem for an interpretation of these passages are phrases like "by way of its consequences" or "advancing through its consequences" when analyzing hypotheses. Hintikka and Remes in their translation use instead of "consequences" the rather vague term "concomitants" or "going together with" (Hintikka/Remes *1974*, 14). This understanding solves a lot of problems, since if "consequences" means "logical deductions" the difference between analysis and synthesis is reduced to the fact that analysis starts with hypothetical statements whose truth is asserted, whereas synthesis starts with true statements. Jones takes this position when he writes that theorematical analysis is a "naive technique using the same kinds of logical steps as synthetic proof, but beginning with the assumption of that which is to be proved, and advancing until a conclusion is reached that is known to be true (or false) independently of the assumption" (Jones *1986*, 67). If one arrives at a proposition independently known to be true, nothing is guaranteed for the truth of the initial hypothesis. Only arriving at a false statement gives certainty, it is a valid disproof of the hypothesis. Therefore, an analysis that leads to something known to be false is a special case of a *reductio ad absurdum*, as Árpád Szabó correctly pointed out (Szabó *1974*, 126).

I do not intend to give a deeper logical interpretation of what is going on in Pappus's analysis. Illuminating information can be found in Hintikka's and Remes' interpretation published in *The Method of Analysis* (*1974*) or, more recently, in Matthias Berning's book on analysis and determination (*1997*). I would only like to hint at one aspect, the directional view on Pappus's analysis that Hintikka and Remes call standard, which is possible, but misleading. For Hintikka and Remes the directional view is represented by a "propositional interpretation or analysis-of-proof view" (Hintikka/Remes *1974*, 32) which they contrast to an "instantial interpretation or the analysis-of-figures view" (ibid.). This distinction, however, does not help answering questions like "what are the conditions for a given hypothesis to be true" which is, I think, a suitable description of what Pappus was aiming at.

Questions like this are not restricted to geometry. Although formulated for geometry, the method of analysis and synthesis can be applied to other fields of knowledge in which problems are to be solved. Let me illustrate this with an example from syllogistics. Consider a given universal affirmative hypothesis of the form "All S are P". The question shall be whether this hypothesis can be derived from true propositions by means of the "perfect" mode BARBARA. The following steps will occur when applying the method of analysis. Given the conditional frame, the hypothesis, if true, has to be derived from true universal affirmative premises. The first step is to determine two premises from which the hypothesis can follow. If the premises found are not known to be true, the procedure has to be continued for each premise until underivable principles are reached or propositions known to be true. The positions of S and P in the premises are determined, e. g. looking for the premises is guided by the rules of the syllogism. Only the mediating middle term M is missing. There is, however, no algorithm to determine M. It has to be "invented" by means of virtues like creativity, intuition, the brute force of the combinatorial method, or quasi-empirical methods like the trial-and-error-method. This example shows:

1. The regressive analysis is an ordering device.

2. The regressive analysis is not one single method, but a set of several different methods. It might even contain non-methodological components.

3. The regressive analysis is not completely determined.

4. The choice of specific methods used during the analytic procedure largely depends on the questions posed.

5. The regressive analysis has creative components. It is a method of invention and research.

Finishing analysis does not yet solve the problem. Analysis only leads to the basic propositions necessary for the proof of a proposition or for the solution of a problem, but it is not the proof or the solution itself. The proof proceeds in a constructive, synthetic way. Analysis and synthesis have to work hand in hand. Analysis is the *conditio sine qua non* for synthesis, or, to speak with Aristotle, the last step in analysis is the first step in genesis (*Eth. Nic.* 112b15–25).

In his *Opera logica*, first published in 1597, Jacobus Zabarella (1533–1589) picked up these thoughts (Zabarella *1597*, lib. III, cap. 3, Sp. 226. Cf. also the note "De regressu" in the same collection). He distinguished between the compositive or synthetic method of proof that combines given premises to a conclusion, and the regressive or resolutive decomposition of given sentences into the conditions from which they follow. He stressed that both methods cannot be isolated. In applicational contexts they have to be understood as a unit with a hierarchical structure. The resolutive or decompositional method has its place inside logic, where it is, however, secondary to the synthetic, compositional method. The resolutive method is the servant of demonstration ("Methodus resolutiua est serua demonstratiuae", Zabarella *1597*, "De methodis libri quatuor", lib. III, cap. 18, col. 266). The resolutive method aims at "inventio", not at "scientia", he says. However, we can only reach perfect knowledge, if, after having gone back from the facts to their reasons, we infer the facts from the reasons deductively.

It is possible to formulate Zabarella's insight in the following way: Scientific work aims at presenting its results in textbook style. This presupposes analysis as a *conditio sine qua non*. The combination of both methods appears to be the essence of the scientific process.

2.2 Logic of Port Royal

Zabarella lived in the 16th century coming close to the authors of my second example, *La logique ou l'art de penser* first published anonymously in 1662, and today known under the name "The Logic of Port Royal". It was written by the Jansenistic theologians and philosophers Antoine Arnauld (1612–1694) and Pierre Nicole (1625–1695) in the spirit of René Descartes' methodological philosophy. Let me quote the beginning of the second chapter from the fourth part "On Method". There the authors write (Arnauld/Nicole *1996*, 233):

> The art of arranging a series of thoughts properly, either for discovering the truth when we do not know it, or for proving to others what we already know, can generally be called method.
>
> Hence there are two kinds of method, one for discovering the truth, which is known as *analysis*, or the *method of resolution*, and which can also be called the *method of discovery*. The other is for making the truth understood by others once it is found. This is known as *synthesis*, or the *method of composition*, and can also be called the *method of instruction*.

Although this is clearly modeled upon Pappus's distinction, it goes further because of the definition of "method" which sounds rather strange to modern ears. Method is "the art of arranging a series of thought", i. e., an ordering device, and ordering is the basic feature of both, discovery and presentation. Along these lines Christian Wolff, the great creator of German philosophical terminology, could translate the Greek term "method" by "Lehrart", i. e., "way of teaching" or "way of presentation". In this form the term was still used by Kant, who informed the readers of his *Prolegomena to Any Future Metaphysics*, that this later shorter work sketches the plan for his widely misunderstood *Critique of Pure Reason*. In the *Prolegomena* he used an analytical way of presentation, "while the work itself [i. e., the *Critique*] had to be executed in the synthetical style, in order that the science may present all its articulations, as the structure of a peculiar cognitive faculty, in their natural combination" (Kant *1783*, A 21–22).

The equations "analysis is discovery", and "synthesis is proof or construction" are, however, far too simple without further qualifications. Let me illustrate this with a quote from Leibniz's paper "De Synthesi et Analysi universali seu Arte inveniendi et judicandi" ("On Universal Synthesis and Analysis or the Art of Inventing and Judging") written between 1683 and 1685 (*1683/85*, 544). There Leibniz repeats the well-known positions in a modified way. "Something is synthesis," he writes, "if one starts with principles, goes step by step through the truths, discovering certain propositions, laws and sometimes even general rules." According to Leibniz, analysis on the other side is applied only for solving certain problems, proceeding as if nothing was discovered hitherto. In this view the scope of analysis is considerably narrowed, in favor of synthesis which is for Leibniz more important. Please note that in Leibniz's opinion, synthesis is a device for discovering new truths, using deductive tools like syllogistic, combinatorics and logical calculi. But even Leibniz could not deny that analysis leads to principles which are the starting points of deductions. Analysis furthermore motivates the choice of primitives being combined by the art of combinatorics.

Leibniz is a proponent of a deductive logic of discovery. If discovery is analysis, there is a deductive analysis which presupposes a regressive analysis making the deductive process possible.

In this Leibnitian context it might be worthwhile to mention that the analytical regressive method, although created in a mathematical setting, should neither be confused with the *mathesis universalis*, i. e., universal mathematics, on the one side, nor with the so-called "mathematical method" on the other. "Mathesis univeralis" concerns calculating outside mathematics. In Leibniz's conception of a general science it is used not only in the framework of the *ars inveniendi*, i. e., the art of discovery, but also in the *ars iudicandi*, i. e, the art to settle disputes. When the *ars iudicandi* has once been erected, Leibniz thinks (Leibniz *1688*, 913),

> two philosophers, who are in a dispute, will argue not differently from two mathematicians. It will be enough that they take their pen, sit down in front of their abacus and say to each other: 'calculemus', let's calculate.

Again I have to stress, that *ars inveniendi* and *ars iudicandi* presuppose the application of the analytical regressive method, that gives the arithmetisation of the problem in question.

The *mathesis universalis* itself has to be distinguished from the mathematical method or the mathematical way of presenting given knowledge. The term "mathematical method" designates in traditional philosophical terminology the erection of systems of propositions according to the model of Euclid's *Elements*. Terms like "mos geometricus", "geometrical method" or "Euclidean method" are therefore synonymous with "mathematical method." Let me quote again Christian Wolff, who began the first volume of his series of German textbooks, the *Anfangs-Gründe aller Mathematischen Wissenschaften* ("The Basic Foundations of All Mathematical Sciences") of 1710 with a text entitled "Kurtzer Unterricht von der Mathematischen Lehrart" ("Short Instruction of the Mathematical Method"). There, he defined "mathematical method" as the order of a mathematical presentation or lecture (§ 1). Such presentation begins with "explanations" (i. e., definitions) from which we go to "principles" (i. e., axioms, postulates). "Doctrines" (i. e., theorems) can be deduced from these basics. They can be illustrated by problems, "additions" (i. e., corollaries), and notes. Wolff stressed that the mathematical method is a general method, it can be applied in all scientific disciplines. Its name "mathematical" or "geometrical method" is only traditional, because it was followed up to then only in mathematics and geometry.

2.3 Hilbert's Axiomatic Method

We have seen, the regressive analytical method has to be distinguished from the "mathematical method". Building up a set of propositions according to the “mathematical method” means that all theorems have to be deduced from definitions and principles (i. e., axioms or postulates) set at top of the system. The procedure of traditional axiomatics is *synthetic* (i. e., constructive) and *dogmatical*. Principles are set. They are evident and immediate. They cannot be proved, but they don't even need a proof. Basically, Hilbert's procedure in his *Grundlagen der Geometrie* of *1899* is in accordance with this kind of “mathematical method”, despite the fact that Hilbert determines the validity of axiomatic systems without any reference to some extra-mathematical reality. Like traditional axiomatics, modern formalistic axiomatics is in its presentation synthetical and dogmatical.

Hilbert was aware of the necessity not to confuse the presentation of systems of statements in axiomatic form with the axiomatic method. Such presentation is only the result of an application of the axiomatic method. The axiomatic method itself is a procedure to structure a set of statements. In his paper “Über den Satz von der Gleichheit der Basiswinkel im gleichschenkligen Dreieck” (“On the Theorem of the Equality of the Base Angles in the Equilateral Triangle”) he defined the axiomatic method as follows (Hilbert *1902/03*, 50):

> I understand under the *axiomatic* exploration of a mathematical truth [or theorem] an investigation which does not aim at finding new or more general theorems being connected to this truth, but to determine the position of this theorem within the system of known truths in such a way that it can be clearly said which conditions are necessary and sufficient for giving a foundation of this truth.

For Hilbert the axiomatic method is an architectural procedure which unveils the relations between conditions and conclusions. The order produced with the help of the axiomatic method allows to relate a proposition to those conditions which are relevant for determining its validity. This ordering effort is a condition for the construction of theories in mathematics. In the winter term of 1919/20 he gave a lecture course on *Nature and Mathematical Knowledge* where he sharpened this idea. Hilbert spoke of progressive and regressive tasks of mathematics. The progressive task consists in developing systems of relations and investigating their logical consequences. The regressive task consists in determining the conditions of a theory on the base of a clear distinction between suppositions and logical consequences. Hilbert spoke of the universality of these tasks. They are not restricted to mathematics (*1992*, 18):

> These two tasks of mathematical thinking are of very general importance. They are not restricted to the sphere of natural sciences. They also hold for other fields of knowledge, e. g. national economy (theory of money). Even in philosophy there are some attempts to give

> effect to the mathematical way of thinking. Spinoza, e. g., imitates the progressive method in his main work, the "ethics", whereas Nelson recently makes use of the regressive method of mathematics in his philosophy.

Hilbert stressed (*ibid.*):

> This regressive method finds its perfect expression in what is called today "axiomatic method". This is a general method of scientific research as such, it celebrates, however, its most brilliant triumphs in mathematics.

We should keep in mind: Hilbert used the expression "axiomatic method" for discovering and marking the starting points of deductions. It stands for the way of axiomatizing a field of knowledge, not for the axiomatic presentation of this field in textbook style.

For Hilbert, the axiomatic method is a representation of mathematical thought, but again, its application is not restricted to mathematics. It can even be applied to sets of statements that cannot be based on axioms. Hilbert himself mentions as an example, the work of the Göttingen philosopher Leonard Nelson. Like Kant, Nelson strictly rejected attempts to apply the mathematical method, i. e., the axiomatic-deductive method, to philosophical questions. In his *Kritik der praktischen Vernunft* of *1917*, however, he proposed to erect ethics by means of the axiomatic method. He even dedicated the book to Hilbert, writing: "The author dedicates this attempt to open up a province for the sovereignty of rigorous science in gratitude and veneration to his teacher and friend David Hilbert" (Nelson *1917*, V). The new province appears to be ethics, and by "rigorous science" is meant a science ordered as a Hilbert style axiomatic system. Given the above definition of "axiomatic method" it is no contradiction that ethics should be axiomatized like geometry, even if one accepts Nelson's assertion that the principles of ethics are no axioms in the Euclidean sense. Nelson identified the axiomatical method with a decompositional procedure (Nelson *1908*, § 167, p. 780; *Gesammelte Schriften* II, 363), understood in the Kantian sense. Nelson referred to Immanuel Kant's definition of transcendental analysis in the *Critique of Pure Reason* as the decomposition "of the entirety of our *a priori* cognition into the elements of the pure cognition of the understanding" (Kant *1787*, B 89). In the *Prolegomena* he called the decompositional procedure "analytical method" and introduced the descriptive name "regressive method" (*1783*, A 42, § 5, note):

> The analytical method, so far as it is opposed to the synthetical, is very different from that which constitutes the essence of analytical propositions: it signifies only that we start from what is sought as if

> it were given, and ascend to the conditions under which it is possible. In this method we often use nothing but synthetical propositions in mathematical analysis and it were better to term it the regressive method in contradistinction of the synthetic or progressive.

I used Paul Carus' translation of Kant's text (Kant *1902*), but I would like to note that Carus translated "method" where Kant wrote the Wolffian term "Lehrart", i. e. "way of teaching" or "way of presentation".

Let's go back to Hilbert. Hilbert intended to use the axiomatic method for "deepening the foundations" (Hilbert *1918*, 407). This metaphor shows the dynamical character and the pragmatical aims of Hilbert's foundational efforts. Such a foundational procedure does not necessarily come to a final or absolute end. It only aims at providing an unproblematic working field for the mathematician by proving the consistency of the systems he or she is working with. The epistemic status of Hilbert style axioms is open, they are regarded as hypotheses, and it is not the task of the mathematician to get rid of this hypothetical status. This is the point where a philosophical foundation could be brought in, done, however, by philosophers. Among the conceptions Hilbert preferred we find the transcendental foundation of axioms, using transcendental deduction, "deduction" understood as justification, i. e. again an analytical method.

3 The Process of Science

This is the end of my survey of some highlights of the discussion concerning the analytical regressive method in history. It is a common feature of my examples that science is regarded as a problem solving activity consisting of two basic parts that can be reconstructed according to an analysis–synthesis scheme. The analytical branch stands for the procedure which starts with the formulation of the problem and ends with the determination of the conditions for its solution. The synthetical branch represents the way from the conditions to the actual solution of the problem. It seems to be indisputable that science aims at presenting knowledge on the base of solved problems in textbook style. So the synthetical branch represents the main purpose of science. This branch of the scheme is, however, deeply connected with the complementary analytical branch. Synthesis cannot be isolated, but presupposes analysis. It seems to be a matter of course that a reflection on science on level (1) should take both branches into account. Astonishing enough, this is not always the case. In wide ranges of the philosophy of science the analytical branch of research is regarded as irrelevant or not interesting. Take, e. g., Kant's distinction between *quid iuris* and *quid facti* questions in philosophy (cf. Kant *1787*, 116–118). According to Kant, philosophy is mainly concerned with *quid iuris* questions, i. e., with justification. For him, the matters of fact, e. g., the genesis of ideas is no philosophical problem. Take Karl Popper's opinion that the way theories are set up is not capable, but also does not require logical analysis (*1934*, 6). This assertion makes clear that logical analysis in Popper's sense is not identical with level (1) regressive analysis in the process of science I was speaking of. Take Hans Reichenbach's important distinction between the "context of discovery" and the "context of justification". For Reichenbach, philosophy or, as he says, epistemology is only concerned with justification. It aims at a "rational reconstruction" of the scientific process, coming close to the manner a mathematician presents a new proof or a physicist publishes his considerations on the foundations of a new theory (Reichenbach *1938*). Like Popper, Reichenbach reduces the analytical part to the "eureka" effect, i. e., the historical incident of discovery. The responsibility for doing research on the analytical part of science is laid into the hands of empirical psychologists. Popper, Reichenbach and their followers obviously disregard fields to which they could contribute effectively.

On the other hand, certain directions in the sociology of science or the sociology of knowledge, and pragmatic directions in the philosophy of science overestimate the analytic branch of the scientific process. The way in which scientific knowledge is produced, and its historical, social and institutional conditions are the foci of these movements. This is as such no fault, but some of these movements maintain that their program makes the synthetical part superfluous.

Let me illustrate the problems which result from focusing on only one side of the analysis–synthesis scheme with examples from the philosophy of mathematics. Those who maintain the aprioricity of mathematical propositions like Kant or who are fixed to their tautological character like the representatives of the Vienna Circle cannot claim to explain all of mathematics. They ignore the analytical side in the scheme by excluding the practical aspect of doing mathematics. The main activities in mathematics, however, do not concern proving but conjecturing and looking for proofs for the conjectures.

On the other hand, the justification of mathematical truth with the help of arguments reflecting what a mathematician is doing when doing mathematics, ignores the synthetical branch of the scheme. Examples are some sociological directions like David Bloor's strong program of a sociology of knowledge (Bloor *1976*), but also reconstructive directions like John Stuart Mill's (cf. Mill *1843*) or Bertrand Russell's (cf. Russell *1973*) interpretation of mathematics as an empirical science, the early Quine's problems in seeing any difference in concept formation in physics and in mathematics (Quine *1951*), Hilary Putnam's conviction that success is *the* criterion of truth of a mathematical proposition (Putnam *1975*, 61), or Penelope Maddy's opinion that we can legitimately argue from "this theory has properties we like" to "this theory is true" (Maddy *1997*, 163). These authors confuse the analytical and synthetical parts in the process of doing science. They confuse the way of finding a foundation with the foundation itself, and obviously think that the possibility of formulating a candidate for a foundational base is reason enough to believe in its validity, if it is only successful in the sense that most members of a relevant group of scientists accept it or that it proved to be fruitful.

As already said, the directions which reduce discovery in science to the "eureka" effect usually prefer the synthetic part of the scheme. The reason is that actual incidents of discovery are contingent incidents, partially based on intuition or lucky ideas, which can be described, but not explained using methods of rational reconstruction and scientific methodology. Carl Gustav Hempel might be right that there is no logic of discovery (Hempel *1965*, 6), at least according to his strict view of logic. But although scientific discovery is not determined by methodological means, it is highly supported by these means, and logic plays in this context, of course, an important role. Again the philosophy of mathematics is my example. Besides "quasi-empirical" methods like the trial-and-error method or confirmational procedures, a lot of different logical tools can be applied, like Charles S. Peirce's inference scheme of abduction (e. g., Peirce *1903*), Carlo Cellucci's computational logic (cf. Cellucci *1993*, *1996*, *1998*, *2000*) Kevin T. Kelly's logic of reliable inquiry (Kelly *1996*) and inductive, probabilistic, non-monotonic logics. These kinds of a logic of discovery should therefore be part of the scope of a scientific methodology.

The combination of the two branches of analysis and synthesis has been applied to several fields of artificial intelligence, theoretical computer science, and in the area of programming methodology (cf., e. g., Mäenpää *1993*, *1998*).

Let's finally come back to Russell. Speaking of the benefits of the regressive method Russell mentions three points of which I would like to cite the first two (*1973*, 282–3):

> In the first place, when a number of facts are shown to follow from a few premises, this is not only a new truth in itself, but also an organization of our knowledge, making it more manageable and more interesting. In the second place, the premises, when discovered, are pretty certain to lead to a number of new results which could not otherwise have been known: in the sciences, this is so obvious that it needs no illustration, and in mathematics it is no less true.

I think that Russell is perfectly right maintaining the creative and innovative character of structuring efforts, and this insight is a challenge to all doctrines that reduce their interest in logical and mathematical propositions to their character as tautologies.

References

Arnauld, Antoine / Nicole, Pierre *1662* Anon., *La Logique ou l'Art de Penser contenant, outre les règles communes, plusieurs observations nouvelles, propres à former le jugement*, Charles Savreux: Paris; last edition supervised by the authors 6th ed., Abraham Wolfgang: Amsterdam 1685; English translation Arnauld/Nicole *1996*.

— *1996 Logic or the Art of Thinking. Containing, Besides Common Rules, Several New Observations Appropriate for Forming Judgement*, transl. and ed. by Jill Vance Buroker, Cambridge University Press: Cambridge (= *Cambridge Texts in the History of Philosophy*).

Berning, Matthias *1997 Analysis und Determination. Eine Studie zur Erkenntnistheorie der Mathematik bei Descartes*, Hartung-Gorre Verlag: Constance.

Bloor, David *1976 Knowledge and Social Imagery*, University of Chicago Press: Chicago, reprinted 1991.

Cellucci, Carlo *1993* "From Closed to Open Systems," in *Philosophie der Mathematik. Akten des 15. Internationalen Wittgenstein-Symposiums I. 16. bis 23. August 1992. Kirchberg am Wechsel (Österreich)*, ed. by Johannes Czermak, Verlag Hölder-Pichler-Tempsky: Vienna, 206–220.

— *1996* "Mathematical Logic: What Has it Done for the Philosophy of Mathematics," in Piergiorgio Odifreddi (ed.), *Kreiseliana. About and Around Georg Kreisel*, A K Peters: Wellesley, MA, 365–388.

— *1998 Le Ragioni della Logica*, Laterza: Rome (= *Biblioteca di Cultura Moderna*; 1136).

— *2000* "The Growth of Mathematical Knowledge: An Open World View," in *The Growth of Mathematical Knowledge*, ed. by Emily Grosholz/Herbert Breger, Kluwer: Dordrecht/Boston/London (= *Synthese Library*, vol. 289), 153–177.

Hempel, Carl G. *1965* "Studies in the Logic of Confirmation," in Hempel, *Aspects of Scientific Explanation and other Essays in the Philosophy of Science*, The Free Press: New York/Collier-Macmillan: London. Original editon *Mind* **54** (1945), 1–26, 97–121.

Hilbert, David *1899* "Grundlagen der Geometrie," in *Festschrift zur Feier der Enthüllung des Gauss-Weber-Denkmals in Göttingen*, ed. by the Fest-Comitee, Leipzig, 1–92, 14th ed. Hilbert *1999*. Engl. translation by E. J. Townsend, *The Foundations of Geometry*, Chicago: Open Court 1902.

— *1902/03* "Über den Satz von der Gleichheit der Basiswinkel im gleichschenkligen Dreieck," *Proceedings of the London Mathematical Society* **35**, 50–68; shortened in Hilbert *1999*, 133–158.

— *1918* "Axiomatisches Denken," *Mathematische Annalen* **78**, 405–415; reprinted in Hilbert, *Hilbertiana. Fünf Aufsätze*, Wissenschaftliche Buchgesellschaft: Darmstadt 1964, 1–11. Engl. translation "Axiomatic Thought," in *From Kant* *to Hilbert: A Source Book in the Foundations of Mathematics*, ed. by William Ewald, vol. 2, Clarendon Press: Oxford 1996, 1105–1115.

— *1992 Natur und mathematisches Erkennen. Vorlesungen, gehalten 1919–1920 in Göttingen. Nach einer Ausarbeitung von Paul Bernays*, ed. by David E. Rowe, Birkhäuser Verlag: Basel/Boston/Berlin.

— *1999 Grundlagen der Geometrie. Mit Supplementen von Paul Bernays*, 14th ed. by Michael Toepell, B. G. Teubner: Stuttgart/Leipzig.

HINTIKKA, Jaakko/REMES, Unto *1974 The Method of Analysis. Its Geometrical Origin and Its General Significance*, Reidel: Dordrecht/Boston (= *Boston Studies in the Philosophy of Science*; XXV).

HYLTON, Peter *1990 Russell, Idealism and the Emergence of Analytic Philosophy*, Oxford University Press: Oxford.

JONES, Alexander *1986* "Introduction," in Pappus *1986*, 1–80.

KANT, Immanuel *1783 Prolegomena zu einer jeden künftigen Metaphysik die als Wissenschaft wird auftreten können*, Johann Friedrich Hartknoch: Riga. Akademie edition (Kant *1902–*), vol. 4, 52–383, English translation Kant *1902*.

— *1787 Critik der reinen Vernunft* [*KrV*], 2. Aufl., Johann Friedrich Hartknoch: Riga; Akademie edition (Kant *1902–*), vol. 3; Engl. translation Kant *1998*.

— *1902 Prolegomena to Any Future Metaphysics*, ed. by Paul Carus, Open Court: La Salle, Ill.; quoted according to Kant 1996.

— *1902– Kant's gesammelte Schriften*, ed. by the Königlich Preußische Akademie der Wissenschaften, Georg Reimer: Berlin, later publisher Walter de Gruyter: Berlin/Leipzig [Akademie edition].

— *1998 Critique of Pure Reason*, translated and ed. by Paul Guyer/Alan W. Word, Cambridge University Press: Cambridge (= *The Cambridge Edition of the Works of Immanuel Kant*).

KELLY, Kevin T. *1996 The Logic of Reliable Inquiry*, Oxford University Press: New York/Oxford (= *Logic and Computation in Philosophy*).

LEIBNIZ, Gottfried Wilhelm *1683/85* "De Synthesi et Analysi universali seu Arte inveniendi et judicandi," in Leibniz, *Sämtliche Schriften und Briefe*, ser. 6, vol. 4, pt. A, Akademie Verlag: Berlin 1999, no. 129, 538–545.

— *1688* "De arte characteristica ad perficiendas scientias ratione nitentes," in Leibniz, *Sämtliche Schriften und Briefe*, ser. 6, vol. 4, pt. A, Akademie Verlag: Berlin 1999, no. 189, 909–915.

MÄENPÄÄ, Petri *1993 The Art of Analysis. Logic and History of Problem Solving*, Ph. D. Dissertation, Helsinki.

— *1998* "Analytic Program Derivation in Type Theory," in *Twenty-five Years of Constructive Type Theory. Proceedings of a Congress Held in Venice, October 1995*, ed. by Giovanni Sambin/Jan M. Smith, Clarendon Press: Oxford 1998, 113–126.

Maddy, Penelope *1997 Naturalism in Mathematics*, Oxford: Clarendon Press.

Mill, John Stuart *1843 A System of Logic, Ratiocinative and Inductive. Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation*, 2 vols., J. W. Parker: London.

Nelson, Leonard *1908* "Über das sogenannte Erkenntnisproblem," *Abhandlungen der Fries'schen Schule* n. s. 2 (1908), 4th issue, 413–818; again in Nelson, *Gesammelte Schriften in neun Bänden*, vol. 2: *Geschichte und Kritik der Erkenntnistheorie*, Felix Meiner: Hamburg 1973, 60–391.

— *1917 Kritik der praktischen Vernunft*, Verlag "Öffentliches Leben": Göttingen (= Nelson, *Vorlesungen über die Grundlagen der Ethik*, vol. 1; again in Nelson, *Gesammelte Schriften in neun Bänden*, vol. 4, Felix Meiner: Hamburg 1972.

Pappus of Alexandria *1986 Book 7 of the* Collection, ed. by Alexander Jones, vol. 1, Springer: New York etc. (= *Sources in the History of Mathematics and Physical Sciences*; 8).

Peirce, Charles S. *1903* "Lectures on Pragmatism", in Peirce, *Collected Papers*, 6 vols., ed. by Charles Hartshorne/Paul Weiss, Harvard University Press: Cambridge, Mass. 1931–135, 5.14–212.

Peckhaus, Volker *2000* "Análisis regresivo," *Adef Revista de Filosofia* (Buenos Aires) **15**, No. 2 (November 2000), 23–38.

Popper, Karl R. *1934 Logik der Forschung*, Julius Springer: Vienna. English translation *The Logic of Scientific Discovery*, Hutchinson: London 1959, ^{10}1980.

Putnam, Hilary *1975* "What is Mathematical Truth," in Putnam, *Mathematics, Matter and Method*, Cambridge University Press: Cambridge ^{2}1979, reprinted 1995 (Putnam, *Philosophical Papers*, vol. 1), 60–78.

Quine, W. V. O. *1951* "Two Dogmas of Empiricism," in Quine, *From a Logical Point of View*, Harvard University Press: Cambridge, Mass., reprinted 1980, 20–46.

Reichenbach, Hans *1938 Experience and Prediction*, The University of Chicago Press: Chicago.

Russell, Bertrand *1973* "The Regressive Method of Discovering the Premises of Mathematics," in Russell, *Essays in Analysis*, ed. by Douglas Lackey, George Allan & Unwin: London, 272–283.

Szabó, Árpád K. *1974* "Working Backwards and Proving by Synthesis," in Hintikka/Remes *1974*, 118–130.

Wolff, Christian *1710 Der Anfangs-Gründe aller Mathematischen Wissenschaften Erster Theil, Welcher Einen Unterricht von der Mathematischen Lehr-Art, die Rechen-Kunst, Geometrie, Trigonometrie und Bau-Kunst in sich enthält*, Renger: Frankfurt/Leipzig, ^{7}1750; Reprint of the 7th ed., Christian Wolff, *Gesammelte Werke*, ed. Jean École etc., sect. I, vol. 12, ed. J. E. Hofmann, Olms: Hildesheim/New York 1973.

ZABARELLA, Jacobus *1597 Iacobi Zabarellae Patavini Opera Logica . . . Editio Tertia*, Lazarus Zetzner: Cologne; reprint ed. by Wilhelm Risse, Olms: Hildesheim 1966.