DYNAMIC INTERACTIONS WITH THE PHILOSOPHY OF MATHEMATICS†

Donald GILLIES*
Yuxin ZHENG**

* Department of Philosophy, King's College London, Strand, London WC2R 2LS, U.K.
E-mail: Donald.Gillies@kcl.ac.uk

** Department of Philosophy, Nanjing University, Nanjing 210093, China.

BIBLID [0495-4548 (2001) 16: 42; p. 437-459]

ABSTRACT: *Dynamic interaction* is said to occur when two significantly different fields A and B come into relation, and their interaction is dynamic in the sense that at first the flow of ideas is principally from A to B, but later ideas from B come to influence A. Two examples are given of dynamic interactions with the philosophy of mathematics. The first is with philosophy of science, and the second with computer science. The analysis enables Lakatos to be characterised as the first to develop the philosophy of mathematics using ideas taken from the philosophy of science.

Keywords: Dynamic Interactions, Philosophy of Mathematics, Philosophy of Science, Computer Science.

CONTENTS

1. Introduction

Imre Lakatos' work in philosophy focused on the study of the growth of knowledge -particularly in mathematics and science. He suggested new ways of analysing this growth, namely the pattern of 'proofs and refutations' in mathematics, and the concept of 'research programme' for science. The present paper is very much in this tradition, and tries to formulate a concept, that of *dynamic interaction*, which we believe is a commonly occurring pattern in the development of ideas and the growth of knowledge. This concept could be used for the study of many examples of the growth of mathematics and science. However in this paper we propose to apply it to the development of philosophy of mathematics, which, we will claim, has involved significant interactions in the twentieth century with two other fields: philosophy of science and computer science. The first of these interactions establishes another link with Imre Lakatos whom we characterise as being the first significant thinker to apply ideas from the philosophy of science to the philosophy of mathematics.

2. The Concept of Dynamic Interaction

Let us start then with our characterisation of the concept of dynamic interaction. It has the following four features.

(1) A connection is established between two different fields or subjects (A and B say).

(2) Both sides may then benefit from this connection, or, more specifically, from the resulting interaction.

(3) The relationship between the two sides is not static but dynamic. Suppose, for example, the flow of ideas is at first principally from A to B. It will then characteristically happen that, after a while, the direction of the flow of ideas is reversed and ideas from B start to influence A. Typically the interaction between A and B has two phases separated by a turning point, although, as is to be expected, the turning point is not precisely defined.

(4) Although the two sides interact, it is important that each one should at the same time preserve some degree of autonomy. As our above analysis of the dynamic nature of their relationship in (3) shows, this is a necessary condition for further development.

This concept of dynamic interaction has been developed from the work of Emily Grosholz. In a series of publications (1981, 1985, 1991, 1992), Grosholz has studied a number of cases in which knowledge (particularly mathematical knowledge) has advanced through the interaction of separate domains. In 1981, she considers Logic and Arithmetic, in 1985 Logic and Topology, while in her 1992 she argues that Leibniz invented and developed the calculus by bringing together geometry, algebra, number theory, and mechanics. Her 1991 book shows that to a remarkable extent all Descartes' intellectual work can be seen as bringing together different domains. As she says (1991, pp. 2-3):

> (...) Cartesian domains (...) can be understood as a novel amalgamation of formerly distinct or at least very incompletely unified domains: the *Geometry* brings together geometry and algebra, the *Principles* geometry and physics, the *Treatise of Man* physics and medical physiology, and the *Meditations* mechanical philosophy and scholastic theology.

Although Grosholz approves of the method of bringing together separate domains, she nonetheless criticizes the way in which Descartes carries out this process. In her view the interaction of different domains is most fruitful, if, while interacting, they nonetheless retain some degree of autonomy. An attempt to reduce one domain to the other will generally inhibit fruitful developments. As she says (1991, p. 3):

> (...) the unification of domains contributes to the growth of knowledge when and because it exploits partially shared structure between domains that none the less retain their autonomy and distinctness. Revelation is impaired when domains are held too far apart, or assimilated too closely. But Descartes's way of constructing knowledge can produce both these unfortunate outcomes (...).

Grosholz's stress on the need for interacting domains to retain some degree of autonomy seems to us correct, and we have incorporated this idea into our concept of dynamic interaction.

Having thus introduced the concept of dynamic interaction, let us now examine how it applies to philosophy of mathematics in the twentieth century.

3. *First Example: Interactions between Philosophy of Mathematics and Philosophy of Science*

We will begin by arguing that the philosophy of science of the Vienna Circle was strongly influenced by philosophy of mathematics. The Vienna Circle put forward a new conception about the essence of philosophy and laid particular emphasis on the great importance of the method of logical analysis for philosophy, thus developing a new tradition in philosophy, i.e. analytic philosophy which had been introduced by Frege, Russell and Wittgenstein, and which has now dominated philosophy in the English-speaking world for a long time. The famous pamphlet of the Vienna Circle (Scientific Conception of the World) said clearly on this point (Hahn et al. 1929, p. 8):

> The task of philosophical work lies in this clarification of problems and assertions, not in the propounding of special "philosophical" pronouncements. The method of this clarification is that of *logical analysis*; (...).

The Vienna Circle were strongly influenced by philosophy of mathematics -particularly logicism. Carnap had studied under Frege (cf. his 1963), while Hahn conducted a seminar on Russell and Whitehead's *Principia Mathematica* in the academic year 1924-25 during which the participants went through that work chapter by chapter. This is not to say of course that philosophy of mathematics was the only influence on the Vienna Circle. Their study of Einstein's work on relativity was most important as well, and there were other significant influences. Moreover, it should be emphasized that the Vienna Circle's work in the field of the philosophy of science was inseparably connected with their general basic philosophical position, so that only by taking the latter as a background for analysis, can we understand properly the nature of their work in this special field, and especially the great influence of the philosophy of mathematics.

For example, only from such a perspective, can we understand clearly the target which the Vienna Circle set for themselves in the field of the philosophy of science, because the latter was in fact a concrete manifestation of their basic philosophical position in this field. The two main features of the Vienna Circle (or of logical positivism in general) are no doubt their opposition to so-called metaphysics (or, which is the same, their insistence on the basic principle of empiricism) and the emphasis on the method of logical analysis. Thus, their basic philosophical position directly determined their target in the field of the philosophy of science, that is, to make clear the real meaning of every scientific proposition and concept by reducing them step by step to those propositions or concepts which are about or relate directly to what is given in experience (otherwise, if it appears impossible to construct a term out of the given, all the propositions including this term must be regarded as senseless, i.e., as pseudo-propositions of a metaphysical nature), and thus, as a whole, to construct or reconstruct the whole system of science on the basis of the given.

In this programme, we can see clearly the great influence of the philosophy of mathematics: it was precisely the foundational work of logicism in the philosophy of mathematics, i.e. how to build or rebuild the whole of mathematics on the basis of logic, which had provided the necessary example for the Vienna Circle.

However, just as the logicists' work of reducing the whole of mathematics to logic had encountered serious difficulties, the Vienna Circle's efforts at constructing a whole 'reducing system' for science did not proceed in calm water either, and this led to further theoretical thinking in this field. In particular, people began to become aware of the great importance of the following questions. What is the real empirical basis of science? Is it personal experience or the records of public observation? And what is the relationship between the so-called 'theoretical propositions' and 'observation propositions', and in what sense can observation propositions confirm related theoretical propositions?

In comparison with concrete constructions or reconstructions of scientific theories, the above thinking obviously reached a deeper level, because its subject was no longer the concrete logical structure of any particular scientific theory but rather scientific theories in general. Thus, it in fact represented a great change in the Vienna Circle's conception of philosophy of science. That is, the philosophy of science is nothing but meta-science.

Obviously, we can see here once again the influence of the philosophy of mathematics, because the concept of 'meta' was borrowed from the philosophy of mathematics. It is in fact a concept originating from Hilbert's foundational studies, i.e., the concept 'meta-mathematics'.

So, on the whole, it is justifiable to say that the Vienna Circle used in their studies of the philosophy of science ideas and concepts originating from the philosophy of mathematics. Moreover, in the first decades of the twentieth century, the flow of ideas and influence in the interaction relationship was mainly from the philosophy of mathematics to the philosophy of science.

About the 40's of this century, the philosophy of mathematics entered a stagnant and pessimistic period; but the philosophy of science was at the same time just on the eve of stepping out of the epoch of logical positivism, and entering a new era of growth. From the perspective of this paper, one of the most important reasons for the latter development of the philosophy of science is that, while under the deep influence of the philosophy of mathematics, it retained some degree of autonomy. In particular, the philosophy of science has from its very beginning had some special problems which are quite different from the foundational problems of the philosophy of mathematics and it was specifically around these problems that the philosophy of science began its own development. For example, there was firstly the debate between the logical positivists and Popper on the question of what was the main feature of scientific propositions: confirmation or refutation; then, covering a wider area, we see the opposition between logical empiricism and historicism[1] on the essence of science; and finally there was the emergence of the new historicism which made severe criticisms of all the older traditions in the field. Thus, on the whole, the philosophy of science has, since it broke away from the older tradition of logical positivism, made great progress.

As there are so many new concepts, problems and ideas appearing in modern studies of the philosophy of science, it has attracted many scholars working in the field of the philosophy of mathematics. For example, it is quite natural for people in the latter field to think: Should we in the study of the philosophy of mathematics consider the same (or similar) questions which have proved so important for the understanding of the essence of science? And, could those concepts or ideas which have taken a key position in modern studies of the philosophy of science also be transplanted or extended into the field of the philosophy of mathematics?

For example, it was exactly in such a mood that Crowe (1975), Mehrtens (1976) and Dauben (1984) made consecutively contributions to the problem of the applicability of Kuhn's theory of scientific revolutions to mathematics. And furthermore, the following words of Tymoczko (1985, p. 127) seem to represent the general attitude of those working in this new direction: "Indeed the philosophy of science seems to be making progress (...) Why not the philosophy of mathematics?".

As far as the relationship between the philosophy of mathematics and the philosophy of science is concerned, the above situation clearly represents a radical change: it is now the philosophy of science which is beginning to acquire the more important position and to influence the study of the philosophy of mathematics.

Lakatos' work can be regarded as the turning point in the above transition. As we observed earlier, the turning point is never precisely defined, and this is shown in the present case by the fact that Lakatos' work proceeded in two different directions. Firstly it went from the philosophy of science to the philosophy of mathematics. That is, by extending Popper's fallibilist philosophy of science into the field of mathematics, Lakatos developed his quasi-empirical view of mathematics. Secondly, using his logic of mathematical discovery as the basic conceptual framework, Lakatos developed his philosophy of science, i.e. the methodology of scientific research programmes (cf. Zheng 1990), and therefore moved in the opposite direction, i.e. from the philosophy of mathematics to the philosophy of science. (Lakatos had also planned to use his new philosophy of science to improve his philosophy of mathematics; but unfortunately, his early death precluded this possible line of development). Thus, Lakatos can in fact not only be regarded as the person who brought about the turning point of the radical change of the dominating relationship between the philosophy of mathematics and the philosophy of science, that is, he is probably the first who used an idea originating in the philosophy of science to develop some new ideas in the field of philosophy of mathematics, but also as a person who successfully made a good balance between these two fields and thus made substantial progress in both fields through interdisciplinary studies.

Table 1 (cf. Zheng 1990) is a brief summary of the whole development of Lakatos' philosophy, which also shows clearly the dynamic interaction between these two subjects.

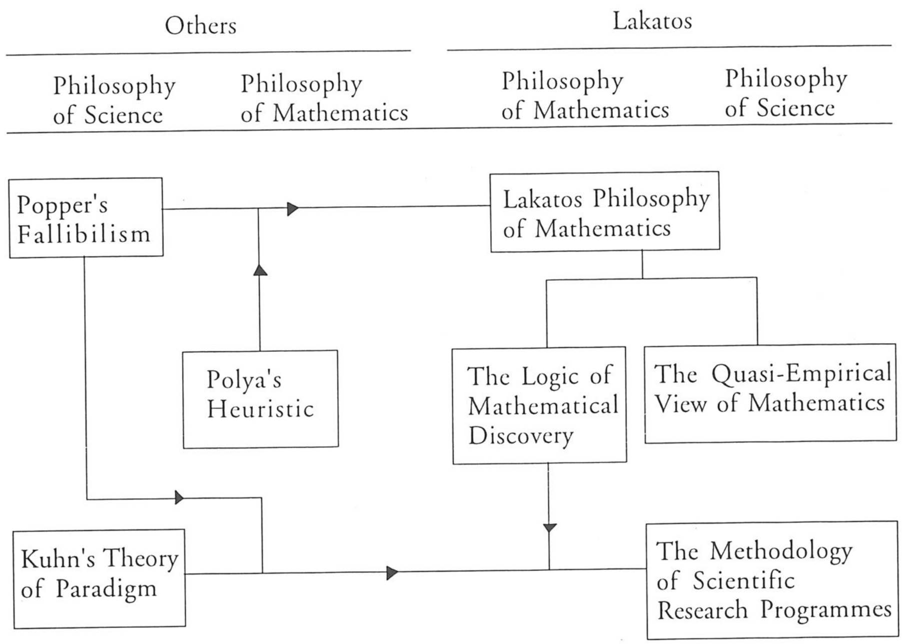

Table 1

Although the initial work in the direction from the philosophy of science to the philosophy of mathematics consisted mainly of extending and transplanting, the new influence of the philosophy of science on the further development of the philosophy of mathematics is profound. In our view this influence, in combination with self-conscious reflections on previous work in the field, especially the critique of the foundationalist philosophy of mathematics, has in fact led to a revolution in the philosophy of mathematics, because the modern development in this field compared to the older foundationalist philosophy of mathematics represents not only a radical change in the basic views of mathematics, but also big changes in what are taken to be the field's central problems, methodology, and basic nature.

In further work in the field of the philosophy of mathematics, we should keep a look-out for instructive concepts, ideas and problems from the philosophy of science which could be absorbed and assimilated. But, while confirming the positive influence of the philosophy of science, we at the same time should also maintain some autonomy for the philosophy of mathematics, especially by paying more attention to the peculiarity of mathematics.

In this connection, Echeverria, Ibarra and Mormann really focused on the major point when they said in their *Introductory Afterthought* (1992, p. xv):

> What has modern philosophy of science to tell us about mathematics? Philosophy of science has been philosophy of the empirical sciences. Hence, any answer to this question is driving at the similarities and analogies between mathematical and empirical knowledge.

That is to say, the basic reason why the philosophy of science could exert direct influence on the philosophy of mathematics is exactly that there are some common points between mathematics and science in general. And it is also for this reason that people working in this new direction of the philosophy of mathematics, such as Lakatos, and, at one stage of his development, Putnam, always pay great attention to the analysis of and arguments for the above-mentioned similarities and analogies. However, as was emphasized above, we should at the same time also take note of the peculiarity of mathematics. For example, it is just in this sense that we should affirm clearly not only the empirical but also the quasi-empirical nature of mathematics, because the latter is in fact a direct confirmation of the peculiarity of mathematics.

As a matter of fact, the peculiarity of mathematics can be seen clearly even if we limit ourselves to the discussion about revolutions in mathematics (cf. Gillies 1992). While the corresponding discussion in the philosophy of science chiefly focused on the question of whether 'ordinary periods' of 'normal science' existed during the historical development of science, what concerned the philosophers of mathematics was precisely the opposite question, i.e. whether there have actually ever been revolutions in mathematics.

To sum up, we can now see clearly that the development of both the philosophy of mathematics and the philosophy of science in this century has been to a great extent realised through their dynamic interaction.

4. Second Example: Interactions between Philosophy of Mathematics and Computer Science

We will begin the account of our second example of a dynamic interaction with the philosophy of mathematics by giving a brief survey of the relevant parts of both the philosophy of mathematics and computer science. The sixty years between 1879 and the outbreak of the second world war was the *foundational period* in the philosophy of mathematics. The subject was dominated by attempts to create a secure foundation for mathematics. The three rival schools: logicism, formalism, and intuitionism, all shared this aim, even though they differed in how it was to be achieved. This period opens with the publication of Frege's *Begriffsschrift* in 1879. It is sometimes thought of as closing with the publication of Gödel's incompleteness theorems in 1931. However, it is probably better to consider the period as continuing until the late 1930's in order to include the development of recursive function theory which arose from Hilbert's programme and Gödel's theorems.

Leaving aside precursors such as Babbage, computer science may be said to begin with the construction of the first electronic computer in the modern sense. Of course it is a matter of argument what exactly an 'electronic computer in the modern sense' is. However, Pratt considers that the best candidates for the first such thing are MADM (Manchester Automatic Digital Machine) which began running in 1948, or the EDSAC at Cambridge (UK), which ran its first stored program in 1949 (Pratt 1987, p. 169). Roughly then the computer and computer science may be said to have begun around 1948 -about a decade after the foundational period in the philosophy of mathematics came to an end.

We will here take computer science in a broad sense to include the design and building of computers, the development of programming and programming languages, and also the development of artificial intelligence, that is the attempt to program computers to simulate behaviour which in humans would be regarded as intelligent. Now the striking fact is that during the first three decades of computer science, those working in the field drew extensively on ideas and theories which had been developed by philosophers of mathematics during the foundational period. Indeed three of the most important computer pioneers: Max Newman (who headed the Manchester team), Alan Turing, and John von Neumann in the United States had all worked on the foundations of mathematics before the war. What makes this situation surprising, and indeed not a little mysterious, is that the philosophers of mathematics of the foundational period were, with one or two exceptions which we will note later, not involved, even in a marginal degree, with questions relating to computing. For purely philosophical reasons they invented theories and techniques which later turned out to be applicable in the new field of computer science. We will now give a brief survey of these ideas from the philosophy of mathematics, and show how they were used in computer science[2]. For reasons of space, we will deal only with ideas drawn from the logicist and formalist programmes in the philosophy of mathematics. Our account could therefore be supplemented by examining the influence of ideas drawn from intuitionism on computer science. The influence of intuitionism has been particularly noteworthy in the work of Per Martin-Löf and his followers (cf. Martin-Löf 1982, and Nordström, Petersson, and Smith 1990).

We will deal with the ideas from the philosophy of mathematics in the reverse order of their invention, and so start with the concept of Turing machine, which was introduced in Turing's famous paper of 1937. A Turing machine is a theoretical computer, and so one of the exceptions to our general rule that the concepts imported from the philosophy of mathematics had been developed with no consideration of computing. Obviously in formulating the idea of a theoretical computer Turing must have had considerations of computing in mind. Yet, as the title of the paper suggests, the concept does appear to have been introduced to solve Hilbert's *Entscheidungsproblem* (problem of decidability) -a problem which had arisen naturally from Hilbert's programme in the foundations of mathematics. It is possible that Turing was already thinking of constructing a real computer in 1937, but there is no direct evidence for this. Hodges who looks into the matter in detail (1983, note 2.38, pp. 545-546) is unable to reach a conclusion, and concludes that 'the question must be left to tantalise the imagination.'

We can however say that, while the concept of Turing machine did arise at least partly from problems within the foundations of mathematics, it subsequently exerted a very great influence on computer science. This influence began right at the beginning, for the theory of Turing machines had an important role in the design of the first electronic computers. In this context, the most relevant point in Turing's original 1937 paper was his formulation of the concept of a *universal machine.* His idea was roughly this. Given any particular machine T say, its table could be coded and placed on the tape of the universal machine U. With this coding in place, U would then exactly simulate the behaviour of T. If T for input i gave output o, so would U.

With this concept of universal machine in mind, let us turn to the related question of what constituted the first computer in the modern sense. I have already given one of the standard answers, namely Manchester's MADM of 1948 or Cambridge's EDSAC of 1949. But is this correct? Much bigger calculating machines had become operational at an earlier date. For example the ENIAC with about 18,000 valves was built by the Moore School of Engineering in Pennsylvania, and became operational in 1946. It was used to perform ballistics calculations for the U.S. military. How, if at all, does the modern computer differ from giant calculating machines of this type? The crucial point is perhaps the following.

If the ENIAC was set up to perform a particular calculation, it could do that calculation very quickly and efficiently, but the process of setting it up for the calculation took a long time. As Pratt says of the ENIAC (1987, 165-166):

> Once a problem had been set up on it, it calculated more quickly than any other contemporary machine. But the process of setting up was complicated and time-consuming. Each of the sub-units that was to play a part in a given calculation had to be configured using banks of switches dispersed throughout the machine; connections between the different sub-units had to be organized (using plug and adaptors); the master programmer unit had to be set, and the necessary constants entered using either switches or a card-reading unit.

One could say that the ENIAC had to be adjusted physically until it became the particular Turing machine needed to perform the calculation required. Rather than going through these complications, would it not be easier to build a universal machine for which one could then write a program to perform any particular calculation? Arguably it is this approach which characterises the modern computer as opposed to the earlier digital calculating machines.

After the war, Turing began working at the National Physical Laboratory on the design of a computer called the Automatic Computing Engine (ACE), though later he transferred to Max Newman's team in Manchester. There is no doubt that Turing's basic idea was to design a machine which would be a kind of practical implementation of the universal machine he had described in his 1937 paper. This is shown clearly in the following passage from a lecture he gave to the London Mathematical Society in February 1947 (quoted from Hodges 1983, pp. 319-320):

> Let us now return to the analogy of the theoretical computing machines with an infinite tape. It can be shown that a single special machine of that type can be made to do the work of all. It could in fact be made to work as a model of any other machine. The special machine may be called the universal machine; it works in the following quite simple manner. When we have decided what machine we wish to imitate, we punch a description of it on the tape of the universal machine. This description explains what the machine would do in every configuration in which it might find itself. The universal machine has only to keep looking at this description in order to find out what it should do at each stage. Thus the complexity of the machine to be imitated is concentrated in the tape and does not appear in the universal machine proper in any way.
>
> If we take the properties of the universal machine in combination with the fact that machine processes and rule of thumb processes are synonymous, we may say that the universal machine is one which, when supplied with the appropriate instructions, can be made to do any rule of thumb process. This feature is parallelled in digital computing machines such as the ACE. They are in fact practical versions of the universal machine. There is a certain central pool of electronic equipment, and a large memory. When any particular problem has to be handled the appropriate instructions for the computing process involved are stored in the memory of the ACE and it is then 'set up' for carrying out that process.

This suggests that we can define a computer in the modern sense as an automatic electronic digital machine with internal program storage[3]. It is this definition which justifies the claim of Manchester's MADM of 1948 to be the first computer. Now the Manchester team were certainly influenced by Turing's ideas, and thus it seems reasonable to claim that the concept of the Turing machine (particularly the idea of a Universal Turing Machine) played a crucial role in the creation of the modern computer.

In the period immediately after the war, attempts to build an electronic computer were being carried out in the United States as well as Britain, and there was in fact collaboration and exchange of ideas between the British and Americans. In the American effort, a key figure was John von Neumann. Before the ENIAC had been completed, some of its deficiencies had become obvious, and in 1944 von Neumann joined the Moore School team as a consultant to consider how these problems could be overcome. The next year the team prepared a proposal for a Successor to ENIAC to be called EDVAC (Electronic Discrete Variable Calculator). The Draft Report on the EDVAC (30 June 1945) was signed by von Neumann, and had a considerable influence on the British as well as the American efforts. It is of great importance in having formulated the concept of stored program. So the question naturally arises as to whether von Neumann was influenced by the concept of the universal Turing machine. The answer is that he almost certainly was, since he knew Turing personally, and was familiar with his work. Indeed, one of von Neumann's colleagues Dr. S. Frankel wrote in 1972 (quoted in Randell 1972, p. 10):

> I know that in or about 1943 or '44 von Neumann was well aware of the fundamental importance of Turing's Paper of 1936 "On computable numbers (...)" which describes in principle the "Universal Computer" of which every modern computer (perhaps not ENIAC as first completed but certainly all later ones) is a realization. Von Neumann introduced me to that paper and at his urging I studied it with care (...) he firmly emphasized to me, and to others I am sure, that the fundamental conception is owing to Turing -insofar as not anticipated by Babbage, Lovelace, and others.

On the other hand, there were very practical reasons for introducing stored programs, and von Neumann may have learnt these from other members of the Moore School team -particularly Eckert and Mauchly[4]. The difficulty with the ENIAC was the slowness involved in setting up the machine, as opposed to the speed with which the electronic calculations could be done. Even the fastest method of instructing the machine what to do, viz. feeding in the information on cards, was much slower that the processes carried out electronically. A natural solution was to build a machine with a memory and store the instructions in this memory where they could be consulted at electronic speed. This was the stored program concept which Turing had reached by considering his universal machine. Thus we can say that this crucial concept arose partly from theoretical considerations and partly from practical engineering considerations.

The influence of the concept of Turing machine on computer science is by no means confined to its role in the design of the first computers. On the contrary it has remained a general tool for the analysis of computers[5]. An example of this is complexity theory which was developed in the 1960's and 1970's. This makes essential use of the theory of Turing machines, and is itself of the very greatest importance for the study of the difficulty of computational problems. Turing's original proof of the equivalence of his notion of Turing computability with Church's notion of λ-definability may also have exercised considerable influence on computer science. Part of the proof consists in the translation of Church's functions into a form in which they can be computed by Turing machines. This is exactly the idea of a compiler which translates a higher level programming language into machine code, and so Turing's proof may have had an influence on the development of compilers.

The preceding example naturally brings us to the contribution of Church whose invention of the λ-calculus arose out of his attempts to develop the logicist position of Russell and Whitehead (1910-13). Russell and Whitehead had written the class of all x's such that f(x) as $\hat{x}$ f(x). Church wished to develop a calculus which focused on functions rather than classes, and he referred to the function by moving the symbol ∧ down to the left of x to produce ∧x f(x). For typographic reasons it was easier to write this as λx f(x), and so the standard notation of the λ-calculus came into being (cf. Rosser 1984, p. 338).

Church had intended his first version of the λ-calculus (1932) to provide a new foundation for logic in the style of Russell and Whitehead. However it turned out to be inconsistent. This was first proved by Kleene and Rosser in 1935 using a variation of the Richard paradox, while Curry in 1942 provided a simpler proof based on Russell's paradox. Despite this set-back the λ-calculus could be modified to make it consistent, and turned out to be very useful in computer science. It became the basis of programming languages such as LISP, Miranda, and ML, and indeed is used as a basic tool for the analysis of other programming languages.

Let us now move back to an earlier stage in the development of the philosophy of mathematics. Bertrand Russell devised the theory of types in order to produce a new version of the logicist programme (the programme for reducing mathematics to logic) when Frege's earlier version of the programme had been shown to be inconsistent by Russell's discovery of his paradox. Thus Russell's motivation was to establish a particular position in the philosophy of mathematics (logicism), and there is no evidence that he even considered the possibility of his new theory being applied in computing. Indeed Russell's autobiographical writings show that he was worried about devoting his time to logicism rather than to useful applied mathematics. This in his 1959 *My Philosophical Development*, he writes of the years immediately following the completion of his first degree (p. 39):

> I was, however, persuaded that applied mathematics is a worthier study than pure mathematics, because applied mathematics -so, in my Victorian optimism, I supposed- was more likely to further human welfare. I read Clerk Maxwell's *Electricity and Magnetism* carefully, I studied Hertz's *Principles of Mechanics*, and I was delighted when Hertz succeeded in manufacturing electro-magnetic waves.

Moreover in his autobiography, Russell gives a letter which he wrote to Gilbert Murray in 1902 which contains the following passage (1967, p. 163):

> Although I denied it when Leonard Hobhouse said so, philosophy seems to me on the whole a rather hopeless business. I do not know how to state the value that at moments I am inclined to give it. If only one had lived in the days of Spinoza, when systems were still possible (...)

In view of Russell's doubts and guilt feelings, it is quite ironical that his work has turned out to be so useful in computer science.

Russell's theory of types failed in its original purpose of providing a foundation for mathematics. The mathematical community preferred to use the axiomatic set theory developed by Zermelo and others. Indeed type theory is not taught at all in most mathematics departments. The situation is quite different in computer science departments where courses on type theory are a standard part of the syllabus. This is because the theory of types is now a standard tool of computer science. Functional programming languages such as Miranda and ML are usually typed, and indeed some form of typing is incorporated into most programming languages. It is desirable when specifying a function e.g. f(x,y) to specify also the types of its variables x, y, otherwise errors can be produced by substituting something of the wrong type for one of the variables which will often produce a nonsensical answer. Of course the type theories used in contemporary computer science are *not* the same as Russell's original type theory, but they are descendants nonetheless of Russell's original system. An important link in the chain was Church's 1940 version of the theory of types which was developed from Russell's theory, and which influenced workers in computer science. Davis sums up the situation very well as follows (1988b, p. 322):

> Although the role of a hierarchy of types has remained important in the foundations of set theory, strong typing has not. It has turned out that one can function quite well with variables that range over sets of whatever type. So, Russell's ultimate contribution was to programming languages!

Lastly we come to the predicate calculus introduced by Frege in his *Begriffsschrift* of 1879 which opened the foundational period in the philosophy of mathematics. This has become one of the most commonly used theoretical tools of computer science. It is fundamental to program or hardware verification. In his 1965 paper, Alan Robinson developed a form of the predicate calculus (the clausal form) which is particularly suitable for use on the computer. This is used both in automated theorem proving, and in some machine learning programs (for some further details about the latter area see Gillies 1996, 2.4, pp. 41-44). Predicate logic is also fundamental to logic programming and the logic programming language

PROLOG (for an account of the development of the latter see Gillies 1996, 4.1, pp. 72-75). At an even more fundamental level, the *Begriffsschrift* is the first example of a fully formalised language, and so, in a sense, the precursor of all programming languages[6].

Frege like Russell was motivated to establish a particular position in the philosophy of mathematics, namely that arithmetic could be reduced to logic. Curiously enough, however, Frege does make one reference to questions of computation. His predecessor Boole had also introduced a system of formal logic, and Jevons, influenced by Babbage, had actually constructed a machine to carry out logical inferences in his own version of Boolean logic. Jevons had the machine constructed by a clockmaker in 1869, and describes it in his paper of 1870. Frege made a number of comments on these developments in a paper written in 1880-1, although only published after his death. He wrote (1880-1, pp. 34-35):

> I believe almost all errors made in inference to have their roots in the imperfection of concepts. Boole presupposes logically perfect concepts as ready to hand, and hence the most difficult part of the task as having been already discharged; he can then draw his inferences from the given assumptions by a mechanical process of computation. Stanley Jevons has in fact invented a machine to do this.

Frege, however, made clear in a passage occurring a little later that he did not greatly approve of these developments. He wrote (1880-1):

> Boolean formula-language only represents a part of our thinking; our thinking as a whole can never be coped with by a machine or replaced by purely mechanical activity.

On the whole it seems that Jevons' attempts to mechanise logical inference had only a slight influence on Frege's thinking. So we can say that considerations of computing had almost no influence on Frege's development of the predicate calculus, and yet the predicate calculus has proved a very useful tool for computer science.

So far in this section we have discussed the striking influence which results developed during the foundational period of the philosophy of mathematics exerted on the development of computer science. Our model of dynamic interaction suggests that after a period of time the reverse influence might show itself, and computer science begin in its turn to have an effect on the development of philosophy of mathematics. In fact we have begun to see signs of this reverse influence occurring in the last two decades.

The part of computer science which has most noticeably begun to influence philosophy of mathematics is the development of automated theorem proving. An excellent survey of this development is to be found in MacKenzie (1995). The first result which provoked discussion among philosophers of mathematics was the proof of the four colour theorem in 1977. The point here was that a computer was used to check through a number of special cases, the calculations involved being simply too long to have been carried out by hand. Indeed it would have been impossible for these calculations to have been checked by a human mathematician. This naturally gave rise to some philosophical discussions since it had been widely assumed before this that a mathematical proof should be humanly surveyable. Indeed Wittgenstein had explicitly formulated this view in the following passage, written in 1939-40 (1956, Part III, p. 143):

> 1. 'A mathematical proof must be perspicuous.' Only a structure whose reproduction is an easy task is called a "proof". (...)
>
> 2. I want to say: if you have a proof-pattern that cannot be taken in, and by a change in notation you turn it into one that can, then you are producing a proof, where there was none before.

On the basis of such arguments, some thinkers have denied that the computer-assisted proof of the four colour theorem was a genuine mathematical proof.

However further developments of automated theorem proving have made the situation more problematic. In the case of the four colour theorem, the computer was used to fill in some details of a proof which had been devised by human mathematicians. Now however some automated theorem provers -particularly those of Larry Wos- can devise proofs without any help from humans, and have even been able to prove some important open conjectures for which human mathematicians had not been able to provide proofs. Moreover some of these computer proofs are not only not surveyable, but need not even be based on any humanly comprehensible general strategy. Clearly these new results require a rethink of the notion of mathematical proof, and have thus given rise to a major problem in current philosophy of mathematics.

So far we have illustrated the way in which philosophy of mathematics has dynamically interacted with computer science. However the influence on computer science of philosophy of mathematics of the foundational period has been not only remarkably extensive but also surprising. It seems worth concluding this section by asking how this influence came about.

The problem as we have already indicated is the following. Those who worked on philosophy of mathematics during the foundational period, such as Frege, Russell, Hilbert, Church, etc. were influenced either not at all or to a negligible extent by considerations to do with computing. Why then did their work later on prove so useful in computer science?

Before the work of Frege, Peano, Russell and Hilbert, mathematics might be described as semi-formal. Of course symbolism was used, but the symbols were embedded in ordinary language. In a typical proof, one line would not in general follow from the previous ones using some simple logical rule of inference. On the contrary, it would often require a skilled mathematician to 'see' that a line followed from the previous ones. Moreover even skilled mathematicians would sometimes 'see' that a line in a proof followed from earlier lines when it did not in fact follow. As a result mistaken proofs were often published, even by eminent mathematicians. Moreover the use of informal language often resulted in ambiguities in the concepts employed, which could create confusions and errors.

Of course mathematics is still done today in this semi-formal style, but Frege, in his quest for certainty, thought that he could improve things by a process of formalisation. Concepts would have to be precisely defined to avoid ambiguities and confusions. The steps in a proof would have to be broken down, so that each individual step involved the application of a simple and obviously correct logical rule. By this process, which Frege thought of as the elimination of anything intuitive, he hoped to eliminate the possibility of error creeping in. As he put it (1884, p. 2): "The aim of proof is (...) to place the truth of a proposition beyond all doubt (...)". His approach led him to develop a formal system of logic, his *Begriffsschrift* (or concept writing) which is equivalent to present day predicate calculus. He explains how this happened very clearly in the following passage (1884, pp. 102-103):

> (...) the mathematician rests content if every transition to a fresh judgement is self-evidently correct, without enquiring into the nature of this self-evidence, whether it is logical or intuitive. A single such step is often really a whole compendium, equivalent to several simple inferences, and into it there can still creep along with these some element from intuition. In proofs as we know them, progress is by jumps, which is why the variety of types of inference in mathematics appears to be so excessively rich; for the bigger the jump, the more diverse are the combinations it can represent of simple inferences with axioms derived from intuition. Often, nevertheless, the correctness of such a transition is immediately self-evident to us, without our ever becoming conscious of the subordinate steps condensed within it; whereupon, since it does not obviously conform to any of the recognized types of logical inference, we are prepared to accept its self-evidence forthwith as intuitive (...)

The demand is not to be denied: every jump must be barred from our deductions. That it is so hard to satisfy must be set down to the tediousness of proceeding step by step. Every proof which is even a little complicated threatens to become inordinately long. And moreover, the excessive variety of logical forms that has gone into the shaping of our language makes it difficult to isolate a set of modes of inference which is both sufficient to cope with all cases and easy to take in at a glance.

To minimize these drawbacks, I invented my concept writing. It is designed to produce expressions which are shorter and easier to take in, and to be operated like a calculus by means of a small number of standard moves, so that no step is permitted which does not conform to the rules which are laid down once and for all. It is impossible, therefore, for any premiss to creep into a proof without being noticed.

It is now easier to see how the methods which Frege used in his search for certainty in mathematics created a system suitable for use in computer science. What Frege is doing is in effect mechanising the process of checking the validity of a proof. If a proof is written out in the characteristic human semi-formal style, then its validity cannot be checked mechanically. One needs a skilled human mathematician to apply his or her intuition to 'see' whether a particular line follows from the previous ones. Once a proof has been formalised, however, it is a purely mechanically matter to check whether the proof is valid using the prescribed set of rules of inference. Thus Frege's work can be seen as replacing the craft skills of a human mathematician with a mechanical process[7].

In effect if a computer is to be able to handle mathematical proofs at all, these proofs must first be formalised, and this explains why the logicist and formalist programmes with their common interest in formalisation created tools which were useful for computer science.

The process of mechanisation in general takes place in something like the following manner. The starting point is handicraft production by skilled artisans. The next step is the division of labour in the workshop in which the production process is broken down into smaller and simpler steps, and an individual worker carries out only one such step instead of the process as a whole. Since the individual steps are now quite simple and straightforward, it becomes possible to get them carried out by machine, and so production is mechanised.

Frege and his successors in the logicist and formalist traditions were carrying our an analogous process for mathematics. Mathematical proofs were broken down into simple steps which at a later stage could be carried out by a machine. From a general philosophical point of view, Frege, Peano, Russell, Hilbert, etc. were engaged in the project of mechanising thought. Since they lived in a society in which material production had been so successful mechanised and in which there was an ever increasing amount of mental (white collar) labour, this project for mechanising thought was a natural one. Moreover it was equally natural that mathematics should be the area chosen to begin the mechanisation process, since mathematics was already partially formalised, unlike other areas of thought.

These considerations perhaps explain why the philosophy of mathematics has assumed such importance within the philosophy of our time. Naturally as well as the thinkers who have pressed forward with the mechanisation of mathematics, there have been those who have objected to this mechanisation, and stressed the human and intuitive aspects of mathematics. Poincaré, Brouwer, Gödel, the later Wittgenstein, and, more recently, Penrose all belong to this trend. Although this line of thought is in many ways reactionary and of course has not halted the advances of mechanisation, there is nonetheless some truth in it, for, as long as mathematics continues to be done by humans at all, it will evidently retain some intuitive characteristics. This is another reason why the logicists and formalists, although they thought they were building a secure foundation for mathematics and rendering its results certain, were in fact creating a form of mathematics suitable for computer science.

Notes

† The authors would like to thank Carlo Cellucci, Joseph Dauben, Pieranna Garavaso, Mark Gillies, Emily Grosholz, and Samin Ishtiaq for reading earlier versions of this paper and making useful comments, many of which were incorporated into the final version. The research which led to the paper was begun during a visit by Yuxin Zheng to King's College London from April to September 1997. This visit was made possible by Yuxin Zheng's receipt of a British Academy K. C. Wong Fellowship, and a travel grant from the Open Society Institute. Both authors would like to thank the British Academy and the Open Society Institute for the support which made this collaborative research possible.

Earlier versions of the paper were read at the Annual Conference of the British Society for the Philosophy of Science in September 1998, to the Logic Club, Department of Philosophy, University of California, Berkeley in November 1998, at a conference on Philosophy and Computing at King's College London in February 1999, and to the Applied Logic Colloquium at Queen Mary College London in November 1999. The authors are very grateful for the comments received on these occasions, and particularly for some points made by Martin Davis at Berkeley, one of which is mentioned in footnote 6 below.

1 Here and subsequently we will use 'historicism' for the view that the study of the historical development of science and mathematics is of great importance for the philosophy of these subjects.

2 The author responsible for this section of the paper (Donald Gillies) would like to thank a number of computer scientists with whom he discussed this problem and who made many helpful suggestions which have been incorporated in the following survey. These include James Cussens, Mark Gillies, Stephen Muggleton, David Page, and Ashwin Srinivasan. He has also benefited from reading some as yet unpublished writings on Alan Turing by Teresa Numerico, who also supplied the useful reference: Stern, 1985. A similar account in Martin Davis' papers 1988a and 1988b proved most helpful, and a great deal of use was made of Hodges' admirable life of Turing (1983).

3 This definition is given by Hodges (1983, p. 295). We regard Hodges analysis on this point as fundamentally sound, and have followed him.

4 Eckert and Mauchly wanted to claim patents for their contributions to ENIAC and EDVAC. They thought that von Neumann's publication of the EDVAC report had damaged their chances of so doing. A good account of the resulting dispute is to be found in Stern (1985).

5 This point and the subsequent examples I owe to conversations with Mark Gillies and David Page.

6 I owe this point to Martin Davis. See his (1988b, p. 316).

7 It should be stressed that this is our way of viewing Frege's work, and that Frege himself would not have seen things in this light. (We owe this point to Carlo Cellucci.)

BIBLIOGRAPHY

Carnap, R.: 1963, 'Intellectual Autobiography', in Schilpp, P. A. (ed.): *The Philosophy of Rudolf Carnap*, La Salle, Open Court, pp. 3-84.

Church, A.: 1932, 'A Set of Postulates for the Foundation of Logic', *Annals of Mathematics* 33, 346-366.

Church, A.: 1940, 'A Formulation of the Simple Theory of Types', *Journal of Symbolic Logic* 5, 56-68.

Crowe, M.: 1975, 'Ten 'Laws' concerning Patterns of Change in the History of Mathematics', reprinted in D. A. Gillies (ed.) 1992, pp. 15-20.

Curry, H. B.: 1942, 'The Inconsistency of Certain Formal Logics', *Journal of Symbolic Logic* 7, 115-117.

Dauben, J.: 1984, 'Conceptual Revolutions and the History of Mathematics: Two Studies in the Growth of Knowledge', reprinted in D. A. Gillies (ed.) 1992, pp. 49-71.

Davis, M.: 1988a, 'Mathematical Logic and the Origin of Modern Computing', in Rolf Herken (ed.): *The Universal Turing Machine. A Half-Century Survey*, Oxford, Oxford University Press, pp. 149-174.

Davis, M.: 1988b, 'Influences of Mathematical Logic on Computer Science', in Rolf Herken (ed.): *The Universal Turing Machine. A Half-Century Survey*, Oxford, Oxford University Press, pp. 315-326.

Echeverria, J., Ibarra, A., and Mormann, T. (eds.): 1992, *The Space of Mathematics: Philosophical, Epistemological, and Historical Explorations*, Berlin, Walter de Gruyter.

Frege, G.: 1879, *Begriffsschrift. Eine der arithmetischen nachgebildete Formelsprache des reinen Denkens*, English translation in Jean van Heijenoort (ed.): 1977, *From Frege to*

Gödel: A Source Book in Mathematical Logic, 1879-1931, Cambridge, Harvard University Press, pp. 1-82.

Frege, G.: 1880-1, 'Boole's Logical Calculus and the Concept-Script', English translation in *Gottlob Frege: Posthumous Writings*, Oxford, Blackwell, pp. 9-52.

Frege, G.: 1884, *The Foundations of Arithmetic: A Logico-Mathematical Enquiry into the Concept of Number*, English translation by J. L. Austin, Oxford, Blackwell, 1968.

Gillies, D. A. (ed.): 1992, *Revolutions in Mathematics*, Oxford, Clarendon Press.

Gillies, D. A.: 1996, *Artificial Intelligence and Scientific Method*, Oxford, Oxford University Press.

Grosholz, E. R.: 1981, 'Wittgenstein and the Correlation of Logic and Arithmetic', *Ratio* 23, 31-42.

Grosholz, E. R.: 1985, 'Two Episodes in the Unification of Logic and Topology', *British Journal for the Philosophy of Science* 36, 147-157.

Grosholz, E. R.: 1991, *Cartesian Method and the Problem of Reduction*, Oxford, Oxford University Press.

Grosholz, E. R.: 1992, 'Was Leibniz a Mathematical Revolutionary?', in D. A. Gillies (ed.) 1992, pp. 117-133.

Hahn, H., Neurath, O. and Carnap, R.: 1929, *The Scientific Conception of the World. The Vienna Circle*, English translation, Reidel, Dordrecht, 1973.

Hodges, A.: 1983, *Alan Turing. The Enigma of Intelligence*, London, Unwin, 1986.

Jevons, W. S.: 1870, 'On the Mechanical Performance of Logical Inference', *Philosophical Transactions of the Royal Society* 160, 497-518.

Kleene, S. C. and Rosser, J. B.: 1935, 'The Inconsistency of Certain Formal Logics', *Annals of Mathematics* 36, 630-636.

Kline, M.: 1972, *Mathematical Thought from Ancient to Modern Times*, Oxford, Oxford University Press.

Kuhn, T. S.: 1962, *The Structure of Scientific Revolutions*, Chicago, University of Chicago Press.

Lakatos, I.: 1962, 'Infinite Regress and the Foundations of Mathematics', reprinted in (1978), pp. 3-23.

Lakatos, I.: 1963-4, *Proofs and Refutations*, Cambridge, Cambridge University Press, 1976.

Lakatos, I.: 1967, 'A Renaissance of Empiricism in the Recent Philosophy of Mathematics', reprinted in (1978), pp. 24-42.

Lakatos, I.: 1978, *Mathematics, Science and Epistemology. Philosophical Papers, Volume 2*, edited by John Worrall and Gregory Currie, Cambridge, Cambridge University Press.

Mackenzie, D.: 1995, 'The Automation of Proof: A Historical and Sociological Exploration', *IEEE Annals of the History of Computing* 17(3), 7-29.

Martin-Löf, P.: 1982, 'Constructive Mathematics and Computer Programming', in Cohen, L. J.; Los, J.; Pfeiffer, H. P.; and Podewski, K. P. (eds.): *Logic, Methodology and Philosophy of Science VI*, Amsterdam, North-Holland.

Mehrtens, H.: 1976, 'T. S. Kuhn's Theories and Mathematics: a Discussion Paper on the 'New Historiography' of Mathematics', reprinted in D. A. Gillies, (ed.): 1992, pp. 21-42.

Nordström, B., Petersson, K., and Smith, J. M.: 1990, *Programming in Martin-Löf Type Theory: An Introduction*, Oxford, Oxford University Press.

Pratt, V.: 1987, *Thinking Machines*, Oxford, Blackwell.

Randell, B.: 1972, 'On Alan Turing and the Origins of Digital Computers', in B. Melzer and D. Michie (eds.): *Machine Intelligence*, 7, Edinburgh, Edinburgh University Press, pp. 3-20.

Rosser, J. B.: 1984, 'Highlights of the History of the Lambda-Calculus', *Annals of the History of Computing* 6(4), 337-339.

Robinson, J. A.: 1965, 'A Machine-Oriented Logic Based on the Resolution Principle', *Journal for the Association for Computing Machinery* 12, 23-41.

Russell, B.: 1959, *My Philosophical Development*, London, George Allen and Unwin.

Russell, B.: 1967, *Autobiography. Volume 1*, London, George Allen and Unwin.

Russell, B. and Whitehead, A. N.: 1910-13, *Principia Mathematica*, Cambridge, Cambridge University Press.

Stern, N.: 1985, 'Minutes of 1947 Patent Conference, Moore School of Electrical Engineering', University of Pennsylvania, *Annals of the History of Computing* 7, 100-116.

Turing, A. M.: 1937, 'On Computable Numbers, with an Application to the *Entscheidungsproblem*', *Proceedings of the London Mathematical Society*, 2nd series, 42, 230-265; correction, *ibid*, 43, 544-546. Reprinted in Davis, Martin (ed.): *The Undecidable*, Hewlett (NY), Raven Press, 1965, pp. 116-154.

Tymoczko, T. (ed.): 1985, *New Directions in the Philosophy of Mathematics*, Boston, Birkhauser.

Wittgenstein, L.: 1956, *Remarks on the Foundations of Mathematics*. Blackwell, Oxford. 3rd edition, revised and reset, 1978.

Zheng, Y.: 1990, 'From the Logic of Mathematical Discovery to the Methodology of Scientific Research Programmes', *British Journal for the Philosophy of Science* 41, 377-399.

Donald Gillies was born in 1944. He studied mathematics and philosophy at Cambridge, and then began graduate studies in Professor Sir Karl Popper's department at the London School of Economics. After completing his Ph.D. on the foundations of probability with Professor Imre Lakatos as supervisor, he joined the staff of the University of London, and is at present Professor of the Philosophy of Science and Mathematics at King's College London. From 1982 to 1985 he edited the *British Journal for the Philosophy of Science*. He has published extensively in philosophy of science and mathematics.

Yuxin Zheng was born in 1944, and is Professor of Philosophy at Nanjing University in China. He is a member of China's Council for Philosophy of Science, and its Special Board for Philosophy of Mathematics. He specialises in the Philosophy of Mathematics, Mathematics Education, and Modern Logics. He has published extensively in these fields in Chinese, and also several articles in English in the West. He has visited the University of London in the UK and Rutgers in the USA, and has collaborated with Western scholars. He has won awards for excellence for his teaching on several occasions.