Leonard Bloomfield

Linguistics and mathematics*

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1. *Introduction*

Although Leonard Bloomfield (1887–1949) has long been recognised as one of the leading linguists of the first half of the 20th century, and, although in recent years various aspects of his work have been subjected to renewed scrutiny, there are still several strands of his research that remain largely unexplored.[1] In particular, Bloomfield's knowledge of developments in specific branches of contemporaneous mathematics, and the consequences this had for his approach to linguistics, are issues that have never really been discussed in sufficient detail. For instance, although Bloomfield's interest in the work of the Vienna Circle has been considered in the past, there has been no extensive attempt to elucidate the precise nature, and full extent, of his familiarity with symbolic logic, recursive function theory, and the technical machinery of Hilbertian Formalism.[2] In addition, although it is known that Bloomfield produced at least one lengthy unpublished manuscript that was primarily concerned with the foundations of mathematics, the implications this research had for his more mainstream linguistics work have never been adequately considered. Accordingly, in this paper, a preliminary attempt is

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* Thanks are due to the many anonymous *HL* reviewers who read and commented upon this paper.

1. Fought ed. (1999) constitutes the best recent collection of Bloomfield-related scholarship.

2. For example, some of Bloomfield's scientific interests are discussed in Robins (1988), but there is no assessment of his preoccupation with the crisis in the foundations of mathematics. Also, while the influence of the Vienna Circle upon Bloomfield's work is discussed in Hiż & Swiggers (1990), there is no detailed consideration of specific mathematical topics. Instead, the focus is primarily upon Bloomfield's reformulation of *Protokollsätze* (i.e., 'sentences of report'), a term which had been used in Vienna Circle publications.

Historiographia Linguistica xxxi:1 (2004), **105–136.**
issn 0302–5160 / e-issn 1569–9781

made to explore Bloomfield's informed preoccupation with mathematics, and the focus falls upon three related themes. First, the sources of his mathematical knowledge are considered in an attempt to reveal the origins of his familiarity with these topics. Second, the basic outline of his proposal for a linguistics-based solution to the crisis in the foundations of mathematics is reconstructed from existing fragments, and the consequences of this work are assessed. Finally, the influence of Formalism upon Bloomfield's linguistic research is considered, with particular reference to his complex attitude towards the status of form and meaning in linguistic theory. As will be demonstrated, apart from being of interest in its own right, a more complete appreciation of Bloomfield's mathematical work sheds new light on specific developments in syntactic theory in the 1940s and 1950s.

2. *The Foundations Crisis*

The story of the foundations crisis that shook mathematics to its core in the early 20th century has been told many times, and there is simply not space here to recount the whole narrative in exhaustive detail. Nevertheless, the main developments must be presented, even if in a cursory fashion, since familiarity with these topics cannot safely be assumed.[3]

In essence, the foundations crisis was precipitated by the discovery of paradoxes. In the late 19th century, Georg Cantor (1845–1918) had developed a branch of mathematics that he referred to as *Mengenlehre*, but which, in the Anglo-American world, would eventually become known as 'set theory'. Although Cantor's work had its origins in his dissatisfaction with contemporaneous approaches to number theory, it was swiftly recognised that most areas of mathematics could be placed upon a set-theoretical foundation. Consequently, the perceived significance of set theory was due in part to the fact that it appeared to provide a unifying framework that would enable the various disparate branches of mathematics to be combined within a common set-theoretical exposition. However, at the start of the 20th century, difficulties began to emerge, and the most enduring problems manifested themselves as set-theoretical paradoxes. The logician-philosopher Bertrand Russell (1872–1970) was perturbed by these paradoxes, or 'fallacies' as he called them, and, during the years 1903–1910, frequently working in conjunction with Alfred

3. A recent authoritative discussion of this multifaceted topic can be found in Grattan-Guinness (2000). For less demanding but equally provocative discussions, try Mancosu (1998) and Kline (1980), esp. Chapters VIII–XIV.

North Whitehead (1861–1947), he published a series of papers in which he attempted to eliminate the paradoxes of set theory by deriving the whole of mathematics from the parsimonious axioms of logic — an approach that became known as Logicism. Although much of this work required Russell and Whitehead to synthesise the research of their predecessors and contemporaries — most notably Gottlob Frege (1848–1925) and Giuseppe Peano (1858–1932) — they also made several significant theoretical contributions themselves, and perhaps the most controversial of these was the 'theory of logical types'. This theory constituted Russell and Whitehead's response to the fact that the paradoxes of set theory invariably involved self-reference of one kind or another, hence the tendency to refer to them as 'vicious-circle fallacies'. To take just one example of such a fallacy, Russell asked the seemingly innocuous question 'is that set of all sets that are not members of themselves a member of itself or not?', and noted that the answer to this query constituted a paradox since, if the answer was yes, then the answer was no (and *vice versa*).[4] Accordingly, in an attempt to avoid the paradoxes, the theory of logical types was proposed in order to delimit the extent of permissible self-reference. As Whitehead and Russell later explained,

> An analysis of the paradoxes to be avoided shows that they all result from a certain kind of vicious circle. The vicious circles in question arise from supposing that a collection of objects may contain members which can only be defined by means of the collection as a whole [...]. The principle which enables us to avoid illegitimate totalities may be stated as follows: "whatever includes *all* of a collection must not be one of the collection". (Whitehead & Russell 1925 [1910]: 37)

Although it involves an arbitrary and rather elaborate 'principle', this theory at least provided a practical way of avoiding paradoxes while developing set-theoretical concepts from the axioms of the logical system.[5]

As indicated above, the basic intention motivating the Logicist movement was to eliminate the paradoxes of set theory by deriving the whole of mathematics from a small set of logical axioms, while prohibiting specific types of self-reference. Consequently, if the logical axioms themselves were valid, and if all

4. For some background and further discussion, see Grattan-Guinness (2000: 323–326).

5. In the years following the publication of Whitehead and Russell's ideas concerning the theory of logical types, a number of criticisms of the theory emerged. For a general overview of the theory itself and the controversy surrounding it, see Copi (1971).

derived theorems were free from contradiction, then the whole of mathematics would be rendered secure. This research programme culminated in the publication of Whitehead and Russell's *Principia Mathematica* (*The Principles of Mathematics*) between the years 1910–1913, and the influence of this work upon the next generation of logicians was profound.[6] However, although the Logicist movement represented a serious response to the foundations crisis, it was not the only proposed solution, and an equally influential alternative became known as Formalism.

The Formalism movement was associated primarily with David Hilbert (1862–1943). Hilbert had established his reputation as a leading mathematician in the late 19th century by publishing a series of brilliant papers on a wide range of topics including number theory, analysis, and algebra. Consequently, by the 1890s, he was widely recognised as one of the finest mathematicians of his generation and, as a result, Göttingen University, where he was based, became a place of mathematical pilgrimage.[7] In the context of the foundations crisis, Hilbert's earlier work is of considerable interest since he was motivated to develop Formalism partly by his dissatisfaction with existing presentations of the rudiments of geometry. A general mistrust concerning existing axiomatic-deductive geometrical systems (especially Euclid's *Elements*) had enveloped the international mathematical community during the 19th century, prompted mainly by the proliferation of non-Euclidean geometries, a development that substantially undermined the role of spatial intuition as a means of validating geometrical arguments. By contrast with the classical Euclidean methodology, Hilbert was keen to remove all latent remnants of geometric intuition by exploiting the correspondence between geometry and arithmetic. He published a booklet to this effect in 1899, and, in this work, appropriately entitled *Grundlagen der Geometrie* (*The Foundations of Geometry*), Hilbert attempted to provide a viable axiomatic basis for geometry. In particular, he argued that geometric relations could be interpreted as arithmetic relations, in which case the validity of axiomatic-deductive geometrical systems could be guaranteed without the need for arguments based upon spatial-intuition, assuming (of course) that arithmetic itself was constructed upon a secure foundation. This kind of relativistic foundational approach was always destined to be unsatisfactory, since it only enabled one branch of mathematics (i.e., geometry) to be as

6. For a detailed discussion of the influence of *Principia Mathematica*, see Grattan-Guinness (2000), especially Chapter 8.

7. The best general biography of Hilbert is Reid (1996).

secure as another branch of mathematics (i.e., arithmetic). Given this conspicuous and restrictive dependency, it was perhaps inevitable that Hilbert should begin to explore the axiomatic basis of number theory itself directly, and, accordingly, he set about this task in his 1900 paper "Über den Zahlbegriff" ('Concerning the Concept of Number'). This research caused him to consider the difficulties associated with mathematical foundations in general, and he was to focus primarily upon such issues for the rest of his working life. While it is known that Hilbert had been familiar with some of the problems associated with set theory since the late 1890s, it is significant that he seems to have been galvanised into action primarily by the paradoxes that had been collected and discussed by Russell in his *Principles of Mathematics* in 1903.[8] In particular, although he agreed with Russell that the existing paradoxes undermined set theory (at least as it was currently formulated), Hilbert dismissed the assertion that they could be eliminated only by deriving mathematics from a small set of logical axioms. The Logicist research programme was misguided, Hilbert maintained, primarily because logic utilises various mathematical concepts that are later to be derived from it, thus inducing a fatal circularity:[9]

> Arithmetic is often considered to be part of logic, and the traditional fundamental logical notions are usually presupposed when it is a question of establishing a foundation for arithmetic. If we observe attentively, however, we realise that in the traditional exposition of the laws of logic certain fundamental arithmetic notions are already used, for example, the notion of set and, to some extent, also that of number. Thus we already find ourselves turning in a circle, and that is why a partly simultaneous development of the laws of logic and of arithmetic is required if paradoxes are to be avoided. (Hilbert 1967 [1904]: 131)

Hilbert's 1904 paper "Über die Grundlagen der Logik und der Arithmetik" ('Concerning the Foundations of Logic and Arithmetic'), from which this quotation is taken, is often regarded the earliest statement of his Formalist manifesto and, the paper certainly introduced several of the key ideas that were to dominate his mature foundational work.

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8. For instance, Hilbert seems to have known about Georg Cantor's own doubts concerning set theory as early as 1896. For more information, see Grattan-Guinness (2000: 117–119).

9. Whenever possible, the translations given in this paper are taken from the most widely available English versions of Hilbert's work. However, significant German vocabulary is given when it is considered desirable, and full information concerning the original German texts is supplied in the bibliographical references.

Nevertheless, during the 1910s, Hilbert was enchanted by certain aspects of *Principia Mathematica*, and started to write more enthusiastically about logic as a result. In particular, he came to admire the powerful symbolic language that Whitehead and Russell had developed in order to facilitate their logical deductions.[10] Despite his augmented appreciation, though, Hilbert continued to maintain that the Logicist movement was flawed due to the aforementioned circularity inherent in the strategy it adopted, but, during this period, he felt compelled not only to demonstrate the weaknesses of the renewed Logicist agenda, but also to invalidate the Intuitionist arguments that were being advanced by Luitzen Brouwer (1881–1966), and which were beginning to permeate the consciousness of the international mathematical community in the 1920s. Prompted, therefore, by these alternative foundational movements, Hilbert began to present, with greater clarity, his own proposal for salvaging classical mathematics from the paradoxes of set theory. As a result, in a series of publications that appeared during the years 1918–1934, and frequently aided by his assistant Paul Bernays (1888–1977), Hilbert developed his *Beweistheorie* (i.e., 'proof theory') which was intended explicitly to define his formalist position concerning the question of mathematical foundations. As Hilbert's theory evolved over the years, many of the technical details altered, but the underlying principles remained fairly constant. Therefore, rather than attempting to provide a superficial overview of the complete life-cycle of the theory, one particular mature expression of it will be considered in some detail here in order to convey Hilbert's main aims and strategies. The version of the theory discussed will be that presented in the 1927 paper "Die Grundlagen der Mathematik" ('The Foundations of Mathematics'). The exposition Hilbert offered in this paper is comparatively lucid, and reveals many of the abiding concerns that were later to be distorted and exaggerated in countless more extreme accounts.[11] Consequently, in order to recognise this distinction, throughout this paper, the adjective 'Hilbertian' will be used at times in order to distinguish Hilbert's formalism from all other kinds.

"Die Grundlagen der Mathematik" begins with a clear statement of intent that effectively constitutes a non-technical overview of the method developed in the whole paper:

10. For instance, see Hilbert (1918) in which he considers the utility of such a language when constructing axiomatic-deductive arguments.

11. For example, see the discussions of these issues in Church (1944) and Kleene (1952).

> […] I should like to eliminate once and for all the questions regarding the foundations of mathematics, in the form in which they are now posed, by turning every mathematical proposition into a formula that can be concretely exhibited and strictly derived, thus recasting mathematical definitions and inferences in such a way that they are unshakeable and yet provide an adequate picture of the whole science. (Hilbert 1967 [1927]: 464)

This passage clearly indicates that Hilbert's proof theory involved two related tasks. First, a procedure was required that enabled 'every mathematical proposition' to be converted into a 'formula', and it must then be demonstrated that the formulae obtained could be 'strictly derived'. The first task stipulates that mathematical statements must be formalised (i.e., converted into strings of precisely defined symbols) so that mathematics as a whole can be viewed simply as 'an inventory of formulae' (Hilbert 1967 [1927]: 465), and more will be said about the process of formalisation later. The second task involves the derivation of the formulae within a given system. The overriding concern here is with the nature of the proof techniques that are utilised, hence Hilbert's use of the compound noun *Beweistheorie.* Obviously, since this task involves the manipulation of strings of symbols that represent mathematical propositions, it can be said to be characterised by a certain (not necessarily vicious) circularity: proof-theoretical mathematical techniques are used to determine the viability of (suitably encoded) mathematical propositions. It is this apparent self-reference that caused the second of Hilbert's tasks to be referred to as metamathematics; that is, mathematics about mathematics.

Having delineated his basic intentions at the start of the paper, Hilbert immediately proceeded to introduce the fundamental machinery he required, and the three main components he presents are a set of logical operators, a general proof schema and a set of axioms. The logical operators are unremarkable and they include symbols for implication, conjunction, disjunction and negation as well as universal and existential operators. The methodology Hilbert proposed for the validation of mathematical theorems enabled proofs to be viewed as sequences of logical inferences which enable formulae to be derived within a given axiomatic system. It is crucial for Hilbert's project that the procedural definition of a proof is clear and unambiguous, since, as he states later in the paper, it is imperative that 'a formalised proof, like a numeral, is a concrete and surveyable object' (Hilbert 1967 [1927]: 471). It is the property of being 'surveyable' that is so important: if a proof cannot be checked in an infallible manner, then mathematics cannot be raised upon a secure proof-theoretical foundation.

The axioms, mentioned above, that Hilbert introduces in his paper are subdivided into six main categories:

> Group I: Axioms of Implication (e.g., $A \rightarrow (B \rightarrow A)$)
> Group II: Axioms of Conjunction and Disjunction (e.g., $(A \wedge B) \rightarrow A$)
> Group III: Axioms of Negation (e.g., $\neg\neg A = A$)
> Group IV: The ε-axiom: $A(a) \rightarrow A(\varepsilon(A))$
> Group V: Axioms of Equality (e.g., $a = a$)
> Group VI: Axioms of Number (e.g., $a' \neq 0$, where a' means "the number following a")

The axioms in groups I–IV are referred to as 'the logical axioms', while those in groups V–VI are called 'mathematical axioms' since they involve number-theoretic concepts. Once again, this highlights the difference between Formalism and Logicism: Hilbert assumes that certain mathematical objects, such as the numeral '1', are pre-theoretical, existing in the intuition as a thought-object (*Gedankending*), while Whitehead and Russell seek to derive even such basic objects from the principles of logic. The axiom group that seems least intuitive is Group IV since it contains the ε-axiom, which presupposes the possibility of an infinite search, and, as Hilbert was well aware, intuitions concerning infinity had often caused difficulties for mathematicians in the past. However, this axiom is required in order to enable transfinite arithmetic to be incorporated within the basic proof-theoretical framework.[12]

Armed with his set of operators, his proof schema and his axioms, Hilbert was now able to address the issue of proof construction. The central task was to construct a metamathematical proof that would demonstrate the completeness and consistency of a given axiom set. The requirement of completeness simply demands that all well-formed formulae, derived within a given system, can be shown to be either true or false. As for the requirement of consistency, from a proof-theoretical perspective, a given axiom set is considered to be consistent if no formulae taking the form '$a \neq a$' can ever be derived. In other words, a consistent axiom set will never allow contradictions to be proved. The task of proof theory in part, therefore, is to secure the axiomatic system underlying the whole of mathematics by establishing its consistency. This task, for Hilbert at least, was very different from the task of converting mathematical propositions into formal strings of symbols. As he states in the 1927 paper,

12. For Hilbert's awareness of the problems caused by the notion of infinity see Hilbert (1925). For more information about the ε-axiom, see Tomalin (2003: 54–55).

> To prove consistency we therefore need only show that $0 \neq 0$ cannot be obtained from our axioms by the rules in force as the end formula of a proof, hence that $0 \neq 0$ is not a provable formula. And this is a task that fundamentally lies within the province of intuition, just as much as does in contentual number theory the task, say, of proving the irrationality of $\sqrt{2}$ [...] (Hilbert 1967 [1927]: 471)

Statements such as this are not atypical. Hilbert repeatedly emphasised the contentual nature of the metamathematical aspects of proof-theory. For instance, in a 1922 paper, while providing an overview of proof theory, he observes

> In addition to this proper mathematics, there appears a mathematics that is to some extent new, a *metamathematics* which serves to safeguard it by protecting it from the terror of unnecessary prohibitions as well as from the difficulty of paradoxes. In this metamathematics — in contrast to the purely formal modes of inference in mathematics proper — we apply contentual inference; in particular, to the proof of the consistency of the axioms. (Hilbert 1998 [1922]: 212)

The emphasis here is absolutely clear: although formal (i.e., meaning-less) methods may be used in mathematics proper, such methods *cannot* be used during the metamathematical stage of analysis, indicating that, for Hilbert at least, proof theory was considerably more than a game involving the manipulation of meaningless symbols. Statements such as the above, with their focus upon the differences between formalisation and metamathematical analysis, should be recalled when the nature of Hilbertian Formalism is considered. A common misconception presents Hilbert as wanting to reduce the whole of mathematics to a contentless exercise in symbol manipulation that is performed in accordance with clearly defined rules. From this perspective, in the Formalist game, it is the relationship between the strings of symbols that is crucial, and the meaning either of the symbols themselves or the strings they form is deemed to be irrelevant. This misconstrual of Hilbert's programme is partly due to the practice of extracting certain of his comments from out of their immediate context. For instance, as mentioned above, part of Hilbert's contribution in his *Grundlagen der Geometrie* was to demonstrate that the meaning of the geometrical objects he considered need not be accommodated in order to analyse them coherently. In other words, statements about lines, points, and planes, could just as readily be interpreted as statements about arithmetic objects, or, as Hilbert allegedly put it "tables, chairs, and beer-mugs!" (quoted in Grattan-Guinness [2000: 208]). However, this conventional misinterpretation of Hilbert's programme is also the result of his distinction between the formalisation process and the metamathematical process being ignored. On numerous occasions, for instance, Hilbert emphasised that the task of converting mathematical propositions in a formal symbolic language was a mechanical procedure that did not rely upon considerations of meaning. In his 1927 paper, for example, he states that '[...] in my theory, contentual inference is replaced by the manipulation of signs according to rules' (Hilbert 1967 [1927]: 467). Although this observation refers only to the *pre*-metamathematical stage of analysis, when extracted out of context, comments such as this seem to suggest that it is the formal relationships between strings of symbols that matter, not the meaning of the strings themselves, even during the metamathematical manipulations of these strings. It was the (mis)perceived extremity of this emphasis on the formal properties of mathematical statements that caused Hilbert's programme to become known as Formalism. However, as demonstrated above, Hilbert was never so extreme in his own approach, and this observation has caused some commentators to recommend the avoidance of the term when discussing Hilbert, or at least to insist upon an accurate definition.[13] However, it was the caricatured version of Hilbert's original theory that was popularised throughout North America and Europe during the 1930s and 1940s, and which ultimately influenced the development of syntactic theory in the 20th century.

3. *The spread of formalism*

During the 1920s, Hilbert's work began to spread beyond the confines of pure mathematics and started to influence the research of mathematically-inclined philosophers. One of the most significant of these was Rudolph Carnap (1891–1970). Carnap was a member of the heterogeneous collection of intellectuals that came to be referred to as the Vienna Circle, other members of which included Hans Hahn (1879–1934), Otto Neurath (1882–1945), and Kurt Gödel (1906–1978).[14] In 1934 Carnap published *Logische Syntax der Sprache* (*The Logical Syntax of Language*), a work that was profoundly influenced by Hilbert's investigations into the syntax of formal mathematical systems; and the full extent of this influence is apparent when Carnap observes that "the point of view of the formal theory of languages (known as 'syntax' in our terminology) was first developed for mathematics by Hilbert" (Carnap 1937 [1934]: 1). The influence of Hilbert can be detected in Carnap's apparent belief that the syntax of formal languages can be analysed without reference to meaning, with the

13. For timely words of caution, see Ewald (1996: 1106–1107) and Mancosu (1998: 163–164).

14. Excellent recent re-evaluations of the work of the Vienna Circle include Richardson (1998) and Friedman (1999).

result that meaning-less syntactic manipulations could suffice to resolve a whole range of epistemological problems. Consequently, Carnap's intention was to provide a coherent logical system that could be used to analyse sentences in a formal language that are used to analyse sentences in a formal language. In other words, just as Hilbert had created metamathematics (mathematics about mathematics), so Carnap was keen to construct a metalanguage that could be used to define and describe any given language. However, It is crucial to note that Carnap consistently views artificial languages as forming a well-defined subset of natural languages, though he makes it clear that his intention is *not* to describe the syntax of natural language:

> In consequence of the idiosyncrasies and logically imperfect structure of the natural world-languages (such as German or Latin), the statement of their formal rules of formation and transformation would be so complicated that it would be hardly feasible in practice [...]. Owing to the deficiencies of the world-languages, the logical structure of a language of this kind will not be developed. (Carnap 1937 [1934]: 2)

Consequently, Carnap's focus is upon artificial symbolic languages which consist of formulae derived ultimately from primitive symbols by means of rules of inference in the standard Formalist manner. In addition, Carnap explicitly states that the term 'formal' implies a separation between the form and meaning of a sentence or symbol: formal languages are defined solely in terms of the syntactic structure of the formulae they produce, and the meanings of the formulae and primitive symbols are not considered. In order to emphasise this point, an example taken from natural language is discussed. Carnap considers the sentence 'Pirots karulize elatically' and states that this sentence can be parsed accurately as a Noun+Verb+Adverb sequence even though the words are all unfamiliar (Carnap 1937 [1934]: 2), thus demonstrating (or so he maintains) that sentences can be exhaustively analysed solely in terms of their formal syntactic structure even if the meaning of the individual words is not known. This type of argument, which affirms the separation of meaning and syntax, proved to be influential.

By the time Carnap published *Logische Syntax der Sprache* in 1934, Formalism had already started to suffer set-backs. For instance, in 1931 the young Kurt Gödel published an incompleteness theorem which demonstrated that, if a formal system is strong enough to prove theorems from basic arithmetic, then there will always be theorems that are true, but which cannot be proved within the system. In other words, Gödel demonstrated that the criterion of completeness was a chimera, and this proof appeared to invalidate the Formalist approach to the foundations problem. Nevertheless, despite Gödel's results, a number of mathematicians have continued to work within the general framework of proof theory and, the philosophy behind the theory has exerted a profound influence over many different disciplines, including linguistics.[15]

4. *Bloomfield and mathematics*

As indicated in Sections 2 and 3, Hilbert's contribution to the debates concerning the foundations of mathematics that raged during the early 1900s were widely interpreted as implying that the paradoxes of set theory could be obviated by means of meaning-less syntactic analysis. It is not surprising, therefore, that this type of Formalism (which was more extreme than Hilbert's own brand, as suggested previously) should provoke the interest of mathematically-inclined linguistics — an observation which naturally points towards Bloomfield, since, without doubt, Bloomfield was one of the most significant linguists to follow the progress of the foundational debates closely during the 1920s and 1930s. The extent of Bloomfield's interest in these issues can be gauged from his own publications, and the precise nature of his interest is revealing. For instance, the first of Bloomfield's papers to reveal his interest in the methodology of mathematics was "A Set of Postulates for the Science of Language", which appeared in 1926. In this short paper, Bloomfield suggested that linguists should start to use the same basic axiomatic-deductive method which had transformed the study of arithmetic and geometry in the 19th century; in other words, the very axiomatic-deductive method that Hilbert had employed so successfully in his *Grundlagen der Geometrie*. In his paper, Bloomfield uses the term 'postulates' instead of axioms, and, at the outset, he explains why a postulational approach to linguistic analysis could benefit linguistics:

> The method of postulates (that is, assumptions or axioms) and definitions is fully adequate to mathematics; as for other sciences, the more complex their subject-matter, the less amenable they are to this method, since, under it, every description or historical fact becomes the subject of a new postulate […] Nevertheless, the postulational method can further the study of language, because it forces us to state explicitly whatever we assume, to define our terms, and to decide what things may exist independently and what things are interdependent. (Bloomfield 1926: 153)

15. For various perspectives on the development of Formalism see Kreisel (1958), Detlefsen (1993), and Hintikka (1995).

As far as Bloomfield was concerned, then, the axiomatic-deductive method was of value since it could introduce new rigour into linguistics, just as it had been used during the 19th century to render mathematics more exact. The emphasis in the above passage is upon stating assumptions 'explicitly', and determining which aspects of a given theory are 'interdependent' and which can be treated 'independently'. In this way, Bloomfield appears to be recommending a reformulation of linguistics that is intended to engender greater precision. In order to clarify how this new rigorisation process for linguistics might be accomplished, Bloomfield explicitly states later in the paper that, by the judicious use of axioms, definitions, and deduction, "certain errors can be avoided or corrected by examining and formulating our (at present tacit) assumptions and defining our (often undefined) terms" (Bloomfield 1926: 153). In other words, by comparison with more fully developed formal sciences (such as mathematics), Bloomfield considered linguistics to be infested with errors that could be avoided if an axiomatic-deductive approach was adopted, and, in accordance with this proposal, he went on to introduce a set of postulates that could provide a secure foundation for the whole of linguistics. The particular postulates he introduced included definitions and assumptions such as[16]

> **Definition**: An act of speech is an utterance.
> **Assumption**: Within certain communities successive utterances are alike or partly alike.

It is significant that, although Bloomfield recommended the use of a basic postulational methodology because it could make linguistics more precise, as these examples indicate, he did not attempt to introduce a formal symbolic language that would enable the axioms of linguistics to be converted into unambiguous sequences of precisely defined symbols, and, indeed, this stage in the process of formalising linguistic theory was not attempted until the 1940s and 1950s.[17]

The text that Bloomfield cites as the main source of his information concerning the axiomatic-deductive method in his 1926 paper is John Young's (1879–1932) *Lectures on the Fundamental Concepts of Algebra and Geometry*. This text was published in 1911, but it was based upon a series of lectures that had been delivered at the University of Illinois in 1909 (a year before Bloomfield

16. These particular examples can be found in Bloomfield (1926: 154).

17. For more information concerning this crucial development, see Tomalin (2003).

joined the faculty as Professor of German). Consequently, given this date, Young was not able to consider the implications of *Principia Mathematica*, since Russell and Whitehead's work would not be published for another two years, but he did provide a thorough introduction to a wide range of topics including Euclidean and non-Euclidean geometry, logic, set theory, number theory and many other subjects. He openly declared that his primary aim was to provide "an elementary account of the logical foundations of algebra and geometry" (Young 1911: v), a remark that perhaps suggests some kind of sympathy with the Logicist program, and he repeatedly stresses the fact that mathematical propositions are "logically connected" (Young 1911: 1). However, he also admits that, throughout the book, he has adopted a "formal point of view" (Young 1911: v), and certainly his knowledge of Hilbert's proto-formalist work is revealed in Chapters 13 and 14 when he discusses Hilbert's axiomatic approach to geometry in some detail. In this context, it is striking that, by 1911, the task of providing a logical foundation for specific branches of mathematics was already closely associated with the nascent Formalist programme. It should be noted that Young returned to some of the topics he had presented in his 1911 monograph when he came to write *Projective Geometry* with Oswald Veblen in 1918, and Bloomfield was clearly familiar with this text too since he cites it in a 1935 article (Bloomfield 1935: 505n.6).

Given Bloomfield's knowledge of Young's texts, it is reasonable to suppose that, by 1926 at least, he was broadly familiar with the main branches of contemporary mathematics discussed by Young. In addition to this direct mathematical inspiration, though, Bloomfield's interest in axiomatic approaches was also stimulated by the work of the psychologist Albert Paul Weiss (1879–1931). In particular, as is often acknowledged, Weiss seems to have convinced Bloomfield that mathematical procedures could be usefully employed in the mind-based sciences. For instance, in a 1925 paper, Weiss had proposed a set of postulates for psychology, and Bloomfield acknowledged that this attempt at axiomatisation had partly inspired his own proposal for the reform of the methodology of linguistics.[18] Whatever the precise nature of this influence, it is clear that, by 1926, Bloomfield was intrigued by the possibility of using mathematical techniques to facilitate the analysis of cognitive phenomena such as natural language. However, far from being a superficial ephemeral fad,

18. For more information concerning the influence of Weiss upon Bloomfield's work, see Belyi (1967). Bloomfield cites Weiss (1925) explicitly in his own paper (see Bloomfield 1926: 153).

his interest in this topic seems to have increased during the years following 1926. For instance, there are various comments concerning the relationship between language and mathematics in his most famous and influential book, *Language*, which appeared in 1933. To take one example, early on in the text he refers to mathematics as "the ideal use of language" (Bloomfield 1933: 29), and later declares (rather provocatively) that one of the tasks confronting the practising linguist is to "reveal the verbal character of mathematics" (Bloomfield 1933: 507). Although Bloomfield does not state explicitly in *Language* how such a task could best be accomplished, this remark certainly suggests that, by the early 1930s, Bloomfield had begun to consider the possibility of using techniques from linguistics to analyse mathematics, rather than merely using mathematical procedures to explore fundamental properties of language.

Although, as indicated above, Bloomfield's initial knowledge of contemporary mathematics seems to have been derived primarily from secondary sources such as Young's textbook summaries and Weiss's papers, by the 1930s there is no doubt that he was reading primary source material that considered the implications of Hilbert's Formalist agenda directly. In particular, his understanding of Formalism was influenced by the work of the Vienna Circle, and the full extent of his familiarity with this work is revealed in his 1936 paper "Language or Ideas?", since, in this article, Bloomfield explicitly cites five works written by Neurath and six by Carnap, including the latter's *Logische Syntax der Sprache*. While there are several reasons for exploring the influence of Neurath upon Bloomfield's thought and work, it is Carnap's influence that will be assessed later in this paper — in particular Bloomfield's acquaintance with Carnap's provocative ideas concerning logical syntax. At this point, though, it is worth recalling that Bloomfield's bilingualism enabled him to access these publications in the original German; and this is significant since no English translation of Carnap's *Logische Syntax der Sprache* had appeared by 1936, so the text was *only* accessible in German when Bloomfield first encountered it. It should be noted, therefore, that Bloomfield acquired a detailed knowledge of Carnap's work several years before that work began to generate wide-spread interest in North America.[19]

In summary, then, Bloomfield's knowledge of the various foundational movements seems to have been derived not only from secondary sources, such

19. The logician and philosopher Willard Van Orman Quine (1908–2000) was one of the conduits through which Carnap's work on formal syntax passed into North America. For details, see Quine (1985: 86ff.).

as Young's publications and Weiss's work, but also from primary sources, such as the work of the Vienna Circle, which developed and extended techniques and philosophical approaches associated with Russell, Hilbert, and other mathematicians actively involved in the foundations debate. Consequently, by the mid 1930s, Bloomfield would have been familiar with the Logicist, Formalist, and Intuitionist movements as competing proposed solutions to the foundations crisis. However, as implied in the introduction, Bloomfield himself was directly concerned with the implications of the foundations crisis and, significantly, he came to believe that many of the disagreements could be resolved if the formal symbolic languages used to construct mathematical discourses were viewed from the perspective of linguistic theory. It is now necessary to explore this fascinating but neglected aspect of Bloomfield's work in greater depth.

5. *Linguistic Foundations*

In 1935 Bloomfield published an article entitled "Linguistic Aspects of Science", which appeared, significantly, in the journal *Philosophy of Science*. The purpose of this article was to consider the language of science (i.e., mathematics) from the viewpoint of linguistic theory. At the outset, Bloomfield identifies two stages in the process of scientific activity which he characterises as follows:

> The linguist naturally divides scientific activity into two phases: the scientist performs "handling" actions (observation, collecting of specimens, experiment) and utters speech (report, classification, hypothesis, prediction). The speech-forms which the scientist utters are peculiar both in their form and in their effect upon hearers. (Bloomfield 1935: 499)

He later clarifies the nature of this peculiarity by observing that the language of mathematics can only be understood after "severe supplementary training", and that utterances in such a language have the curious effect of causing the hearers to "respond uniformly and in a predictable way" (Bloomfield 1935: 499). Clearly, therefore, the language of science differs significantly from natural language, and the speech-forms of scientific language appear to constitute "a highly specialized linguistic phenomenon" (Bloomfield 1935: 500). It is at this point that Bloomfield's ambitious agenda starts to reveal itself. The following passage is crucial:

> To describe and evaluate this phenomenon is first and foremost a problem for linguistics. The linguist may fail to go very far towards the solution of this problem, especially if he lacks competence in the branches of science other than his own. It is with the greatest diffidence that the present writer dares to

> touch upon it. But it is the linguist and only the linguist who can take the first steps towards its solution; to attack this problem without competence in linguistics is to court disaster. The endless confusion of what is written about the foundations of science or of mathematics is due very largely to the authors' lack of linguistic information. (Bloomfield 1935: 500)

The central idea here is transparent: the complex acrimonious arguments that had come to characterise the foundations crisis debates in the 1920s and 1930s could be resolved if the participants were able to view the problem from a linguistic perspective. Indeed, 'the linguist and only the linguist' can intervene in order to resolve the disputes. Obviously, this is a bold and startling claim, hence Bloomfield self-confessed 'diffidence', but the proposal is serious nonetheless. It is crucial, though, to attend to Bloomfield's language here. While he is willing to recognise mathematical discourse as a particular kind of language use, it is not the case that a sharp distinction is being maintained between mathematics and linguistics. Indeed, (as the above passage implies) Bloomfield's interest in mathematics was always mediated by his abiding preoccupation with linguistics, and this observation is central to the whole of the following discussion. Quite simply, whenever Bloomfield discussed mathematics (particularly the foundations crisis) he was also, of course, discussing linguistics; these interests were inter-connected, if not identical.

Since (infuriatingly) Bloomfield does not cite specific sources in his 1935 discussion, the precise causes of his dissatisfaction with existing proposed solutions to the foundations crisis can only be guessed. It should be recalled, though, that, as mentioned previously, introductory texts such as Young's *Lectures on the Fundamental Concepts of Algebra and Geometry*, pre-dated the main foundational debates, and consequently it did not contain detailed discussions of the central disagreements, suggesting that Bloomfield acquired his knowledge of these debates from primary sources. As mentioned in Section 4, some foundational issues were addressed in certain works produced by the members of the Vienna Circle, and Bloomfield certainly knew some of these publications. However, questions concerning specifics inevitably remain. Had Bloomfield read the main publications associated with Hilbert or Russell? If so, which publications had he read? Certainly, references in Carnap's *Logische Syntax der Sprache* (which Bloomfield *had* read) would have provided him with information concerning Hilbert's most significant pre-1934 articles, and, by the mid 1930s, Whitehead and Russell's work, especially *Principia Mathematica*, had already become a standard starting point for most contemporary work in symbolic logic, and was therefore hardly an obscure and unobtainable text.

Whatever the precise sources of his knowledge, though, it is clear that Bloomfield was well-aware of the fact that the paradoxes which had provoked the foundations crisis in the early decades of the 20th century were associated with specific kinds of self-reference.[20] Indeed, it is this aspect of the whole foundations debate that seems to have intrigued Bloomfield most, since, as he was keen to demonstrate, the basic problem of self-reference can be approached from a linguistic perspective. His particular concerns are manifest in the following footnote in which he reflects upon Kurt Grelling's (1886–1942) well-known 'heterological' paradox.[21]

> An adjective which describes itself is *autological* (e.g., *short* is autological, since the adjective *short* is actually a short word). An adjective which is not autological is *heterological* (e.g., *long* is not a long word). Is the adjective *heterological* heterological? If it is heterological, it describes itself and is therefore autological. If it autological, it does not describe itself and is therefore heterological. (Bloomfield 1935: 500n.3)

Before continuing with the footnote it is worth pausing to clarify the discussion. As should be apparent, Grelling's 'heterological' paradox is closely related to Russell's paradox (discussed in Section 2 above), the main difference being that, rather than outlining the problem in the context of set theory, Grelling illustrated the complexities of self-reference by constructing an example using natural language, thus enabling the issues involved to be viewed from a different stand-point. No doubt, this emphasis on natural language is what enticed Bloomfield, prompting him to focus upon Grelling's paradox. However, a mere restatement of a known difficulty is one thing, but a specific proposal for its resolution is quite another, yet, as the footnote continues, this is precisely what Bloomfield attempts:

> The fallacy is due to misuse of linguistic terms: the phrase "an adjective which describes itself" makes no sense in any usable terminology of linguistics; the example of *short* illustrates a situation which could be described only in a different discourse. E.g.: We may set up, without very rigid boundaries, as to meaning, various classes of adjectives. An adjective which describes a phonetic

20. This fact alone suggests some kind of familiarity with the work of Russell (perhaps his accessible *The Principles of Mathematics* of 1903?), since Russell had been the most assiduous paradox collector, and, as mentioned in Section 2, the theory of logical types had been designed primarily to obviate the kind of set-theoretical self-reference that engendered the paradoxes.

21. A clear presentation of Grelling's 'heterological' paradox can be found in Grelling (1936).

> feature of words is *morphonymic* (e.g., *short*, *long*, *monosyllabic*). A morphonymic adjective which describes a phonetic features of itself is *autological*. A morphonymic adjective which is not autological is *heterological*. The adjectives *autological* and *heterological* designate meanings of adjectives and not phonetic features; hence they are not morphonymic. — Contrast the following sensible discourse: A *hakab* is a word that ends in a bilabial stop (p, b). A word that is not a hakab is a *cowp*. The words *hakab* and *cowp* are hakabs. (Bloomfield 1935: 500n.2)

Although this discussion is necessarily sketchy, constituting as it does a brief footnote, the basic outline of Bloomfield's proposal is clear. His basic intention was to avoid the problem of direct self-reference by reanalysing the categorical allocation of the words involved. In this simple example, by introducing the notion of morphonymic adjectives, Bloomfield suggests that linguistic categories can be redefined in order to exclude the type of direct self-reference that engenders paradox, and it is important to note that, for Bloomfield, this was specifically a linguistic solution for a pervasive problem which happens to manifest itself in particular mathematical contexts.

Unfortunately, in his 1935 article, Bloomfield did not return to the question of a linguistic solution to the problems of self-reference that had provoked the foundations crisis. However, he did not leave his ideas in the inchoate state outlined above; on the contrary, he developed them extensively during the following years. In 1937, for instance, Bloomfield submitted a 300 page manuscript to the Committee on Research of the Linguistic Society, and this work apparently contained a more complete presentation of some of the issues addressed in the 1935 article. The proposed monograph was called *The Language of Science* and it constituted an elaborate attempt to analyse large portions of modern mathematics from a linguistic perspective.[22] Faced with this atypical document, and with becoming humility, the linguists on the committee considered themselves to be unequal to the task of assessing the value of the manuscript, so it was passed on to several professional mathematicians, including the prominent formalist Haskell Curry (1900–1982). Since the manuscript contained a few mathematical errors, Curry advised against publication, but, despite his technical reservations, he was impressed by the scope and ambition of Bloomfield's approach, and he offered general advice as to how the manuscript could be improved. On receiving Curry's comments, Bloomfield replied:

22. The remaining fragments of this manuscript can be found in Bloomfield (1970: 333–338).

> Your report […] reached me yesterday, and I am very much indebted to you for your careful reading and comment. Criticism and correction from someone interested in the subject and familiar with mathematics is something I very much wanted and seemed unable to get. Assuming that the main contentions of the MS are correct, then, in order to be of use, it would still have to be intelligible and interesting to linguists and, even though contradicting the beliefs of mathematicians, it would have to be free from mathematical errors. Whether I can give it these two qualities seems extremely doubtful […]. (Bloomfield 1970 [1937]: 334)

The language used here is revealing. For instance, when Bloomfield states that he was grateful to have found someone (i.e., Curry) who is both 'interested in the subject' (i.e., foundational considerations), and 'familiar with mathematics' (i.e., a professional mathematician) he implies that his consideration of the foundations crisis is a *meta*mathematical exploration. In other words, Bloomfield knew very well that, by outlining a linguistics-based solution of certain foundational problems, he was not reinventing himself as a pure mathematician. Indeed, he appears emphatically to distance himself from 'mathematicians' as an identifiable group when he claims that his manuscript could be characterised as 'contradicting the beliefs of mathematicians'. This, in turn, explains his expressed doubt concerning the task of producing a manuscript that would convince linguists and mathematicians alike. Yet he clearly understood that it was necessary for him to write for both audiences: if he were successfully to demonstrate that (meta)mathematical foundational problems could be solved by the adroit deployment of analytical techniques associated with linguistics, then it was necessary to convince both mathematicians and linguists. Ultimately, though, his doubts seem to have predominated, since, at some stage, he decided that he would not be able adequately to revise the text; he did not resubmit a revised version of the manuscript, and, instead, he used the reverse sides of the pages as scrap paper. Consequently, only a few fragments now survive, but these fragments are enough to reveal the ambitious nature of the work. Thankfully, Bloomfield's basic motivation for writing the text is clearly articulated in one of the surviving passages. After observing that no 'student of human speech' has ever made an extensive study of mathematics, he continues

> Having made the attempt, the present writer has reached the conclusion that such a study, apart from its linguistic interest, leads to the solution of certain problems that have baffled non-linguistic attack — the problems which concern

> the foundations of mathematics. If this conclusion is justified, the following pages should be of wider than linguistic interest. (Bloomfield 1970 [1937]: 335)

This is an extraordinary statement. As indicated above, in his 1935 article, Bloomfield had observed that certain problems of self-reference within mathematics could be avoided if a linguistic approach were adopted. In the light of this remark it becomes apparent that the now lost 1937 manuscript constituted an extended attempt actually to provide a linguistic-based solution to the foundations crisis. Although it is no longer possible to reconstruct Bloomfield's argument in exhaustive detail, some kind of revivification can be accomplished. For instance, a partial chapter list has survived, and, consequently, it is known that the manuscript contained sections dealing with such topics as 'infinite classes', 'recursion', 'logical vocabulary and syntax', and other subjects that were active areas of contemporaneous mathematical research. The reference to a chapter concerning 'infinite classes' is of especial interest since Bloomfield delivered an (unpublished) paper on this topic to the Annual Meeting of the Linguistic Society in 1936, and it was clearly a subject that preoccupied him.[23] Given his familiarity with the foundations debates, this preoccupation is not surprising since, as mentioned in Section 2, many of the paradoxes of mathematics were understood to be associated with the notion of an infinite set, and, therefore, any valid solution to the foundations crisis must either reconsider the implications of such sets, or else must reformulate this aspect of set theory in such a way that such sets were precluded.[24] Indeed, the extant manuscript fragments suggest that, in his 1937 text, Bloomfield focussed primarily upon the task of *naming* infinite sets. For instance, he considers various methods that can be used to define irrational numbers, and criticises the use of summation series

> The members of the summation series can be obtained one by one, but we have no finite formula for the direct naming or recognition of these members. To prescribe the naming, in this form, of an irrational number, is to insist that our hearers *complete the recitation* of an infinite class of speech-forms. This fallacy is still current among mathematicians; we shall return to it in Chapter 22. (Bloomfield 1970 [1937]: 337)

23. This paper is mentioned briefly in Bloomfield (1970: 333).

24. The redefinition of the notion of a set was one common response to the work of Cantor, Russell and Whitehead during the first half of the 20th century. For instance, in the 1920s Stanisław Leśniewski (1886–1939) devised a nominalistic version of set theory, called 'mereology', which avoided some of the problems of self-reference. For more information, see Luschei (1962).

Unfortunately, Chapter 22 no longer exists, so Bloomfield's discussion of this perceived fallacy cannot be completely revived. However, his analysis of the use of limits as a means of defining irrational numbers has survived, as has a short section of his discussion of the Φ class. Bloomfield defines the Φ class using linguistic notions associated with naming. He defined three activities:

(1) Say *decimal point*;

(2) recite any sequence of digits or none;

(3) name a second sequence of digits, not all zeros, as a circulating sequence[25]

and concludes by asserting that "any speech-form of the shape (1)–(2)–(3) or of the shape (1)–(3) is a member of the class Φ" (Bloomfield 1937: 338). With this definition in place, Bloomfield proceeds to consider the implications of naming infinite sets:

> Given the class Φ, together with a formula for well-ordering it [...] we can define, *as functions of* Φ, infinite classes of speech-forms of the type **N**. For instance, we add 1 to the *k*th digit of the *k*th **R** [MT: **R**s are defined earlier as 'thing-nouns'], except that when the sum is 10 we replace it by 1. We thus obtain the infinite class of speech-forms $\mathbf{N}^1$, the non-circulating decimals whose first ten digits are .5471111117. This formula for naming $\mathbf{N}^1$, is stated in terms of Φ and its well-ordering: a digit of $\mathbf{N}^1$ can be named only if one first names *k* digits of the *k*th **R**. Hence to calculate and recite digits of $\mathbf{N}^1$ to the end of one's patience is not to name a number: it is only the formula $\mathbf{N}^1$, interpreted as above, which names a number. (Bloomfield 1970 [1937]: 338)

Although this remnant of a larger discussion is opaque in places, the basic thrust of the passage is clear: the act of enumerating the members of an infinite class (i.e., an infinite set) is not the same as naming the set itself, and, presumably, in the remaining chapters of the manuscript, Bloomfield sought to demonstrate that the paradoxes of set theory could be obviated if this kind of linguistic distinction were systematically observed.

When the remaining manuscript fragments were collected by Hockett in 1970 for inclusion in *A Leonard Bloomfield Anthology* (which he was then editing), he commented concerning the destruction of the manuscript:

> I cannot refrain from expressing my regrets at the loss. Had he lived to rework the topic, benefiting from Professor Curry's suggestions (even if not accepting them all), some of his successors, who have concerned themselves with the

25. The term 'circulating sequence' means that the sequence of digits repeats itself.

> inter-relations of language and mathematics, might have been helped to avoid various stupid errors. (Bloomfield 1970: 334)

Unfortunately, Hockett does not name the linguists who have been guilty of making 'stupid errors', nor does he indicate the particular mistakes that he has in mind. It is likely, though, that this rebarbative comment was directed towards certain prominent syntacticians of the 1940s and 1950s, who were preoccupied with the task of adapting techniques from mathematics and exploiting them for the purposes of linguistic analysis. The partly conjectural discussion of Bloomfield's lost work offered above is necessarily based only on glimpses, but such glimpses hint at the full extent of Bloomfield's ambition, and it is particularly tantalising that several of the techniques, such as recursive function theory, which Bloomfield considered extensively in the lost manuscript, were later incorporated into syntactic theory in the 1950s. These fascinating issues are briefly considered in Section 7.

6. *Form and meaning*

In the foregoing sections, Bloomfield's knowledge of contemporaneous mathematics has been discussed, and his own linguistics-based proposals for the solution of the foundations crisis in mathematics have been partially reconstructed. However, the question remains: did these interests have any consequences for Bloomfield's more mainstream linguistics work? A comprehensive answer to this question is beyond the scope of this article, yet a possible connection between his mathematical interests and his linguistic research can be approach via a consideration of the role of meaning in the type of procedural methodologies outlined in a number of his publications. This discussion should be prefixed with the observation that, while the status of form and meaning in Bloomfield's linguistic work has been assessed many times over the years, it has never been extensively considered with reference to Formalism.[26]

As indicated in Section 3, Bloomfield pursued his interest in the relationship between mathematics and linguistics during the 1930s, and he presented an extended consideration of this general topic in a long essay which he contributed to the *International Encyclopedia of Unified Science* in 1939. The encyclopaedia was a forum for assessing the methodology of scientific research, and

26. For example, see the discussions of meaning in Fries (1954) and Koerner (2002, esp. Chapter 5). A broad consideration of these issues can be found in Matthews (1993: 118–122). The intensity of Bloomfield's distaste for semantics has been questioned from time to time. For some discussion of this position, see Murray (1994: 130–132).

many of the contributors were associated with the type of logical empiricism broadly espoused by members of the Vienna Circle. In particular, Carnap was on the board of editors that read and assessed the contributions, which included Bloomfield's article. This short monograph, "Linguistic Aspects of Science", was based on the 1935 article discussed in Section 4, and this revised version of the paper was intended to serve several purposes. For instance, it summarised various ideas and techniques employed in linguistic research in the early decades of the 20th century, and, in this respect, the bulk of the paper can be viewed in part as a brief informal summary of Bloomfield's 1933 book *Language*. However, in addition, Bloomfield reconsiders the nature of the relationship between linguistics and mathematics, or, more precisely, as he puts it himself, "the relation of linguistics to logic and mathematics" (Bloomfield 1939: 273). Given Bloomfield's knowledge of the foundations debate, this statement should be carefully assessed, since it implies that, for Bloomfield, mathematics and logic were separate fields of research. It is certainly possible that this observation is largely innocuous, yet by stating his interest in this way, Bloomfield is surely consciously avoiding the extreme Logicist viewpoint (associated with Russell and Whitehead). Whatever the exact purport of Bloomfield's remark, having stated his basic intention in this manner, he goes on to consider various aspects of the broad topic he has broached. For example, he declares that "logic is a branch of science closely related to linguistics, since it observes how people conduct a certain type of discourse" (Bloomfield 1939: 273–274), and this observation leads him to suggest in turn that logical arguments can be analysed specifically as linguistic discourses of a particular kind. Such statements certainly imply a close correspondence between linguistics and logic, and they reinforce that suggestion (discussed briefly above) that, during the early 1930s, Bloomfield had started to think of mathematics as a highly specialised form of language that could be amenable to linguistic analysis.

Clearly, then, Bloomfield was fascinated by the relationship between logic and natural language, yet the Formalist slant of Bloomfield's understanding of these issues is apparent when he later enthusiastically accepts a more extreme formalist emphasis on meaning-less syntactic manipulations. For example, Bloomfield makes a clear distinction between formal and informal scientific discourse, describing the former as a manner of communication that "uses a rigidly limited vocabulary and syntax and moves from sentence to sentence only within the range of conventional rules" (Bloomfield 1939: 261), and he later argues that, in considering the 'characters' (i.e., symbols) used in logical and mathematical discourse, he has not "left the domain of language" since

> In general, to be sure, the separate characters have been agreed upon as substitutes for specific words or phrases. In many cases, however, we manage best by ignoring the values and confining ourselves to the manipulation of the written symbols; systems of symbolic logic, especially, may be viewed, in a formal way, as systems of marks and conventions for the arrangement of these marks […] our formal systems serve merely as written or mechanical mediations between utterances of language. (Bloomfield 1939: 262)

This passage, which appears to endorse a conspicuously Formalist position, suggests that Bloomfield was persuaded that this general approach to mathematical enquiry was valid. At the very least, the above passage implies that Bloomfield accepted the formalist dictum that 'we manage best' (to use his own words) if we focus on syntactic manipulations and ignore considerations of meaning. The implications of this statement are considerable and have never been adequately discussed. In essence, the comments cited above suggest that Bloomfield's linguistic research was indeed influenced (to some extent) by Formalism during the 1930s, and the effects of this influence are, perhaps, apparent in his work. For instance, to consider one example, it is well-known that Bloomfield repeatedly expressed scepticism concerning the validity of meaning in linguistic theory. A standard expression of this mistrust, taken from *Language*, runs as follows: "The statement of meaning is […] the weak point in language-study, and will remain so until human knowledge advances very far beyond its present state" (Bloomfield 1933: 140). In the past, attempts to account for this scepticism have focussed upon ideas concerning syntax and semantics within linguistics and the relationship between linguistics and psychology. While there is no doubt that linguistics and psychology were both responsible for determining the direction of Bloomfield's thought in many ways, it is certainly possible that some of his ideas concerning the role of meaning in linguistic theory were directly influenced by his knowledge of Formalism (and/or *vice versa*). While it would be needlessly excessive to claim that Bloomfield mistrusted linguistic meaning *solely* because he had considerable sympathy with Formalism (as initially advocated by Hilbert, and later developed by Carnap and others in the 1930s) it certainly could have been the case that his understanding of the foundational debates within mathematics confirmed his initial misgivings about semantics in linguistic research, causing him to marginalise the role of meaning in his own work, thus unwittingly paving the way for the type of 'formal' syntactic theories that began to emerge in the late 1940s and early 1950s. It is worth noting, though, that the full complexity of Bloomfield's attitude towards the role of meaning in linguistic theory is comparable to Hilbert's attitude towards the role of meaning in metamathematical analysis. For instance, as mentioned in Section 2, Hilbert had refused to adopt a hard-line Formalist position, arguing instead that considerations of meaning were necessarily involved in the task of metamathematical manipulation, and, in a similar fashion, Bloomfield seems consistently to have resisted an extreme Formalist stance. To take just one example, writing in 1943, he remarked that

> In language, forms cannot be separated from their meanings. It would be uninteresting and perhaps not very profitable to study the mere sound of a language without any consideration of meaning. (Bloomfield 1943: 102)

While this is not the place exhaustively to elucidate Bloomfield's various discussions of the role of meaning in linguistic theory, it is possible to posit a correspondence between Hilbert's and Bloomfield's thinking in this regard. Clearly, there are associations here that have yet to be fully revealed.

7. *Conclusion*

The main emphasis in the paper has been upon Bloomfield's interest in mathematics, a topic that has been neglected in the past. As indicated in the foregoing discussion, such a study is of intrinsic significance since it prompts a re-evaluation of the intellectual life and work of one of the leading linguists of the first half of the 20th century. For instance, it is certainly the case that an awareness of Bloomfield's fascination with the foundations crisis, and an appreciation of his active participation in attempts to resolve the crisis, reveals more clearly the full extent of his intellectual range. In addition, with the insights garnered by this reclaimed understanding of his work, Bloomfield's own linguistic research can be reconsidered essentially from a mathematical vantage point, with the result that, certain characteristic features and preoccupations that occur frequently in his writings, and which have been considered many times from various linguistic perspectives, can be reassessed with reference to developments in contemporaneous mathematics. For example, the specific theme considered in this paper, namely Bloomfield's complex attitude towards the role of meaning in linguistic theory, can be re-evaluated with reference to Formalism, indicating that Bloomfield's pronouncements concerning meaning possibly reveal a more profound awareness of contemporaneous scientific culture than has previously been recognised.[27]

As suggested above, such investigations are worthwhile since they cause us to reacquaint ourselves with Bloomfield and his work. However, the consequences of these associations impinge upon syntactic theory in general, and the ramifications are wide-spread. For example, it is well-known that, during the 1940s and 1950s a whole generation of linguists, which included Zellig S. Harris (1909–1992), Charles F. Hockett (1916–2002), F.W. Harwood (dates unknown; cf. Harwood 1955), Yehoshua Bar-Hillel (1915–1975), and Noam Chomsky (b.1928), began to adapt techniques from logic and mathematics in order to render syntactic theory more rigorous. In the light of the above discussion, it is of particular interest that many of the techniques that were incorporated into syntactic theory by the post-Bloomfieldians and the proto-generativists, were associated with Hilbertian Formalism. While this is not the place for a full discussion of these issues, two examples can be briefly considered.[28] In a 1953 paper, for instance, Bar-Hillel proposed that recursive definitions could be helpfully employed in syntactic theory, since such definitions would enable complex and compound sentences to be parsed in a recursive fashion (see Bar-Hillel 1953), and the use of recursive definitions had been popularised by the development of recursive function theory in the 1930s and 1940s, which in turn had developed out of the use of such functions in Hilbertian Formalism. To take just one other example, it is clear that the various kinds of 'transformation' rules that were proposed by several linguists (including Harris, Bar-Hillel, Harwood and Chomsky) in the 1950s were associated with and, to some extent derived from, the transformation rules that Carnap had outlined in *Logische Syntax der Sprache*, a text which, as mentioned in Section 3, was directly inspired by Hilbert's attempts to construct a metalanguage for scientific discourse. Given such mathematico-linguistic associations, which eventually culminated in the construction of Transformational Generative Grammar, it is certainly stimulating to note that Bloomfield was preoccupied

27. For a detailed consideration of the influence of mathematics upon the development of syntactic theory in the first half of the 20th century, see Tomalin (2003).

28. Perhaps it should be re-emphasised, at this point, that the concern in this paper is primarily with the possible influence of Formalism upon Bloomfield's intellectual development. Obviously, there is much that could be said concerning the influence of Logicism (and formal logic in general) upon Bloomfield's work, and, though studies such as Fought (1999) have begun to address some of these issues, there are many aspects of this influence that remain undiscussed.

with the implications of similar techniques and methodologies over twenty years before they became a central preoccupation for syntacticians. This observation becomes especially pertinent when it is recalled that many researchers working in the 1940s and 1950s stated specifically that they identified a similarity, or at least a sympathy, between the techniques they adapted from certain branches of mathematics, and the kind of discovery procedures that Bloomfield and his immediate successors had advocated. For example, writing in 1964, Bar-Hillel recalled

> I think that the only work by a modern professional linguist I had studied in some depth before these talks [i.e., talks with Harris in the early 1950s] was Bloomfield's little contribution to the Encyclopedia of Unified Science, published in 1939. This booklet showed a surprising convergence between ways of thinking of at least certain circles of American linguists and those of say, Carnap, and I made a mental note to pursue this issues further sometime. But only in 1951 did I find the time to do so. (Bar-Hillel 1964: 4)

Later still, Harris commented upon the associations between the foundational debates and the linguistic methods of Bloomfield (and Sapir) which he had observed in the 1950s:

> The expectation of useful mathematical description of the data of language stems from developments in logic and the foundations of mathematics during the first half of the twentieth century. One main source was the growth of syntactic methods to analyse the structure of formulas [...]. In linguistics, the 'distributional' (combinatorial) methods of Edward Sapir and Leonard Bloomfield were hospitable to this approach. (Harris 1991: 145)

These are just two examples (there are many others) and, removed from the mathematical context of the time, such perceived associations simply appear to be unaccountable curiosities: surely it can be little more than a remarkable coincidence that Bloomfield and his immediate successors proposed procedures for the analysis of language that proved to be compatible with techniques derived by a later generation of linguistics from specific branches of mathematics? However, as the main sections of this paper demonstrate, this perceived compatibility can be viewed as much more than mere coincidence, and though the full consequences of the association between Bloomfield's work and developments in contemporaneous mathematics have yet to be considered in exhaustive detail, it is hoped that this paper at least constitutes an initial exploration of this intriguing connection.

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SUMMARY

This paper considers various aspects of Leonard Bloomfield's (1887–1949) interest in contemporaneous mathematics. Specifically, some of the sources from which he obtained his mathematical knowledge are discussed, as are his own proposals for a linguistics-based solution to the foundations crisis which preoccupied leading mathematicians during the first half of the 20th century. In addition, his attitude towards the role of meaning in linguistic theory is reassessed in the light of his knowledge of Hilbertian Formalism.

RÉSUMÉ

Cet article discute divers aspects de l'intérêt que portait Leonard Bloomfield (1887–1949) pour les mathématiques de son époque. Plus précisément, on y traitera de certaines des sources d'où il obtint ses connaissances en mathématiques. On y traitera également de ses propres propositions (tirées de la linguistique) visant à résoudre la crise des fondations qui préoccupait nombre d'illustres mathématiciens au cours de la première moitié du XXe siècle. De plus, on reverra son point de vue quant au rôle du sens en linguistique théorique à la lumière de sa connaissance du formalisme d'Hilbert.

ZUSAMMENFASSUNG

Der vorliegende Beitrag untersucht verschiedenen Aspeckte von Leonard Bloomfields (1887–1949) Interesse an der zeitgenössischen Mathematik. Insbesonders werden einige Quellen diskutiert, von denen er mathematische Kenntnisse erworben hatte, aber auch seine eigenen Vorschläge zur einer auf der Linguistik basierenden Lösung der Krise der Grundlagen der Mathematik, die führende Wissenschaftler in der ersten Hälfte des 20. Jahrhunderts vornehmlich beschäftigte. Darüber hinaus wird Bloomfields Haltung gegenüber der Rolle der Bedeutung in der Sprachtheorie im Lichte seiner Kenntnis des hilbertischen Formalismus neu bewertet.

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