1 *Introduction*

The emergence of Transformational Generative Grammar (TGG) in the 1950s is an event in the history of linguistics that has been recounted many times, in many different ways, by many different people, and since this book is primarily concerned with the development of TGG, some sort of *apologia* is required in order to justify retelling the same story yet again. Accordingly, it is hoped that this introduction will provide the requisite justification, and, in summary explanation, it can be stated at the outset that the main motivation for the particular narration offered here is dissatisfaction – specifically, dissatisfaction stemming from the conviction that none of the existing versions of the TGG narrative provide sufficient information concerning the influence of contemporaneous advances in the formal sciences upon the development of linguistic theory in the twentieth century. If indeed it is the case that this aspect of TGG history has been neglected in the past, then this neglect is certainly surprising, since the earliest proponents of TGG have never disguised the fact that the theory derived considerable inspiration from the formal sciences. For instance, in 1995 (to consider just one example) Chomsky stated explicitly that '[g]enerative grammar can be regarded as a kind of confluence of long-forgotten concerns of the study of language and mind, and new understanding provided by the formal sciences' (Chomsky 1995: 4), and the scientific nature of TGG itself has often been noted over the years. Indeed, it has sometimes been claimed that TGG was a superior linguistic theory primarily because it was more 'scientific' (whatever that means) than the syntactic theories that preceded it. For instance, this was the basic claim made by Robert Lees in his influential 1957 review of *Syntactic Structures*, the text that was partly responsible for inculcating a widespread interest in TGG in the late 1950s. Although Lees' review is well known, it is worth quoting the relevant passage in full:

> Chomsky's book on syntactic structures is one of the first serious attempts on the part of a linguist to construct within the tradition of scientific theory-construction a comprehensive theory of language which may be understood in the same sense that a chemical, biological theory is ordinarily understood

> by experts in those fields. It is not a mere reorganisation of the data into a new kind of library catalogue, nor another speculative philosophy about the nature of Man and Language, but rather a rigorous explication of our intuitions about our language in terms of an overt axiom system, the theorems derivable from it, explicit results which may be compared with new data and other intuitions, all based plainly on an overt theory of the internal structure of languages; and it may well provide an opportunity for the application of explicit measures of simplicity to decide preference of one form over another form of grammar. (Lees 1957: 377–378)

It is significant that Lees' assessment of TGG's status as a scientific theory concentrates upon its use of 'an overt axiom system', suggesting that it is this aspect of the approach that enables the linguistic theory developed to be 'rigorous'. The implication here is that, before TGG, linguistic theories did not use this kind of methodology (or at least not so extensively), and that they provided instead either 'a mere reorganization of the data', or else a 'speculative philosophy' which (presumably) could not be validated empirically. Clearly, then, TGG was perceived by some of its first adherents to be a more scientific theory than its predecessors partly because it employed the axiomatic-deductive method and so obtained 'explicit results', and, given this perception, it is curious that the relationship between TGG and the formal sciences that influenced it has not been explored more fully.

Before assessing the existing historiographical studies of TGG, though, it is necessary to clarify some of the terminology used above. For instance, the phrase 'formal sciences' has already been deployed several times without comment and, since it will haunt the ensuing discussion, an initial consideration of its meaning is already overdue. In particular, it should be confessed from the outset that, in the following chapters, this phrase will be used in a rather broad sense. As expected, it will be understood to include various branches of pure mathematics and symbolic logic, but, in addition, it will also be stretched to include various kinds of applied mathematics and logic – an extension that is not free from controversy.[1] For example, specific attempts to use the methodology of classical logic to analyse processes of knowledge acquisition (e.g., logical empiricism) will be considered as instances of 'formal science', as will efforts to explore the logical relationship between language and the real world (e.g., truth-theoretical semantics). Consequently, the phrase will come to denote a rather heterogeneous collection of related theories drawn mainly from mathematics and philosophy, and, at times, it may seem as if it denotes so much that ultimately it denotes nothing at all. However, it is important to recognise that the theories grouped together beneath this umbrella term all utilise some form of the

axiomatic-deductive method and that, therefore, despite their many differences, they all involve the deduction of consequences (i.e., theorems) from a small set of intuitively obvious axioms or assumptions, and, as a result, they can be viewed as being unified by the same basic scientific method. In the light of this observation, it should be remembered that not all intellectual enterprises (specifically, not even all sciences) can be pursued by means of this method. In order for an axiomatic-deductive system to be constructed at all, it is necessary to be able to state initial assumptions, to identify primary elements of some kind, and to make valid deductive inferences from these assumptions and elements. There are many areas of research that are not understood with sufficient precision to permit an axiomatic-deductive analysis. However, the 'formal sciences' all attempt to utilise this methodology, and it is one of their characteristic features. Other similarities will be mentioned as and when they become relevant to the discussion sustained in the rest of this book, but, from henceforth, the cautionary quotation marks will be dropped, enabling the 'formal sciences' to become merely the formal sciences.

Another terminological problem that was avoided in the first couple of paragraphs concerns the phrase 'Transformational Generative Grammar', and since this phrase appears frequently in this book, it is imperative that its meaning is clear. It is already the case (in the foregoing discussion of Lees' review) that 'TGG' has been referred to as if it were a single, identifiable, unchanging, monolithic entity. However, if the work presented in the following chapters demonstrates anything, it demonstrates that it is alarmingly anachronistic to use the term 'TGG' broadly to refer to the various theoretical stances and projects associated with Noam Chomsky (b. 1928) during the years *c.* 1951–*c.* 1956, since, although the various lines of research eventually fused into an identifiable theory, a coherent picture did not begin to emerge until *c.* 1957, when Chomsky began to draw together his ideas concerning the structure of linguistic theory, the validity (or otherwise) of statistical models in syntactic research, the benefits (or otherwise) of logical syntax and semantics, and so on. However, this convergence of ideas was of ephemeral duration, and by the early 1960s the theory had begun to change once more. Perhaps, instead of 'TGG', the term 'Transformational Analysis' should be used to refer to the theory presented in Chomsky's 1955 text *The Logical Structure of Linguistic Theory*, since this was the title he gave to his 1955 Ph.D. thesis? Unfortunately, though, the range of work that Chomsky accomplished in his earliest publications and manuscripts is sufficiently wide that any single term would cause difficulties and eventually demonstrate its own inadequacy. Consequently, for the purposes of the following discussion, the term 'TGG' will be understood to refer to the syntactic

theory that Chomsky expounded in *The Logical Structure of Linguistic Theory* (*c.* 1955) and, less formally (but in a broader context), in *Syntactic Structures* (1957). As a result, 'TGG' can become TGG again. It should be added, though, that since the intention is to show how the formal sciences gradually began to influence syntactic theory in general, ultimately resulting in the creation of TGG, the focus of the discussion will often be upon various papers, theses, and manuscripts concerning linguistic theory that appeared during the years 1900–1957 and not just on Chomsky's work from the 1950s. The task is to situate TGG more securely (if possible) in the scientific context of the time.

Accordingly, having provided at least an initial discussion of some of the difficulties, terminological and otherwise, that are inevitably encountered in a study of this kind, it is necessary to assess existing publications concerning the genesis of TGG in order to motivate more particularly the need for a serious consideration of the influence exerted by the formal sciences upon the development of TGG. The following discussion of the existing historiographical literature makes no claim to be exhaustively comprehensive. Indeed, the body of secondary literature concerning the development of TGG has started to accumulate at such a rate that a detailed summary of all the posited interpretations, assessments, revisions, and disagreements would leave little time or space for a discussion of primary sources. Instead, the overview offered here is intended generally to indicate how historiographical studies of TGG have developed since the 1960s, with the perceived deficiencies in the existing studies being highlighted. One final word of warning, though. As is usually the case, this introduction was written after the rest of the book was complete. Therefore, some of the topics mentioned in passing here may seem richer, deeper, less arbitrary, and less idiosyncratic after the main chapters of the following text have been perused. Consequently, although the rest of this introduction effectively constitutes a survey of related literature, like all such surveys it should be re-evaluated after the rest of the book has been read.

The earliest historical studies of the development of TGG can be found in the first textbooks devoted to the theory, and these began to appear in the early 1960s. Although these books often contained some sort of overview of the development of the theory, the summaries offered were usually brief and generally gave the impression that the origins of TGG could be traced back to *Syntactic Structures*, and no further. A fine example of this kind of pedagogic text is Emmon Bach's *An Introduction to Transformational Grammars* (1964). The plural of the title is revealing: the syntactic theory Bach presented had not yet developed into the study of a single, genetically embedded Universal Grammar, consequently, there could still be lots of TGGs. In his more historically inclined passages,

Bach gives particular emphasis to the influence of the formal sciences upon the development of TGG. For instance, he states explicitly that the theory 'has taken its inspiration from modern logic' (Bach 1964: 9), and he goes on to suggest that mathematics, logic, and linguistics have moved closer together during the last one hundred years (i.e., 1864–1964):

> In the last century a great deal has been learned about the structure of deductive systems (systems of logic, mathematics, axiom systems for various sciences). Logicians and mathematicians have been concerned more and more with studying various 'language systems' or 'calculi' from an abstract point of view. At the same time, modern linguistics has tended towards describing languages as abstract formalised systems. In many ways, the theory of language presented here may be considered the result of a convergence between these two currents. The grammars that we shall study are attempts to state the principles by which sentences of a language may be constructed, in much the same way that a formalised mathematical theory may be used to construct theorems. (Bach 1964: 9–10)

Like Lees before him, therefore, Bach states clearly that TGG makes use of the same kind of axiomatic-deductive systems utilised by various branches of mathematics, and, in addition, he suggests that linguistics and mathematics have been converging for at least a century. Later, he observes more specifically that the use of techniques derived from 'modern logic and mathematics' in TGG may constitute 'the most lasting result of the linguistic research of the last decade' (Bach 1964: 143). Unfortunately, though, Bach feels that 'to document this bit of cultural history in detail would take us well beyond the bounds of this introduction' (Bach 1964: 144). Consequently, he leaves this topic largely undeveloped and does not return to it later in the book.

As the 1960s progressed it became increasingly obvious to the international linguistics community that TGG was significantly more than an ephemeral fad, and consequently the issue of the historical roots of the theory began to inspire more interest. Chomsky himself contributed to this general trend when he published *Cartesian Linguistics: A Chapter in the History of Rationalist Thought* (1966), a somewhat disingenuous attempt to demonstrate that historical precedents existed for concepts such as deep structure and surface structure – concepts he was then in the process of elaborating. Specifically, Chomsky declared that TGG could be viewed as 'a reawakening of interest in questions that were, in fact, studied in a serious and fruitful way during the seventeenth, eighteenth and early nineteenth centuries' (Chomsky 1966: 1), and he sought to argue his case by focusing on such works as the *Port-Royal Grammar* (1660) and by reassessing the work of linguists such as Wilhelm von Humboldt. Although (much to his

annoyance) Chomsky's book has never really been taken seriously by linguistic historiographers, who tend to classify it as a work of ideological propaganda rather than as an objective historical assessment of the development of syntactic theory, it certainly seems to have inspired an interest in the task of situating TGG securely within the history of ideas.[2] However, since Chomsky was primarily concerned with linguistic research that had been completed before the mid-nineteenth century, he did not consider the actual development of TGG itself, and therefore *Cartesian Linguistics* offers few insights into the emergence of generative grammar. Curiously, a more revealing contemporaneous insight into the nature of this development can be gleaned from the writings of the post-Bloomfieldian Charles Hockett (1916–2000), who, by the late 1960s, had become a rather lonely figure in the world of international linguistics. During the previous decade Hockett had been generally recognised as one of the dominant linguists of his generation, but, as the years passed, linguistic theory started to take a distinctly Chomskyan turn, and Hockett found himself marginalised; a prematurely redundant relic of a bygone era. His response was to publish *The State of the Art* (1968), a book in which he expressed his dissatisfaction with Chomsky's general approach to syntactic theory, and it was while he was preparing the ground for his robust critique of Chomskyan syntax that Hockett recalled the circumstances surrounding the development of TGG in the 1950s:

> Lacking any explicit guidance as to where to turn for a broadened basis for linguistic theory, Chomsky was forced on his own resources and tastes, and turned towards the abstract fields of logic, mathematics, and philosophy, rather than to science. If Harris' work suggested either of these directions, it was the former. Indeed, a number of us at the time, in our search for 'rigor', were gazing longingly towards mathematics . . . the move was reasonable, since linguistics (or language) surely has interconnections of various sorts with these scholarly endeavours just as it has with anthropology, psychology and biology. (Hockett 1968: 36)

Certainly Hockett cannot be presented as an impartial historian of syntactic theory, mainly because he himself was an active participant in the various debates of the 1950s, and was specifically interested in utilising techniques derived from mathematics in linguistic research. Nevertheless, his comments are perhaps of some significance.[3] It is revealing, for instance, that, while recalling the heady days of the previous decade, when linguists (and not just those closely associated with TGG) were 'gazing longingly' towards mathematics, Hockett uses a first-person plural pronoun, suggesting that a number of linguists were turning to mathematics as a possible source of analytical techniques because they felt

that particular methodologies employed by the formal sciences (i.e., for Hockett, 'abstract' sciences such as logic and mathematics) could enable linguistic theory to be endowed with greater rigour. As will be shown in section 2.3, the use of the term 'rigour' in this context is replete with significance. Unfortunately, though, like Bach before him, Hockett does not provide a detailed account of precisely which branches of mathematics he considered to have been especially influential, nor does he indicate how the linguists managed to acquire a working knowledge of contemporaneous developments in mathematics.

During the 1970s, as it became increasingly clear that Chomsky's place in the history of linguistics was secure, his early work began to be scrutinised more closely by linguistic historiographers. For instance, John Lyons discussed various aspects of TGG in his relatively non-technical book *Chomsky* (Lyons 1970), and while he certainly argues in this text that Chomsky's work had ushered in a new period of linguistic research (describing *Syntactic Structures* as a 'short but epoch-making book' (Lyons 1970: 36)), Lyons is keen also to emphasise the similarities between TGG and the type of grammatical research conducted by the post-Bloomfieldians. For example, at one point he remarks that

> Chomsky's general views on linguistic theory as presented in *Syntactic Structures* are in most respects the same as those held by other members of the Bloomfieldians school, and notably by Zellig Harris. In particular, it may be noted that there is no hint, at this period, of the 'rationalism' that is so characteristic a feature of Chomsky's more recent writing. His acknowledgement of the influence of the 'empiricist' philosophers, Nelson Goodman and Willard Van Orman Quine, would suggest that he shared their views; but there is no general discussion, in *Syntactic Structures*, of the philosophical and psychological implications of grammar. (Lyons 1970: 36)

It is striking that Lyons should emphasise the apparent absence of rationalism in Chomsky's early work, and that he should associate this absence with the influence of the 'empiricist' philosophers Nelson Goodman (1906–1998) and Willard Van Orman Quine (1908–2000). However, although Lyons returns to the topic of 'the evolution of Chomsky's thought from empiricism to rationalism' (Lyons 1970: 38) later in the book, he does not discuss the influence of Goodman and Quine's constructive nominalism upon Chomsky's early work, nor does he assess Chomsky's later rejection of nominalistic techniques. Instead, the focus of Lyons' discussion is exclusively upon Chomsky's better-known 1959 critique of behaviourism.[4]

Another authoritative (and now sadly neglected) study of Chomsky's work, which appeared in the early 1970s, was Finngeir Hiorth's *Noam Chomsky: Linguistics and Philosophy* (1974). As the title suggests, Hiorth was as interested

in the philosophical implications of Chomsky's research as he was in its consequences for linguistics, and, as a result, his book discusses the origins of TGG in greater detail than most previous (and subsequent) studies. For instance, Chomsky's frequently ignored 1953 paper is discussed in (comparatively) considerable detail, and Hiorth concludes that it 'does not necessarily show that Chomsky ever had great faith in the importance of formal, purely symbolic, analyses for the purpose of empirical linguistic research' (Hiorth 1974: 35). In this context Hiorth goes on to consider the influence of constructive nominalism upon the young Chomsky, observing that

> In 1952–1953 Chomsky not only had a firm belief in the power of formal methods; he also believed in an 'inscriptional nominalistic'. . . approach. The trend in his thinking at that time was due to the influence of Nelson Goodman and W. V. Quine. The occurrence of the term 'nominalistic' here did not reflect any mature belief in philosophical nominalism. To my knowledge in Chomsky's later work there is no trace of nominalism. (Hiorth 1974: 37)

This (brief) mention of Goodman and Quine is unusual in the historiographical literature devoted to TGG, where virtual silence is the norm. Regrettably, though, Hiorth does not trace the precise nature of the influence of constructive nominalism, and it is suggested that after 1953 Chomsky rejected nominalistic methods altogether – a rather misleading claim, as the discussion in chapters 4 and 5 of this book seeks to demonstrate. Hiorth's interest in the influence of mathematics and logic upon TGG is also manifest, though, when he considers Chomsky's complex attitude towards logical syntax and semantics and the nature of their relationship to syntactic theory. The focus of his discussion is upon the exchange between Yehoshua Bar-Hillel (1915–1975) and Chomsky in the mid-1950s, but there is a brief mention of Rudolf Carnap (1891–1970), particularly his influence upon Bar-Hillel. After this the bulk of Hiorth's text concentrates upon *Syntactic Structures* and Chomsky's publications from the 1960s.

The year after Hiorth's text appeared, Chomsky himself published his own account of the genesis of TGG. His overview was contained in his introduction to *The Logical Structure of Linguistic Theory*, a truncated version of which was published for the first time in 1975. Chomsky's discussion is detailed, and the section that concerns his own intellectual development during the late 1940s and 1950s contains the following passage:

> At Harris's suggestion I had begun to study logic, philosophy, and foundations of mathematics more seriously as a graduate student at the University of Pennsylvania, and later at Harvard. I was particularly impressed by Nelson Goodman's work on constructional systems. In its general character, this work was in some ways similar to Harris's, and seemed to me to provide the

> appropriate intellectual background for the investigation of taxonomic procedures that I then regarded as central to linguistic theory. But Goodman's ongoing critique of induction seemed to point in a rather different direction, suggesting the inadequacy in principle of inductive approaches. Goodman's investigation of the simplicity of systems also suggested (to me at least) possibilities for nontaxonomic approaches to linguistic theory. Quine's critique of logical empiricism also gave some reason to believe that this line of enquiry might be a plausible one. Quine argued that the principles of scientific theory are confronted with experience as a systematic complex, with adjustments possible at various points, governed by such factors as general simplicity. (Chomsky 1975[1955]: 33)

This revealing summary suggests that during the early 1950s Chomsky perceived a close association between the methodology of constructional system theory and the techniques employed by the post-Bloomfieldians, especially Harris. Unfortunately, as will be shown below, Chomsky's reference to 'Goodman's ongoing critique of induction' seems to have convinced certain linguistic historiographers that Goodman was a rationalist who rejected empirical procedures when, actually, the opposite was in fact the case. Indeed, as is shown in section 5.3 below, Goodman's apparent critique of induction was merely the prelude to a robust defence of the same, and Chomsky himself was well aware of this. Quine, who started out as an adherent of Carnapian logical empiricism, shifted his position more than Goodman, so that by the 1950s he was certainly disillusioned with Carnap's approach, and, as indicated in the above passage, his writing from this period influenced Chomsky directly. Chomsky's recollections also indicate the direct association between Goodman's ideas concerning the simplicity of constructional systems and his own early preoccupation with the concept of grammatical simplicity. It should be noted, though, that, while it is illuminating to read Chomsky's own reflections upon these aspects of his early work, his reminiscences do not really reveal anything that was not already implied by certain comments and footnotes in his original papers and manuscripts.

Another significant passage in Chomsky's 1975 recollections occurs when he considers some of the particular branches of mathematics that influenced his thinking during the formative years in the early 1950s.

> Perhaps a word might be usefully added on the general intellectual climate in Cambridge at the time when [*The Logical Structure of Linguistic Theory*] was written. Interdisciplinary approaches to language communication and human behaviour were much in vogue . . . Oxford ordinary language analysis and Wittgenstein's later work were attracting great interest. The problem of reconciling these approaches (if possible) with Quine's provocative ideas on

> language and knowledge troubled many students. Mathematical logic, in particular recursive function theory and metamathematics, were becoming more generally accessible, and developments in these areas seemed to provide tools for a more precise study of natural language as well. All of this I personally found most stimulating. (Chomsky 1975[1955]: 39)

This passage supports Hockett's description of linguists 'gazing longingly' towards mathematics in the 1950s. Recursive function theory and metamathematics are specifically highlighted, but, since no details are given, questions remain. Which aspects of recursive function theory and metamathematics interested linguists at this time? Where was information concerning these subjects obtained? Who was making these ideas accessible? Precisely which linguists were interested in these topics? There are many unresolved issues. Indeed, although the above passage helpfully confirms the nature of the influences that are detectable in Chomsky's early publications and manuscripts, it also frustrates since it raises numerous further queries without providing answers.

In the same year that Chomsky published his authoritative account of the development of TGG, detailing the manner in which he had reformulated linguistic theory by rejecting the 'taxonomic' approaches of the post-Bloomfieldians, another text appeared that seemed to question this interpretation of linguistic history. This text, *American Structuralism*, was written by Dell Hymes and John Fought, and their detailed re-evaluation of structuralist research in the 1940s and 1950s undermined the (then) generally accepted view that Chomsky's work had inspired a complete revolution in syntactic theory. Hymes and Fought's main argument was that the post-Bloomfieldians could not usefully be grouped together as a single group that pursued a coherent research programme, since, in truth, they constituted a disparate collection of individuals who were responsible for 'a variety of alternative conceptions, and individual directions of interest' (Hymes and Fought 1981[1975]: 156). Therefore, Hymes and Fought argued that it was misleading to view Chomsky's work as a reaction against a homogeneous generation of linguists, and, in a passage reminiscent of Lyons, they provocatively suggest that 'in 1957 Chomsky's work might appear to be to a great extent a victory for one Bloomfieldian approach to syntax (that of Harris) as against another (that of Trager, Smith, et al.)' (Hymes and Fought 1981[1975]: 155). However, with specific reference to the origins of TGG, Hymes and Fought do not explore the influence of the formal sciences upon the work of the post-Bloomfieldians in any great detail, and they largely neglect the gradual mathematisation of syntactic theory that occurred during the 1940s and 1950s, with the result that their account of the genesis of TGG, though impressive, is, in this respect, incomplete.