REVIEW ARTICLE

Tracking the origins of transformational generative grammar[1]

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Marcus Tomalin, *Linguistics and the formal sciences: The origins of generative grammar* (Cambridge Studies in Linguistics 110). Cambridge: Cambridge University Press, 2006.
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Tracking the main influences of 19th- and 20th-century mathematics, logic and philosophy on pre-1958 American linguistics and especially on early Transformational Generative Grammar (TGG) is an ambitious cross-disciplinary endeavour. Ideally it would call for expertise in the methods of intellectual historiography, the history and content of 20th-century American linguistics, the history and philosophy of science (including logic and mathematics), the tools and results of mathematical logic, and the theory of computable functions. Scholars fully versed in all of these fields are rare indeed. If Marcus Tomalin makes some mistakes in his book (henceforth *LFS*), that should not be surprising. What is surprising is how much progress he makes in furthering intellectually serious work on the history of modern linguistics, and how wide his reading in the relevant technical literature has been.

LFS locates the intellectual roots of TGG in the methods developed by 19th- and 20th-century mathematics and logic for exhibiting the conceptual structure of theories and constructing rigorous proofs of theorems. Tomalin discusses the methods developed by Augustin-Louis Cauchy for the rigorisation of the calculus in the 1820s; Whitehead & Russell's use of the axiomatic method in *Principia Mathematica* (1910–1913); the Hilbert program (in the 1920s) to prove all of mathematics consistent; Bloomfield's early axiomatisation of a general linguistic theory (1926); Carnap's logical empiricist proposals for the logical reconstruction of science on an experiential basis in the 1920s and 1930s; and Goodman's (1951) adaptation and revision of Carnap (1928). Earlier histories of TGG have not investigated the linguistic relevance of this literature. Tomalin argues persuasively that the two-level approach to theorising now associated with early TGG (grammars as theories of languages, plus a metatheory about the form of grammars) was a specialised adaptation to linguistics of techniques developed for doing metatheoretical work on mathematics and logic, which were also adopted and applied in the philosophy of science. And he points out that the approach can be found in linguistics from Bloomfield (1926) on.

In this article we concentrate mainly on places where we think there are further questions that should be asked, or where we disagree with Tomalin, or where we think he made mistakes. However, we want to emphasise from the start that Tomalin deserves much credit: the scope of his reading of the primary literature is broad and deep, and his book provides a valuable scaffolding for future work in the area, even if it goes wrong in some of the details.

1. AXIOMATISATION AND THE FORMAL SCIENCES

The introductory chapter of *LFS* presents a rough characterisation of what the formal sciences are. For Tomalin, the phrase 'the formal sciences'

> will be understood to include various branches of pure mathematics and symbolic logic, but, in addition, it will also be stretched to include various branches of applied mathematics and logic

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> . . . Consequently, the phrase will come to denote a rather heterogeneous collection of related theories drawn mainly from mathematics and philosophy . . . 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. (*LFS*: 2f.)

This crucially assumes that logical consequences in the formal sciences are DEDUCTIVE consequences, not semantic consequences. Thus Tomalin immediately signals what will become a central theme of the book, that the kind of proofs that are characteristic of formal sciences are DERIVATIONS. This excludes some tools of logic and mathematics that are typically taken to fall within axiomatic deductive methods. As Tomalin tells the story, what was really central to the birth of TGG was the idea of a formal theory, which he identifies with Hilbert's proof theory (*Beweistheorie*). So model theory appears to be excluded from the methods of the formal sciences – a very radical restriction, excluding from the formal sciences a huge amount of pure and applied mathematics logic, and large areas of formal linguistics. (As we shall see, the exclusion of semantics was NOT in fact characteristic of the methods used to rigorise the calculus.)

Tomalin's view of what 19th-century mathematicians would have understood by 'rigorisation' is the one expressed by Grabiner (1981: 5, quoted in *LFS*: 26f.):[2]

> First, every concept of the subject had to be explicitly defined in terms of concepts whose nature was held to be already known . . . Second, theorems had to be proved, with every step in the proof justified by a previously proved theorem, by a definition, or by an explicitly stated axiom . . . Third, the definitions chosen, and theorems proved, had to be sufficiently broad to support the entire structure of valid results belonging to the subject.

Newton, Leibniz and others had accepted certain results of the calculus on the grounds that they were predictively accurate. Cauchy wanted to prove these already accepted results from explicitly stated axioms and, crucially, explicit definitions of concepts such as 'limit', 'convergence', 'continuity', 'derivative', etc. The idea was not only to put the calculus on an epistemologically secure foundation but also to see the connection between the defined concepts and the result proved. Thus the proofs were not mere symbol manipulations; as Grabiner says, 'the *derivation* of a result by manipulating symbols was not a *proof* of the result'. Both the definitions and the axioms were supplied with meaning through a reduction to algebra, which was regarded as more secure than the calculus was in the 18th century.

Tomalin does not separate out the various components making up Cauchy's axiomatic-deductive methods, but they could be stated as practical maxims. Our proposed phrasing of the maxims attributable to Cauchy (we will suggest other maxims later) would be as follows:

(A) State the fundamental assumptions of the pre-formal theory as axioms.
(B) Define all non-primitive concepts explicitly.
(C) In proofs of theorems, use only definitions, axioms and previously established theorems.
(D) Reduce the axioms, definitions, and theorems to a better-understood and less controversial theory.

The use of (A) and (B) in mathematics dates back at least to Euclid's *Elements*, so they are certainly not novel in 19th-century mathematics. Even Newton called his laws of motion 'axioms', so

(A) does not distinguish Cauchy's rigorous methods from Newton's empirically justified, prediction-oriented calculus. But in (B) we see a dramatic difference between Euclid and Cauchy, on the one hand, and Newton and Leibniz, on the other: Cauchy wanted to replace Newton's inchoate intuitive notion of 'fluxion' and Leibniz's use of 'momentary differences' by explicitly defined concepts.

And even more importantly, (C) and (D) are key components of Cauchy's contribution to the development of axiomatic-deductive methods. The use of reductive methods like (D) is typical when theories are axiomatised for epistemological reasons. For Cauchy, (D) was adopted in part to exhibit the relationships between concepts, fundamental assumptions, and theorems. But there are also other epistemological reasons for adopting (D). One is EPISTEMOLOGICAL FOUNDATIONALISM: the view that theories must be made secure against rational doubt. The idea that mathematical and logical theories can achieve foundationalist goals by means of (D) came under attack in the last half of the 20th century, and the need for foundationalist projects in mathematics has been rejected in recent decades (see Shapiro 1991 for an excellent discussion). Tomalin, however, appears to assume that epistemological foundationalism is not just a motivation for using the axiomatic method, but part of it: he fails to distinguish between foundations and foundationalism (see, for example, pp. 28, 38–45).

Tomalin does not compare the goals of different mathematical or logical programmes that developed and advocated the use of axiomatic methods. Indeed, he writes as if there is a unique, univocal axiomatic-deductive method regardless of goals. This, we think, is a mistake: not every maxim is adopted by every theorist, because different maxims are useful in achieving different goals.

The set-theoretic paradoxes that were discovered in the second half of the 19th century (*LFS*: 29–32) provoked at least three philosophical movements which Tomalin surveys: Logicism, Formalism, and Intuitionism. It is the first two that loom largest in *LFS*.

Logicism is the programme in the philosophy of mathematics that was pursued by Frege, and by Whitehead and Russell; Tomalin describes it as the project of using logic 'as a basis for arithmetic' (p. 33), though in fact there are various different formulations (see Rayo 2005 for a discussion).

Hilbert's Formalism is a different philosophical programme. Most philosophers of mathematics would characterise it as the programme of showing that the whole of mathematics is consistent and complete. Hilbert's proof theory was not the whole programme; rather, it was the most famous strategy for working on the programme.

Tomalin claims that 'perhaps the main achievement of this book has been to associate TGG with both Formalism and Logicism' (p. 186). The trouble is, Tomalin equivocates on 'Formalism', correctly pointing out a common misconception, but then apparently falling into it himself. To make this clear we need to explain a bit about Hilbert's programme.

A crucial step in Hilbert's strategy for showing the consistency of mathematics (see Hilbert 1927) was to convert ordinary arithmetical propositions into formulas by a fully automatic procedure referred to by Tomalin as FORMALISATION (p. 40). Hilbert's insight here is that the propositions of mathematics can be mechanically paired with syntactically defined formulas. This conversion step is a formalisation in the sense that it mechanically produces formulas that serve as meaningless representatives of the propositions of ordinary arithmetic. Hilbert calls them copies (*Abbilder*) of those propositions.

A formula is then said to be PROVABLE if it is the last line of a DERIVATION in a sense that should be very familiar to those who know generative grammar: a derivation is a sequence of formulas each of which either is an axiom, or is a substitution instance of an axiom, or is obtained from the axioms and substitution instances by an inference rule. Consistency was then to be proved by showing that some mathematical contradiction like $0 \neq 0$ is NOT provable. (That shows that the whole system is consistent, because in an inconsistent system EVERYTHING is provable. Incidentally, the linguist acquainted with basic results on generative capacity will see here the seeds of Gödel's celebrated later result: to assume we have a mechanical method for showing unprovability of a formula is to assume that the set of all provable formulas is a decidable set; the problem is that it might not be.)

Inference rules function rather like rewriting rules in a generative grammar (see section 5 below), and are formal in the sense that they rely purely on form: they do not depend on any interpretation of the formulas. Hilbert thus employs string manipulation techniques on meaningless copies of the meaningful propositions of arithmetic, as a way of building consistency proofs. But IT DOES NOT FOLLOW from this that mathematics itself is a game played with meaningless symbols: the propositions of mathematics are not meaningless (Hilbert's strategy merely employs meaningless *Abbilder* as surrogates for them).

Tomalin seems to understand all this; he spends a page (44) warning that it is a 'common misconception' that Hilbert thought mathematics was a game played with meaningless symbols – a misconception that was partly due to people ignoring the 'distinction between the formalisation process and the metamathematical process'.

Note carefully that Tomalin uses 'formalisation' for the step of converting mathematical propositions into formulas; 'formal' for systems and proofs that make no reference to meaning; and 'Formalism' for the larger enterprise of proving mathematics consistent.

But only five pages later, in the context of pointing ahead to his subsequent discussion of Hilbert's influence on Carnap and Quine, Tomalin (49) changes his terminological policy, and conflates Formalism with being formal. He notes correctly the 'interest in the syntax of formal languages' displayed in Quine's *Mathematical Logic* (1940), but incorrectly claims 'his presentation is conspicuously influenced by Formalism'. The example he cites involves a passage where Quine

> introduces an alphabet of primitive symbols and adds, with reference to strings formed from this basic symbol set, 'all these characterisations are *formal systems*, in that they speak only of the typographical constitution of the expressions in question and do not refer to the meanings of those expressions' (Quine 1940: 283), and it is this emphasis on form, rather than the content of symbolic expressions, that reveals the influence of Hilbert's proof theory (as mediated by Carnap).

In Tomalin's view, it is not the programme of showing all of mathematics to be consistent that has 'conspicuously influenced' Quine in electing to work with strings over a fixed finite alphabet; nor is it what Tomalin earlier called the FORMALISATION step; it is merely being formal.

This is not a mere quibble. Which of the three – the formalisation step, the project of Formalism, or being formal – does Tomalin claim to have linked to the origins of TGG?

Tomalin mentions some other axiomatisations of mathematical and logical theories that all satisfy at least maxims (A)–(C): John Young's *Fundamental Concepts of Algebra and Geometry* (1911), W. V. O. Quine's *Mathematical Logic* (1940), and Alonso Church's *Introduction to Mathematical Logic* (1944), for example.[3] In the context of discussing Church, he hints at what he understands the axiomatic-deductive method amounted to (49):

> Church presented the same basic topics which, by the mid-1950s, were rapidly becoming an incantational mantra (i.e., primitive symbols, variables, quantifiers, propositional calculus, first-order predicate calculus, second-order predicate calculus, and so on) . . .

The first three 'topics' that Tomalin mentions are not topics at all, but just standard categories employed in the syntax and semantics of typical formal languages. What Tomalin might have in mind is that the following maxim is often adopted in the most formal presentations of mathematical and logical theories:

(E) Use a well-defined invented formal language to state the axioms.

And the last three 'topics' are simply distinct types of logic, some of which had properties that were well known by the mid-1940s. Possibly Tomalin means to suggest another maxim, (F):

(F) Identify the logic that is used in deriving the consequences of the (non-logical) theory.

(This would be a special case of Suppes' observation (1956: 156) that in mathematics 'a statement of what theories are assumed is needed'.)

The motivation for adopting (E) is often taken to be to rid axiomatisations of the vagueness and ambiguity of natural language. The motivation for adopting (F) is that without it there is no explicit objective standard of what follows from the axioms of the theory.

LFS began by claiming that the formal sciences 'can be viewed as being unified by the same basic scientific method' (3). It should be clear that (A)–(F) do not characterise any single method. They are a family of maxims, from which different researchers adopt different subsets for particular purposes. There is no unified 'axiomatic-deductive method': which of the maxims a theorist chooses to respect will depend on the goals of the axiomatisation project, and goals are diverse.

2. The mathematicising of linguistics

Tomalin's long chapter headed 'Mathematical linguistics' is not about mathematical linguistics. It is a survey, in 53 pages, of historical material rich enough to make a book on its own: the extent of the influence of mathematical logic, recursive function theory, and logical empiricism on the mathematicisation of linguistics in the period 1926 to 1953. It is necessarily somewhat superficial, but could serve as a useful guide to sources for the future work on the use of formal methods in linguistics and how they influenced linguists' conceptions of good linguistic theorising.

The chapter argues that 'the' axiomatic method as developed in mathematics and logic was applied to presenting linguistic theories during the quarter of a century before Chomsky (1955 [1975]). So part of what makes the chapter important given the goals of *LFS* is its role in showing that there was tacit agreement among a group of American linguists (in particular those most influenced by Bloomfield) about the general form linguistic theories should take.

Tomalin begins by discussing Bloomfield's paper 'A set of postulates for the science of language' (1926), which attempted to set out an entire approach to linguistic theorising in axiomatic form. He quotes one or two examples of Bloomfield's axioms and definitions, but does not discuss the content of the paper in detail. This is an interesting signal that *LFS* itself takes the curious view that the form of linguistic theories is worth discussing quite independently of their content. This is curious because evaluation of the appropriateness of axiomatising must surely depend in part on what is to be axiomatised.

We do not agree at all with Tomalin's comparison between Bloomfield (1926) and Cauchy's rigorisation of the calculus. Tomalin writes (55):

> The comparison with Cauchy's rigorisation programme is not vacuous since the emphasis . . . is upon stating assumptions 'explicitly', and determining which aspects of a theory are 'interdependent' and which can be treated 'independently'.

But the mere fact that both Cauchy and Bloomfield (like Euclid) respected maxims (A) and (B) does not make it reasonable to draw a favorable comparison between Bloomfield's short paper and Cauchy's major project. The two projects are utterly different in important ways. Maxim (C) is irrelevant to Bloomfield (1926), because he did not prove, or try to prove, anything. Nor did Bloomfield follow maxim (D): he did not reduce any linguistic theory or grammar to a different theory that he favored at the time – say, Weiss's (1925) axiomatisation of behaviourist psychology (which, as Tomalin notes, was a model for Bloomfield 1926).

Bloomfield (1926) laid out the assumptions of his general approach to linguistic theorising, and urged that other linguists should do so as well. He wanted technical vocabulary to be explicitly defined in order to avoid certain kinds of error, and to show which specialised linguistic terms were undefinable and primitive and which could be defined in terms of the primitives (1926:26). He warned that otherwise linguists were in danger of falling prey to making obscure speculations about languages, as did Humboldt, or engaging in fruitless psychological disputes like the ones that had gone on between Paul, Wundt, and Delbrück.

Tomalin performs a very useful service in reminding linguists (or perhaps in many cases informing them for the first time) of the almost forgotten work of Harwood (1955), which suggested stating grammars for generating finite sets of strings in two parts: (i) a set of strings taken to be axioms, and (ii) a set of inference rules for deriving further strings from them. Chomsky (1957) does cite this work, but only to reject it as inadequate for not containing any provision for recursion. Harwood's motivation was apparently to consider methods for measuring economy of description of finite corpora. That may explain why Harwood seems content with a description that entails a finite bound on sentence length.

Tomalin's consideration of recursive devices (66) attempts to link Dedekind's use of recursive functions, Peano's inductive definition of the successor function, and Bar-Hillel's definition of 'sentence' that uses 'sentence' in the definiens (Bar-Hillel calls the latter 'recursive in disguise'). He then explores the influence on non-TGG linguistics of the Lvov-Warsaw school of logic founded by Twardowski.[4] The logicians who emerged from this extraordinarily important school included Łukasiewicz, Lesniewski, Ajdukiewicz, and Tarski. Tomalin is especially (and rightly) interested in the influence of Ajdukiewicz's definitions of logical calculi on Bar-Hillel's conception of syntax, which gave rise to modern categorial grammar. In our view, however, Bar-Hillel's work was more important to the history of linguistics than Tomalin explicitly recognises: it is not just all the work in categorial or type-logical syntax and semantics that largely sprang from it (Bach, Carpenter, Dowty, Jacobson, Lambek, Montague, Moortgat, Partee, Steedman, Szabolcsi, . . .), but also to some significant degree early HPSG, and Chomskyan minimalism, and the work of Keenan & Stabler (2003).

As Tomalin sees it, Bar-Hillel attempted to combine two things. One was the methods Harris (1951) developed to attempt to read off the structure of languages from corpora. The other was Ajdukiewicz's notation and approach to defining logical calculi. Tomalin holds that Bar-Hillel's adaptation of Ajdukiewicz-style formation rules 'implies that the type of formal languages used in symbolic logic (particularly the techniques developed by Ajdukiewicz) are closely related to natural languages and that therefore methods developed to analyse the former can readily be adapted in order to provide analyses of the latter' (73). Tomalin regards it as significant that categories can be combined to form well-formed strings by a 'purely mechanical process' (72). So Bar-Hillel's mechanical combinatory procedure could be regarded as analogous to Hilbert's formalisation, but it is not formal in the sense of being unassociated with a semantics.

Tomalin fails to note that, although Bar-Hillel never uses the word 'generate', he was unquestionably proposing a generative grammar, several years before Chomsky. Bar-Hillel's proposal was for a BOTTOM-UP generative grammar, in the sense that well-formed expressions are built up from smaller categorised parts (as in the post-1990 minimalist program) rather than being derived top-down from a start symbol as in pre-1958 TGG. This is important for Tomalin's project. As he notes later in the book (164), the 'particular concepts and techniques that eventually came to be associated with TGG were already spreading throughout the linguistics community, albeit in an imprecisely articulated form, by the mid-1950s.' Bar-Hillel's work, rooted in the work of the Lvov-Warsaw school, should have been mentioned in the same context.

3. LOGICAL EMPIRICISM

Tomalin is the first historian of linguistics to seriously examine the relevance to TGG of Carnap's constructional system theory, and Nelson Goodman's revision of it. Carnap's project encapsulated the logical empiricist research programme. It was set out in Carnap's *Der logische Aufbau der Welt: Versuch einer Konstitutionstheorie der Begriffe* (1928) (hereafter *Aufbau*). In *The Structure of Appearance* (1951; henceforth *TSA*), Goodman significantly modified Carnap's project.

Tomalin aims to articulate the direct line of intellectual influence from Carnap to Goodman and Quine, and from Goodman and Quine to Chomsky. He claims that the *Aufbau* 'exerted a lasting influence over syntactic theory in the 1950s' (*LFS*: 73). This is certainly right, in the sense that *Aufbau* influenced conceptions of what a scientific theory is, quite generally, from the 1930s to at least the 1960s.

It is well known that Chomsky took courses from Goodman at the University of Pennsylvania; but *LFS*, to its credit, actually discusses the content of Goodman's *TSA* (a revision of his dissertation), and Carnap's *Aufbau*. What Tomalin aims to show is that Goodman's revision of Carnap's logical empiricist project for rationally reconstructing all of science was adopted in Chomsky (1951) and (1955[1975]).

However, *LFS* does not discuss in any detail the aims of the logical empiricist program as articulated in the *Aufbau*. What matters to Tomalin is that the *Aufbau* endorsed maxims (A)–(D) in its proposal for constructing (or really, reconstructing) all of science. Hilbert had earlier proposed extending his program of Formalism to physics this way (see Corry 2004). But what the logical positivists wanted to do was to reconstruct all of science, psychological and sociological theories included; it was not part of their project to offer an explicit proof of consistency.

The *Aufbau* aimed to provide a metatheoretical framework that would (i) unify all scientific theories, (ii) secure the foundation of science by showing that it can be reconstructed on a purely experiential base, and (iii) show that all philosophical problems are pseudo-problems. Importantly, the *Aufbau* did not rationally reconstruct any particular scientific theory. It merely set out a programme, and addressed metatheoretical problems that might impede it. This is the way Carnap describes the project of reconstructing science:

> Unlike other conceptual systems, a constructional system undertakes more than the division of concepts into various kinds and the investigation of the differences and mutual relations between these kinds. In addition, it attempts a step-by-step derivation or 'construction' of all concepts from certain fundamental concepts, so that a genealogy of concepts results in which each one has its definite place. It is the main thesis of construction theory that all concepts can in this way

> be derived from a few fundamental concepts, and it is in this respect that it differs from most other ontologies. (Carnap 1967: 5, quoted in *LFS*: 74f.)

The fundamental experiential basis was an ‘autopsychological basis’ that ‘consists only of conscious experiences’ (1967: 102f.). These conscious phenomenal experiences are ‘the given’: a theory-neutral indubitable foundation in terms of which all concepts and objects of science were to be explicitly defined. The empiricism of ‘logical empiricism’ is located in this indubitable autopsychological base. As Tomalin notes, this reductive empiricist base was intended to be the secure epistemological foundation of science, and the idea that empirical knowledge needs such a base is the foundationalist dogma of empiricism that Quine (1953) inveighs against.

Tomalin seems to think that the *Aufbau* aimed ‘to consider questions of knowledge acquisition’ (74). This is something of a misunderstanding. The goal of the project described in the *Aufbau* was to JUSTIFY the claims that scientific knowledge is unified, based only on experience, epistemologically secure, and free of metaphysics. The ACQUISITION of scientific knowledge can be described historically or psychologically, but it is hard to understand how a programme for reconstructing all of science on an experiential basis could answer any question about the way people acquire scientific knowledge. For surely, even working scientists acquire much of their scientific knowledge from the testimony and publications of other scientists, not merely from their own phenomenal experiences.

Tomalin tells us that Quine’s familiarity with the *Aufbau* began in the early 1930s: it motivated him to visit the Vienna Circle in 1932 and 1933, and ultimately to visit Carnap, who was lecturing in Prague at the time. (We would add that Quine also visited Warsaw during this year; Dresner (1999) argues that through the influence of Carnap, Ajdukiewicz had a lasting influence on Quine’s philosophy of language.) He reports that Quine first ‘came into regular contact’ (78) with Goodman when Carnap was lecturing at Harvard on logical syntax in 1935. And Tomalin emphasises that Quine and Goodman shared an interest in the *Aufbau*.

What is more important, because of its later direct influence on Chomsky (1955 [1975]), is that Tomalin discusses – perhaps for the first time for an audience of linguists – the content of *TSA*, Goodman’s revision of Carnap’s programme in the *Aufbau*. Moreover, Tomalin emphasises that by 1943 Goodman had developed a deep interest in economy and simplicity as criteria of adequacy for constructional systems that would influence early TGG.

The discussion of economy in *LFS* includes the following striking passage that is very closely paraphrased in Chomsky (1955 [1975]: 114, n. 2, quoted in *LFS*: 116). Goodman writes:

> The motives for seeking economy in the basis of a system are much the same as the motives for constructing the system itself. A given idea *A* need be left as primitive in a system only so long as we have discovered between *A* and the other primitives no relationship intimate enough to permit defining *A* in terms of them; hence the more the set of primitives can be reduced without becoming inadequate, the more comprehensively will the system exhibit the network of interrelationships that comprise its subject matter. (Goodman 1943: 107, quoted in *LFS*: 83)

This idea of economy was, as Tomalin shows, adopted in early TGG as an adequacy condition on being one of the genuine primitives of an axiomatic deductive system, the primitives being all those concepts (or terms denoting them) that cannot be explicitly defined in the system without circularity. Highly formalised mathematical and logical theories aim for this kind of economy, i.e., they require that the primitives be independent of each other. If the goal of using the axiomatic-deductive method is to make the STRUCTURE of the subject matter perspicuous, then reflecting on the relations between the primitive concepts of the pre-formal theory (and their relations to each other as stated in the axioms) might be thought to display a structure of the subject matter of the theory. But, we should note, there will be other structures that are attributable to the subject matter under reaxiomatisations of the theory.

And we also note that Goodman speaks of THE network of interrelationships: he seems to be claiming in the above passage that basal economy might serve to constrain the theory up to the UNIQUE structure of a scientific subject matter!

Although Tomalin does not mention this point, the above passage from Goodman might be understood to express Hilbert's idea that the axioms of a mathematical theory give an implicit definition of the primitives of the theory. For example, in his correspondence with Frege, Hilbert wrote that in geometry terms like 'point' and 'line' should not be explicitly defined in intuitively spatial terms, as Frege thought, but rather that the set of axioms should be taken to implicitly define them, thus identifying the relations that exhibit the structure of the subject matter. In philosophy, this kind of definition is called an 'implicit functional definition', and it has been used by philosophers to state functionalist definitions of mind as an alternative to behaviourism. (The connections to syntax should be clear: a phrase structure grammar gives an implicit functional definition of the categories it employs.) This suggests that it might be worth investigating the genealogy and uses of implicit definitions from Hilbert through Carnap and Goodman to Chomsky. Instead, what Tomalin himself takes from the above quotation is this:

> In essence, a 'simpler' system is a better system, so long as it does not become 'inadequate'; and 'better' in this context means a more economical system, since such systems are understood to provide more profound insights into the phenomena analyzed. (*LFS*: 84)

This is a seriously uncharitable reading of Goodman. It is more plausible that Goodman is introducing a further standard part of the axiomatic-deductive method (see Suppes 1956: 156). It could be expressed in a further maxim:

(G) Ensure that all primitive terms independent of each other.

Tomalin's aim in the following section, entitled 'Formal linguistic theory' (88–106), is to establish a stream of influence flowing from Hilbert through Carnap's logical syntax to Bloomfieldian linguistics. The first step is to show Hilbert influencing Carnap. But there is a major flaw in Tomalin's exposition. In order to illustrate the 'indebtedness to Formalism' in Carnap's book he quotes Carnap's *The Logical Structure of Language* saying that a system is formal when

> no reference is made to the meaning of the symbols (for example words) or to the sense of expressions (e.g. sentences), but simply and solely to the kinds and order of the symbols from which the expressions are constructed. (Carnap 1934 [1937]: 1, quoted in *LFS*: 91)

But once again, Tomalin is merely referring to being formal, though he calls it 'Formalism'. It is true that Hilbert has been mistaken for a 'formalist' (in the game-playing sense – the 'common misconception' referred to earlier), and also that Carnap was one of those who misread Hilbert in this way; but Corry (2004) argues convincingly that it is indeed a misreading (as Tomalin seemed to accept on page 44). Tomalin is thus trying to trace Hilbertian influence through the work of someone who misunderstood Hilbert. If Tomalin is going to catalogue the genuine influence of Hilbert's actual views on Carnap and early TGG, then these threads of misunderstanding need to be sorted out.

The next step in Tomalin's argument is to establish the influence of Carnap on Bloomfield. Tomalin regards Bloomfield as a Carnapian 'formalist'. But why? Solely because 'Bloomfield repeatedly expressed skepticism about the role of meaning in linguistic theory' (95). It is true that Bloomfield located the study of meaning in psychology, not linguistics; and, notoriously, by the time he wrote *Language* (1933) he regarded behaviourism as the most promising research program in psychology. But Bloomfield's view that linguistics is about form in natural languages and not about meaning is present in the postulates that he published two years before the first (German) edition of the *Aufbau*. Bloomfield (1926: 27) states that the language of a speech community is 'the totality of utterances that can be made in that speech community', and that 'an utterance is made up wholly of forms'. If utterances are constituted by their non-semantic properties, and linguistics studies utterances, then it is no part of linguistics to investigate meanings. Bloomfield's exclusion of meaning from the subject matter of linguistics is thus entirely independent of Carnap (1928).

Bloomfield's view of linguistics has nothing to do with either formalisation, or Formalism, or being formal, as these were earlier characterised. Bloomfield proposed axioms and definitions, but they were fully interpreted: when he said 'utterance' he meant to refer to utterances. He was not interested in proofs of consistency; he proposed no mechanical procedures for obtaining such proofs; and he did not take either grammatical descriptions or general linguistic theories to be uninterpreted symbol systems.

4. Goodman, Chomsky, and generative grammars

The fourth chapter opens with a brief personal and intellectual biography of Noam Chomsky up to the mid-1950s. After that it attempts to 'identify the presence of a particular influence' on Chomsky, and to 'trace its development as his research gradually matured during the 1950s' (108). (This sounds like development over a whole academic career, but keep in mind that Tomalin is talking about largely unpublished work by a young man between the ages of 22 and 28!) The influences discussed include reminders of what was established in chapter 3, which makes the chapter rather repetitious.

Tomalin understands Chomsky's MA thesis, *The Morphophonemics of Modern Hebrew* (1951, henceforth *MMH*), as aiming to combine Harris' goal of developing a discovery procedure for grammars from a corpus with Goodman's constructional system theory and conception of simplicity. There is a long discussion of Chomsky's use of the concept of simplicity in MMH, and of his more substantive view of simplicity in his massive 1955–1956 work *The Logical Structure of Linguistic Theory* (Chomsky 1955 [1975], *LSLT*). In effect, Tomalin thinks that Chomsky took Goodman's program in *TSA* and applied it to Hebrew morphophonemics. (This would mean that Goodman's influence on Chomsky predated the publication of *TSA*, perhaps through material presented in classes.)

In section 4.4, 'Constructive nominalist syntax' (121–125), Tomalin discusses Chomsky's first published paper, 'Systems of syntactic analysis' (1953), which definitely does show some influence of Goodman's technical work. But we see again the equivocation about Formalism: 'The Formalist emphasis of Chomsky's paper is clear', he asserts, because he aims to keep syntactic analysis 'purely formal' (122), following the early 1950s 'trend in syntactic theory to develop methods of analysis that do not require access to semantic information' (121). This remark embodies a confusion (the same one that we just saw with reference to Bloomfield). The semantic information that the analysis needs no access to is the semantics of the natural language being analysed, not the semantics of the language in which the analysis is couched. Chomsky's interest in a mechanical procedure for going from a corpus to a description is perhaps reminiscent of what Tomalin calls 'formalisation'; but it is not the project of proving mathematics consistent that is influencing linguistics here.

We finally arrive at a discussion of early TGG in Tomalin's fifth chapter, which revisits (i) Chomsky's much-discussed rejection of discovery procedures and advocacy of evaluation measures; (ii) the idea that the 'constructional levels' of the *Aufbau* are hierarchical and so is the general theory of linguistic structure in *LSLT*; and (iii) the differences between transformational rules, recursive rules, and formal syntax. (The frequent returns to previously discussed topics for the purpose of examining their influence on early TGG reveals again that *LFS* is organised in a less than optimal way.)

Tomalin misses an important opportunity in his treatment of Hockett (1955) on pp. 144f., in a section where he rehearses Chomsky's already much-repeated arguments against finite-state automata as an adequate model for English. To set the stage for this, Tomalin notes that in the introduction to *A Manual of Phonology* Hockett sketches 'a Grammatical Headquarters (GHQ), which is responsible for generating the sentences that are spoken' (*LFS*: 144). This takes the form of a stochastic finite-state generator, not just another structuralist analytical method. What Hockett proposes is that a probabilistic generative grammar is an actual component of the human language user. The sketch given is crude; the illustrative example, which generates a tiny finite language, is just an expository toy, and many important differences separate Hockett's GHQ from later conceptions of the language faculty. Nonetheless, Tomalin completely misses this evidence that Hockett (whose book Chomsky actually reviewed in *International Journal of American Linguistics* in 1955) was an advocate of both generative grammars and their neuropsychological reality (see also Hockett 1948, also missed by Tomalin, where the latter point is extremely clear).

Another opportunity is missed in the discussion of Chomsky's arguments against stochastic models of languages – the only new topic introduced in this chapter. *LFS* cites the true claim from Chomsky (1957) that *Colorless green ideas sleep furiously* and *Furiously sleep ideas green colorless* had the same frequency in English before 1957 (namely, zero). But he adds the false claim that 'Therefore, [Chomsky] is obliged to conclude that frequency reveals nothing about grammaticality' (148). This is not true. Chomsky does assert that 'in any statistical model for grammaticalness, these sentences will be ruled out on identical grounds as equally remote from English', but he is wrong. He is assuming that the probability of a type of event must be regarded as zero if it has not occurred so far. That is the result that one gets from using the technique now known as maximum likelihood estimation (MLE). Chomsky was not obliged to adopt MLE. A more suitable technique had been developed during the Second World War by A. M. Turing and I. J. Good (see Sampson 2001, chapter 7, for an elementary exposition), and although it took a while to become known, Good had published on it by 1953.

Chomsky was simply not very interested in applying statistical methods to linguistic material, and knew little about them. In his disdain for such work he was followed by most linguists for the next forty years. But when Pereira (2000) finally applied Good-Turing estimation ('smoothing') to the question of how different the probabilities of the two famous word sequences are from normal English text, he found that the first (the grammatical one) had a probability 200,000 times that of the second.

In a section devoted to the topic of discovery procedures and grammar evaluation (149–155), Tomalin addresses Chomsky's early bias toward, and later bias against, logical empiricism and the influence of Goodman's thinking. Here we encounter a significant mistake in philosophical interpretation: Tomalin erroneously takes Goodman to be a logical empiricist. Tomalin refers to 'the kind of logical empiricism advocated by Goodman' (150) and writes that in the mid-1950s Chomsky abandoned the 'hard-line logical empiricism espoused by Goodman, and (seemingly) championed by Chomsky' (151). One can certainly read parts of *LSLT* as following the path of Carnapian (reductive) logical empiricism, and *Syntactic Structures* as rejecting it. But it is a mistake to charge Goodman with being a reductive empiricist. He was an outspoken critic of reductive empiricism, and of foundationalism, just as Quine was. As Geoffrey Hellman put it (1977: xxiii):

> Goodman has consistently been an original and leading opponent of the traditional empiricist dogma that all knowledge can be built up from some perceptual stratum free of conceptualization, for it is denied that such a stratum exists.

Tomalin counts his extended discussion of the influence Goodman and Quine had on Chomsky as one of the significant contributions that *LFS* makes to the history of linguistics. He is certainly right that the influence of Carnap, Goodman, and Quine on early TGG had previously been given no serious attention. But perhaps because Tomalin is venturing into literature that is unfamiliar to him, he gets Goodman wrong.

The question of whether Chomsky followed Quine and Goodman in rejecting foundationalism is worth considering on at least two levels: with reference to the epistemology of linguistics, and with reference to the epistemological question of how we can 'know' a language. There is some support for thinking that Chomsky retained his earlier foundationalism. However, to show this we first have to point out a further mistake of Tomalin's in interpreting Goodman.

Tomalin speculates that Chomsky's mistrust of inductive generalisation was based on Goodman's arguments in *Fact, Fiction and Forecast* (1954, henceforth *FFF*). He thinks that Goodman sees induction as a research method, and that the standard Humean problem of induction as discussed in *FFF* was taken as an 'alarming condemnation of empiricism as a practical philosophy' (153). But in fact the point of Goodman's discussion of Hume's (old) problem of induction in *FFF* is to argue that principles of induction (e.g., the principle that the future will resemble the past) are justified in just the same way that deductive rules of inference are justified. Goodman sees inductive and deductive rules of inference as equally justified. His discussion concerns the justification of rules of inference generally, not research methods, or empiricism (reductive or not), and there is little reason to think it contains the seeds of Chomsky's doubts. If it did, it would follow that justification of deductive inference was no better off. One may speculate that Chomsky's mistrust of conclusions reached by inductive inference stemmed from his foundationalism, since even good inductive arguments do not establish their conclusions with the same certainty as good deductive arguments. But his doubts concerning induction are not based on Goodman's arguments.

5. Transformational-generative grammars and the work of Post

Tomalin moves on to discuss TGG specifically, first describing some aspects of the theory developed in *LSLT*, and then discussing transformations and recursion. It is regrettable that he does not make use of or cite the very important and thought-provoking assessment of *LSLT* in Sampson (1979); it would have been useful to see Tomalin engage with the contrarian argument given there.

In the remainder of the discussion of TGG we find the most significant citational failure in *LFS*: its failure to note the importance of the work of the mathematical logician Emil Leon Post for the mathematical foundations of TGG. Post is cited very briefly seven times in *LFS*, always in order to mention (in a rather repetitive way) his work on the mathematics of mechanically enumerable sets of positive integers as summarised informally for a mathematical audience in Post (1944). There Post uses the device of finitely describable idealized procedures (e.g., 'write down 2, 4, 8, 16, 32, . . . , and continue thus forever') as a way of specifying infinite sets whose membership could be computationally enumerated. In the terminology Post uses, these procedures GENERATE the sets. And to read Tomalin, one might think that the only relevant thing about Post is that Chomsky (1959: 137n.) credits him with having used the technical term 'generates' to denote the relation between an enumeration procedure and the enumerated set (see *LFS*: 64, 169–170).

That citation of Post (1944) as the source for 'generates' appears to be the only actual bibliographical citation of Post that Chomsky ever made. The passing mentions of Post in Chomsky (1962: 539) and (1965: 9) are not accompanied by citations. Chomsky nowhere mentions Post or references his work in *LSLT*, though he does cite Rosenbloom (1950), which was strongly influenced by Post (see especially Rosenbloom's chapter IV). This may mean that Chomsky got his understanding of Post mainly via Rosenbloom. The fact is that Chomsky never cites the papers in which Post makes the contributions that are most relevant to TGG: the ones applying his formalisation of deduction to general combinatorial problems about strings.

Post's 1920 doctoral dissertation, cut by a third to produce the published version in Post (1921), was devoted to providing a fully mathematicised treatment of the deductive system of propositional logic employed in Whitehead & Russell (1910–1913). Part of this involved devising a kind of rule system for manipulating uninterpreted symbol strings, one which would be general enough to embrace any imaginable deductive consequence relation (see 1921: 276). This involved the formalisation step in that it mechanically mapped logical propositions to meaningless strings of symbols; it was formal in the sense that no semantic properties of propositions were relevant to what the system did with their corresponding strings; and it was a kind of Formalism in that it was aimed at establishing consistency proofs, not for mathematics but for Whitehead & Russell's propositional calculus. It would thus have been essential material for Tomalin, given his aim of linking Formalism to the origins of TGG, but he overlooks the whole of Post's main body of work.

Taking up his work twenty years later (after a long period of illness), Post (1943) defined a 'canonical production system' over a finite symbol alphabet *A* as a set of initially given strings defined over *A* (the 'primitive assertions') and a set of rules (he called them 'productions') for deriving further strings. A rule, very much like a (generalised) transformation in early TGG, consists of a structural description and a structural change. The structural description is a set of patterns of the form $g_0P_1g_1P_2g_2 \ldots P_kg_k$ against which symbol strings are to be matched. The *g*s are given strings that must be matched by copies of themselves, and the *P*s are free variables over strings that can be matched by any substring. A set of such patterns 'produces' a further string, defined by an analog of the structural change of a transformation: another sequence of strings and variables in which all of the free variables are copied from those in the structural description. A canonical production system is said to generate the set of all and only those strings that can be produced through iterative use of its rules.

What these papers reveal is that it was Post who invented rewriting systems. Linguists appear to have overlooked this point, but theoretical computer scientists have not; see the very clear discussions in Brainerd & Landweber (1974: 159ff.) and Kozen (1997: 256ff.).

Post also did the first work on generative power. The extremely general canonical systems of Post (1943) were equivalent to Church's lambda calculus and to Turing machines. Post defined a NORMAL production system as a very restricted kind of canonical system, with only one initial string, only one pattern in a structural description, only one variable in a pattern, and rules limited to the very spare form 'g_0P_1 produces P_1g_1' – more simply, '$xZ \rightarrow Zy$' where *x* and *y* are specific strings and *Z* is a variable over strings. Each such rule removes the designated *x* from the beginning of the string and adds *y* at the end. Post proved by a succession of reductions that normal systems can define any set of strings over an alphabet *A* that canonical systems can define, provided only that certain extra symbols not in *A* are permitted to appear in the strings manipulated by the rules. These extra symbols are what linguists now call non-terminals, or syntactic category symbols. (The free string variables are different: they appear only in rules. The extra non-terminal symbols appear in intermediate strings in derivations, though not in the strings ultimately counted as belonging to the language.)

The differences between Post's systems and the TGGs of Chomsky (1957) lie mainly in the additional devices that Chomsky assumed, like rule ordering and obligatory application; but these turn out neither to restrict nor to enhance generative power. Post had, in effect, already proved that restricting transformations to the form '$xZ \rightarrow Zy$' did not reduce the generative power of a grammar (and thus that 'Is string *w* generated by grammar *G*?' is not in general decidable – a partial anticipation of the later result of Peters & Ritchie concerning the 1965 version of TGG).

There is another simplified form of rule which Post also showed did not reduce generative power: a system in which all the rules are of the form '$ZxW \rightarrow ZyW$'. Post (1947) called this a rule of 'semi-Thue' type. The Norwegian mathematician Axel Thue (1914) had introduced bidirectional rules of the form '$ZxW \leftrightarrow ZyW$', meaning that ZxW and ZyW may be substituted for each other (equivalent to a pair of rules containing $ZxW \rightarrow ZyW$ and $ZyW \rightarrow ZxW$), and had posed the question of finding a general method for solving problems like 'Can the rules convert string *v* into string *w*?' for arbitrary *v* and *w*. Post (1947) settled the question by proving that there was no such method. In fact he proved a stronger claim: the problem is undecidable even for unidirectional replacement rules (like $ZxW \rightarrow ZyW$).

The semi-Thue type of rule is of particular importance, because Chomsky (1962: 539) directly acknowledges that '[a] rewriting rule is a special case of a production in the sense of Post; a rule of the form $ZXW \rightarrow ZYW$'. The type-0 rules in the classification of Chomsky (1959) are simply semi-Thue rules.

Thus, more than ten years before *Syntactic Structures*, Post had defined rewriting systems of a very general sort (canonical systems) and had shown that neither limiting them to normal systems ($xZ \rightarrow Zy$) nor limiting them to type-0 rewriting rules ($ZxW \rightarrow ZyW$) altered their Turing-equivalence.

In short, Post's role in developing the methods used in early TGG has been underestimated. This was an important topic for Tomalin's project, but because of his reliance on Chomsky for bibliographical references, he never came upon the relevant papers.[5]

6. Conclusion

As mentioned above, Tomalin sees his accomplishment in the following terms (186):

> . . . perhaps the main achievement of this book has been to associate TGG with both Formalism and Logicism, two intellectual movements that profoundly influenced scientific methodology in the early twentieth century. Indeed, this general issue seems to have been the single destination towards which the various paths of enquiry have led. With its focus on syntax instead of semantics, with its use of a logic-based notational system, with its identification of the analogy between a proof and the generation of a grammatical sentence, and with its use of such procedures as recursive definitions and axiomatic deduction, TGG unambiguously reveals its associations with the formal sciences.

But Tomalin has not associated TGG with the substance of the programmes of Formalism and Logicism. One cannot base a substantive 'association' between early TGG and these long-discarded movements in the philosophy of mathematics on the basis of nothing more than some shared methods. All of these methods were used in pre-Chomskyan linguistics. Euclid used postulates, but that does not associate geometry with Bloomfield (1926). What Chomsky drew on that was new to linguistics was the tool of Post-style production systems; and that is a development Tomalin says nothing about.

The key point about the programmes of Formalism and Logicism is that they were crucially bound up with the task of proving logical consistency and completeness. This task simply does not arise in TGG. Chomsky was not trying to rescue linguistics from paradoxes, or to prove consistency of linguistic theories. The notion of an inconsistent TGG does not make sense. In logic there is a distinction between well-formed formulas and theorems, and in linguistics there is not. In a consistent and complete logic, each formula p has an anti-formula $\sim p$, its negation, which must be provable if and only if p is not. Intuitively, a logic is inconsistent if and only if some provable assertion has a provable negation. There is no analog in natural languages. The strings of formatives generated by grammars for natural languages do not have negations in the relevant sense. (Within English, *Jesus wept* is regarded as having *Jesus didn't weep* as its negation, but the whole point is that the grammar SHOULD generate both of those, not that this should be avoided. In logic each formula has a negation which must NOT be derived, i.e., proved; the analogy between generating and proving breaks down at this point, and to use 'deriving' for both is just a pun.) What Gödel showed concerning the Hilbert Programme was that it could not succeed, because completeness and consistency could never be attained together within a given system that was powerful enough to express arithmetic. Nothing comparable could ever be done to TGG, because notions of completeness and consistency do not arise there.

Tomalin concludes with an odd leap from 1957 to the present day. He discusses (pp. 198f) two conflicting claims about whether TGG is a science. One is negative: Paul Postal's view that 'the principles and accomplishments touted in recent years are almost embarrassing in their inadequacy and shoddiness' (quoted by Huck & Goldsmith 1995: 141f.). The other is positive: Massimo Piattelli-Palmarini's claim that generative grammar is 'well on its way to becoming a full-blown natural science whose idealisations, abstractions, and deductions will eventually match in depth and subtlety those of the most advanced domains of modern science' (from the introduction to Uriagereka 1998: xxv). Tomalin thinks that this implicit disagreement can be resolved by answering a historical question, and that *LFS* answers it. His idea is that any theory presented in terms of the axiomatic method is a formal science, hence a science; and Piattelli-Palmarini stresses 'idealizations, abstractions, and deductions' as if these are what science strives for. But it is more than a little naive to think that a history of theory FORM in linguistics will be able to settle the disagreement between Postal and Piattelli-Palmarini. What matters for Postal is the CONTENT of linguistic theories, not just their form. Postal's point is that the referees for linguistic journals should be like employers in the private sector as characterised in Dan Ackroyd's immortal line from *Ghostbusters*: 'They expect results.'

Another general problem that afflicts *LFS* has to do with the fact that it discusses the work of Chomsky, which illumines the linguistics of the second half of the 20th century with such brilliance that those who attempt to write the history of the period often seem to be blinded by the light. Like others before him, Tomalin has followed Chomsky too closely and uncritically at many points in his research.

- Because Chomsky does not cite Carnap, Tomalin does not appreciate the extent to which Chomsky follows Carnap in his foundationalism.
- Because Chomsky mentions recollecting a critique of induction by Goodman, Tomalin thinks Goodman must have been sceptical about induction, which he was not.
- Because Chomsky regards Bar-Hillel (1953) as merely an advocate of applying logic to natural language, and Hockett (1955) as simply an example of a model of grammar limited to finite-state expressive power, rather than seeing both as early advocates for types of generative grammars, Tomalin does too.
- Because Chomsky asserts that no probabilistic account could distinguish stark ungrammaticality from mere incoherence, and cites no work on probabilistic methods, Tomalin follows suit.

- Because Chomsky never cites Post's key technical papers, Tomalin likewise overlooks them, missing the crucial fact that Post invented the machinery of generative grammars and proved the first theorems relating rule form to weak generative capacity.

In general, though, linguists and philosophers interested in the history of generative linguistics will find that there is much to be learned from Tomalin's book, despite the fact that it does not complete its historiographical task, it surveys its sources too superficially, it assigns too much importance to the form of theories (as opposed to the claims that are put into that form), and its expository scope is unintendedly limited by following the bibliographical materials and opinions of its leading figure much too closely. It is imperfect, but we nonetheless think that everyone interested in the history of 20th-century linguistics should read it. We are certainly glad that we did. We learned things we didn't know, confronted issues we had not been thinking about, and were led to literature that we had long neglected.

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FOOTNOTES

1 We are very grateful to two anonymous *JL* referees whose comments led to our making significant revisions to this article. Conversations with Dan Everett, Jeff Pelletier, and Stewart Shapiro have also been extremely helpful.

2 Here and elsewhere we correct small errors in Tomalin's quotations by comparing them with the originals. In no case does this alter the sense.

3 Publication data for works that we mention in passing but do not make specific reference to can in all cases be found in Tomalin's bibliography.

4 Tomalin misspells Twardowski's name throughout as 'Tawardowski'.

5 Tomalin also follows Chomsky in a small mathematical error. He quotes from Chomsky (1959: 137) the claim that, where L is some language (i.e., a set of strings), '[a] grammar of L can be regarded as a function whose range is exactly L'. This cannot be right: every set has a function whose range is exactly that set (the identity function, for example), yet most sets are not computably enumerable and thus do not have grammars. And TGGs are not functions anyway. Tomalin quotes the remark, but does not notice the error.

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