Through a glass darkly:
a critique of the influence of linguistics on
theories of music

Edward McDonald

Abstract

If music is treated as a kind of 'language', then it makes sense for musicology to borrow
from linguistics in order to define exactly what sort of a 'language' music is. However,
not only does this avoid the challenge of defining music on its own terms, it also
brings across a whole lot of unnecessary historical baggage. Through a close analysis
of selected texts, the current article critiques some recent work in musicology, showing
how it is based on a far too narrow understanding of what constitutes linguistics, i.e.
basically formal linguistics. It points out some of the dangers of interdisciplinary work
such as Brown's (2001) 'musilanguage' model, and shows how a less than careful bor-
rowing of linguistic concepts can vitiate the usefulness of such system-building. It then
traces the source of much of this borrowing in Lerdahl and Jackendoff's (1983) highly
influential work drawing on generative linguistics, and shows how a framework that
privileges structure over meaning, system over text, and the cognitive over the social,
is unable to provide a broader understanding of music beyond pattern recognition. It
calls for a greater methodological scepticism among musicologists towards linguistics,
and a greater self-consciousness about borrowings across disciplines.

Keywords:  music, language, theory, musicology, linguistics, structure

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1 The ‘language of music’ and the linguistics of musicology

The ‘language of music’: seldom has such a simple phrase given rise to such wide-ranging confusion. Does it represent an analogy? ‘music is like language’ (and if so in what ways and to what extent); a classification? ‘music is a type of language’ (and if so is their relationship one of sisters, or one of mother and daughter); or a metaphorical yoking together of two phenomena of which the latter represents a sort of mysterious transcendence of the former? ‘You language where all language ends’, in Rilke’s words (quoted in Chanan 1994: xi).

One source of the problem, to get back to semiotic basics, is that the expression plane of both – sound, or more specifically the embodied human voice – is the same, and often used in such similar ways that it is difficult to distinguish between the two; while their respective interpretation planes seem to lead us in completely opposite directions: to oversimplify a common drawn dichotomy, music straight to feeling, and language to thought.

Another major contributor of confusion has been the academic extension of the ‘language equals music’ equation into a further, at least partial, identification of linguistics and musicology. This identification, which has a long if rather inglorious tradition – whether on the musicological side, as in Derryk Cooke’s The Language of Music (1959) or Leonard Bernstein’s The Unanswered Question (1976); or from the linguistic side, as in Lerdahl and Jackendoff’s A Generative Theory of Tonal Music (1983) or van Leeuwen’s Speech, Music, Sound (1999) – is nicely encapsulated in Joseph P. Swain’s recent work Musical Languages (1997). In Swain’s own summary formulation, the main question of the book is ‘How is music like language’ – to which he adds the cautionary footnote ‘and so what if it is?’ – but his immediately following explanatory statement shows that the transfer is in fact being made from linguistics rather than from language as such:

…[this] is a book that shows how deeply all we have learned about spoken language from modern linguistics and cognitive psychology can explain our experience of music, and thereby seeks to revive and renovate the analogy of music and language, to make the ubiquitous expression ‘musical language’ seem fresh, precise, intellectually respectable, and useful for the critical writing in which it most often appears. (Swain, 1997: ix)

2 Linguistic background

This may seem to be splitting hairs: if linguistics is the study of language, surely when we attempt to apply the linguistic analogy to music, the first place to go is the theory of language, viz. linguistics? Well, yes and no. Unfortunately, there is no one ‘theory of language’ to go to, although this is something either
unknown or ignored by most scholars coming from outside linguistics, such as the musicologist Swain, or the psychologist Steven Pinker, who has written two popular works on language, *The Language Instinct* (1994) and *Words and Rules* (1999).

Over the last two centuries, the discipline of ‘linguistics’, earlier known as ‘philology’, has gradually broadened its scope to deal with all the varied phenomena involved in linguistic ‘performance’, broadly speaking. Scholars in the Western tradition, with significant input from traditional Indian and Chinese scholarship, first came up with what were in effect theorisations of the insights already embodied in the orthographies of the languages they studied: with the concept of minimal unit of sound, the phoneme (earlier called ‘letter’) and theories of phonology, and of the minimal unit of word- ing, the morpheme and theories of morphology (earlier called ‘accidence’). Going beyond this to areas largely ignored by orthography, they moved on to patterns of word combination, in the study of syntax, and of the pitch and rhythmic aspects of speech, in intonation and prosody. More recently there has been a great flowering of research into the contexts in which linguistic behaviour takes place, in theories of social context and of the cultural and ideological dimensions of language use.

Standard histories of linguistics tend not to regard the last two centuries in terms of this steadily broadening scope. Most, indeed, prefer to use a more Kuhnian model of paradigm change, with significant shifts in the earlier decades of the twentieth century, associated with the work of Saussure; and then again just after mid-century in the work of Chomsky. This second so-called ‘revolution’ is often regarded as making all previous developments redundant. The distinguished American linguist Derek Bickerton is merely repeating a widely held view when he remarks, in a recent volume on *The Origins of Music* (Wallin et al., 2000), that ‘[w]e have found out more about human language in the last thirty years than we did in the preceding three millennia’ (2000: 154); although from a historical point of view, what Bickerton implies to be ‘[the] only one among competing theories [which] will be compatible with the entire range of constraints’ identified by research on language within and beyond linguistics, is in fact only one narrow and inward-looking school of linguistics which for a multitude of reasons, ideological and political not least among them, has come into prominence over the last half century.

In fact the school of thought inspired by the work of Noam Chomsky – initially known as ‘transformational generative grammar’, then after transformations fell out of fashion, ‘generative grammar’, and often these days, with conscious or unconscious arrogance ‘theoretical linguistics’ – represents only one strand in thinking about language in the last hundred or so years. To mention only English-language scholarship, the names of the American
linguist Edward Sapir, who took his concern with the patterns of language in the direction of psychology and culture (Sapir, 1949), or the British linguist J. R. Firth, who made original and radical connections between language and social context (Firth, 1950), are known nowadays mostly only among linguists, and often there only second hand; but either of their theories would have proved a far more insightful basis for a theory of music than Chomsky’s. Many of Chomsky’s basic theoretical concepts, as we will see below, derive specifically from Bloomfieldian descriptive linguistics, also known as American structuralism, in the specific form given it by Chomsky’s first teacher of linguistics, Zellig Harris, and the fact that Chomsky and his followers only selectively acknowledge their theoretical debts, particularly to their immediate predecessors, only obscures this fact.

The basic relationship that any linguistic theory has to deal with, that between sound and meaning in language, is largely, as Saussure pointed out ([1916] 1959: 67), an unmotivated one: that is, there is no necessary relation between particular meanings and the (combination of) sounds used to express them. There is thus between phonology and syntax in language what, following Saussure, may be called a line of ‘arbitrariness’, and it is this feature which allows a limited set of sounds to realise what is in principle an unlimited set of meanings. However, despite Chomsky’s claims for an ‘autonomous syntax’, that is, that a similar ‘line of arbitrariness’ separates syntax and semantics, there is no a priori reason to assume that this should be the case: syntactic categories are most easily understood as an abstraction from semantic categories (Matthews, 1981: Chapter 1). In the case of phonology, while Chomskyan linguistics, under the influence of the linguist Roman Jakobson, has long assumed that phonological distinctions rest firmly on the ground of phonetic, specifically acoustic, naturalness, it exhibits a reluctance to deal directly with the relation between form and meaning, and therefore attempts to explain syntactic forms without appealing to semantic distinctions. This reluctance can be traced back to one of the founding figures of American structuralist linguistics, Leonard Bloomfield, whose methodological caution in holding off on questions of meaning perhaps made sense at the relatively undeveloped stage of the discipline in the 1920s and 1930s; but since Chomsky elevated it into a matter of theoretical principle from the foundation of his paradigm, the problem of how to ‘bring meaning back in’ after having thus unceremoniously thrown it out, has bedevilled Chomskyan linguistics ever since.

The paradoxical and rather disturbing outcome of this situation is that when musicologists like Swain set out to apply a linguistic model to the study of music, the model they apply is Chomsky’s formalist, mentalist interpretation
of only one rather extreme strand in the linguistic tradition of the last two centuries. The sorts of linguistic presuppositions Swain brings to his examination of music are hinted at by his chapter headings, where it is clear that he is working with a three-level model of language in which, by a typical linguistic pathology of English, the phenomenon studied and its study go by the same label: semantics; syntax; phonology.

The thinking that this reflects is a modular one, whereby the two basic observables of sound and meaning are put into the two carefully separate and discrete boxes of phonology and semantics, while their interface is seen as a set of abstract forms whose investigation can be carried out simply on the basis of their possible ‘combinations’ (the literal meaning of ‘syntax’, this metaphor being taken to its extremes in Chomskyan formal syntax), and which, by a separate step, is taken as the central focus of study.

Once these three modules have been defined, of course, we can study their ‘interaction’ – phonology with syntax, syntax with semantics – and then investigate how the whole stack of boxes is related to its material expression on the one hand – thus the separate areas of phonetics and acoustics – and its use in actual contexts on the other – thus pragmatics. Along with this modularisation, where the different phenomena focused on are studied by very different methods and in quite distinct steps, goes a concentration on the system which enables linguistic behaviour, and a comparative lack of interest in the texts which are a manifestation of that system: in Chomsky’s terms, a focus on competence rather than on performance (1965: 4).

Now while such a model of language claims direct relevance to cognitive psychology – in effect, it sets out to represent ‘what is inside the head’ of language users – in actual application it is fraught with problems. Chomsky’s claim for language as a cognitive faculty *sui generis*, while it does not seem to be a necessary feature of his competence/performance split, and indeed is openly repudiated by the psychologist Pinker in his application of the model to evolutionary questions, hypostatises it as an independent entity which has no links to other cognitive faculties, is learned by way of a discrete ‘language acquisition device’, and which in the history of the species developed *ex nihilo* in a sort of blinding cognitive flash (I exaggerate only slightly).

We may thus feel justified in being more than a little sceptical of Swain’s claim that ‘modern linguistics’ and ‘cognitive psychology’, the latter in its modern form heavily influenced by Chomskyan preconceptions, are our best guides for explaining our ‘experience of music’. In Swain’s own summary:

Sound over meaning. Mutual interference of notes, syntax, semantics and community. Context shaping all. The essential components of a music language… (Swain, 1997: 176)
A framework that turns the basic semiotic complementarity of sound and meaning in music into a dichotomy, and relies on 'mutual interference' between what the theory represents as mutually incommensurable phenomena, is perhaps going to be less than useful for understanding the complex relations between sound and meaning in music, let alone in relating musical organisation to wider issues of musical perception and cognition, as well as use and social context. I plan to go on in another paper (McDonald, in preparation) to explore other schools of thought in linguistics, musicology, psychology and philosophy that can provide alternative insights into the nature of these two very complex and still only partially understood phenomena of language and music. In the meantime, I would like to concentrate on the formalist tradition in linguistics which has hitherto had the greatest influence on the formulation of theories of music.

3 Interdisciplinary pitfalls

In examining what I see as the largely deleterious influence of currently influential theories of language on music, a recent collection called *The Origins of Music* (Wallin et al., 2000) provides us with a useful starting point. Useful partly because it is recent, and therefore can be expected to give us clues as to the current consensus, but also because of its interdisciplinary focus, which thus should show up some of the difficulties of traversing disciplinary boundaries, one of the main points of the present discussion.

The first paper I will look at here is one by Derek Bickerton entitled ‘Can biomusicology learn from language evolution studies?’, a title whose question form is very apposite. Professor Bickerton, already quoted from above, is an eminent linguist widely known for his work on pidgins and creoles, from which research he has drawn general principles for the understanding of both language development and language evolution. (A good accessible introduction to his views on the emergence of language from ‘protolanguage’ can be seen in his ‘dialogue’ with the biologist William Calvin, *Lingua ex Machina*, 2000.) Bickerton works within a specifically Chomskyan version of generative linguistics, and his empirical findings have been hailed as providing hard evidence for the sorts of linguistic universals posited by Chomsky and his colleagues.

Bickerton’s paper presents a sort of cautionary tale for students of music evolution, warning them not to go down the sorts of theoretical blind alleys already traversed by linguists, as well as making a plea for greater interdisciplinary cooperation. As he points out in a striking metaphor:

During the nineteenth century, human behaviour was divided up by the disciplines of the day in much the same way as Africa was simultaneously
being divided up by the colonial powers. No surprise, the boundaries of these disciplines were often determined as arbitrarily and as illogically as were the boundaries of colonial possessions. Consequently, many contemporary researchers, like many contemporary African states, remain trapped within their own history. They limit themselves to meeting those constraints on possible theories that are imposed by their own particular discipline, completely ignoring the often more rigorous constraints imposed by others. (Bickerton, 2000: 154)

There are already some problems with Bickerton's stance here. The notion of 'rigour' is not easy to define, particularly interdisciplinarily, where one theory's 'rigour' very easily becomes another one's 'rigor mortis'. Moreover, from the point of view of the present study, intra-disciplinarily, Bickerton shows what seems like a fairly 'complete ignorance' of his own, as well as a serious lack of historical perspective, in relation to the range of theoretical frameworks available in the study of language. He opens with the rather complacent claim quoted above, i.e. 'that we have learned more about language in the last thirty years...' – a claim which has become almost ritualistic in certain circles, but which is more often stated than justified – seemingly unaware that a similar self-congratulatory mood was widespread in American linguistics in the 1950s, just before Chomsky came along to problematise the whole basis of the field. Whatever one makes of such claims (see Newmeyer, 1980, for a partisan view from within generative linguistics, and Hockett, 1968, for a more measured, if highly critical, historical perspective), and how exactly one would go about proving them, the historical parallels with 1950s American structuralism should perhaps serve as a salutary warning.

More damagingly, however, Bickerton's claims about what 'we' have 'discovered' about language in these last three decades show that not only does he seem unaware of how the theoretical premises of generative linguistics, like any theory, limit the phenomena which will be recognised as significant, but he also seems ignorant of the many alternative views about language that are being actively explored within linguistics and related disciplines. Firstly, Bickerton's contention that '[w]e can now be sure that all human languages share a number of nonobvious characteristics, and that these characteristics derive directly from human biology' takes as a broad consensus – who exactly does 'we' refer to? – something that is still very much a debatable, and fiercely debated, point (Elman et al., 1997; Sampson, 1999). For example, the 'nonobvious characteristics' touted by Bickerton could very easily be seen as a result of the tendency in generative linguistics to posit universal linguistic features of a high degree of abstractness, something in a sense forced on it by its insistence on taking structure as the basic variable, rather than any sub-
stantive characteristic of language as such. Bickerton criticises Greenberg's universalist programme (Greenberg, 1963) of identifying tendencies in the 'linear ordering of constituents' as dealing with 'mere.. surface similarities' (2000: 155) between languages, but it does not occur to him that the notion of 'constituent' itself, which is what his 'nonobvious characteristics' are largely based on, might perhaps be the wrong site to look for linguistic universals in the first place.

If we go on to list the main points of this supposed consensus, we can very clearly see the sources, as well as the limitations, of Bickerton's views:

- his identification as the correct approach to linguistic universals being that of Chomsky (1965) 'which seeks to analyse language at a deeper level and uncover highly abstract properties that all natural languages share' (2000: 155–6);
- his suggestion, again from Chomsky (1980), that 'language … may be decomposable into two components, conceptual and computational' (2000: 156);
- the related claim that what separates human language from protolanguage, the latter 'potentially sharable with other species', is a 'syntactic mechanism [with] a complex hierarchical, parsable structure' (2000: 156);
- his claim, again based on Chomsky's work (1981; 1988), that 'differences [between languages] are relatively trivial, and that the deeper principles that underlie them are shared by all language without exception', these principles having 'little in common with general principles of thought or other apparatuses that might be attributed to the human mind' (2000: 158);
- the related 'fact' that 'those principles can be expressed most successfully not as a set of positive admonitions but rather as a set of constraints on otherwise unlimited potentialities' (2000: 158).

Anyone familiar with the body of literature on generative linguistics will recognise these so-called 'features of language' as, in fact, 'features of linguistic theory', in the specific form taken by Chomsky's 'minimalist' version of generative grammar (see Radford, 1997, for an accessible and up-to-date introduction). An abstract syntax as the core of language, a clear split between 'concepts' and 'structures', hierarchical constituency relations as the main model for language structure, universal structural principles underlying all languages which have nothing in common with other cognitive faculties, the expression of these principles as a set of constraints – all these are features of generative grammar as it is currently put forward.
I must not be understood here to be criticising Bickerton’s proposals simply because they happen to differ from those of other schools; any theory embodies in its own theoretical categories a view of the phenomena which it sets out to describe: this is an inescapable condition of all theorising. However, what is disturbing about Bickerton’s ‘features of language’, apart from the suspicion that they are constructs of a particular bias of the theory towards ‘structure’ over ‘meaning’ as the main goal of explanation – a bias which can be shown to have clear historical roots – is how little attempt is made to distinguish between the phenomenon itself and the theoretical characterisation of that phenomenon, even though we have no access to the former except through the latter. In other words, by what was referred to above as a ‘linguistic pathology’ of English, not only is ‘syntax₁’, the patterns of meaningful elements in a language, equated with ‘syntax₂’, the study of those patterns (Chomsky, 1965: 25) acknowledges a similar ‘systematic ambiguity’ in his use of the term ‘grammar’), but by a further metalinguistic pathology, ‘syntax₂’ is understood monistically as ‘Syntax’, i.e. one particular theoretical claim as to how that study should be conducted.

When Bickerton complains, for example, that the ‘certainty’ that ‘all human languages share a number of nonobvious characteristics’ is ‘obscured by mainly terminological disputes between holders of rival but largely equivalent theories’ (2000: 154), he displays a breathtaking ignorance of the many highly influential and long-developed theories of language that are far from being ‘largely equivalent’ to generative linguistics. To mention only two: the Prague School (Vachek, 1966) has made a major contribution to the understanding of phonology in linguistics generally, and continues to make a contribution, through its notion of ‘functional sentence perspective’, to the study of syntax broadly conceived. Closer to home for Bickerton, the ‘neo-Bloomfieldian’ linguist Charles Hockett, whose The State of the Art (1968) is a devastating attack on the philosophical bases of generative linguistics (which, however, few generativists seem to have read), has provided in his Refurbishing our Foundations (1987) a summing up and critique of the preoccupations of American structuralist linguists to the present day, with whom Chomsky shares much more than he is willing to acknowledge, particularly if you compare American with European schools of linguistics.

If Bickerton’s article is both a warning against and, ironically, a demonstration of theoretical blinkeredness, the second article I will examine here, Steven Brown’s ‘The ‘musilanguage’ model of music evolution’, exhibits an in some ways more sophisticated but equally unsconscious confusion of theoretical claims with observable phenomena. If we look at Brown’s article from the same point of view as the work I briefly examined at the beginning of this paper, Swain’s Musical Languages, we can see how difficult it is to free oneself
of unexamined theoretical presuppositions, particularly when taking over concepts from another field.

Brown starts off by criticising studies such as Swain's for being confined to the level of 'metaphor making', pointing out quite justly that '[c]oncepts such as musical language ... and speech melody are never taken beyond the domain of metaphor into the domain of mechanism' (2000: 272). However, it is in Brown's attempts to characterise this 'mechanism', on the hard-to-quarrel-with assumption that 'music and language have strong underlying biological similarities in addition to equally strong differences' (2000: 272), that we see the effects of over-simplistic and undigested borrowings from linguistics.

Brown decides to attack the problem of the 'similarities that exist between language and music' by way of a 'metaphor [that] is quite pervasive in musicology', that of 'grammar' (2000: 272). While warning that 'the notion that musical phrase structure (can) have a hierarchical organisation similar to that of linguistic sentences ... must be viewed as pure parallelism', he nevertheless claims that there are 'important underlying features' which support the use of the grammar analogy for music, and that these features 'provide a biological justification for this potential for hierarchical organisation in music' (2000: 272–3).

What Brown does not seem to realise is that this basic opening gambit is one specific to generative accounts of linguistic patterning, presented in its classic form in Chomsky's first major work, *Syntactic Structures* (1957), and which can be traced back through Chomsky's teacher, Zellig Harris, to one of the founding fathers of American structuralism, Leonard Bloomfield. In other words, far from being a value-free claim, if such a thing is possible, it is deeply embedded in the particular view of language description known as distributionalism. This methodological concept, most comprehensively applied in Harris's *Methods in Structural Linguistics* (1951), but already foreshadowed in Bloomfield's *Language* (1933), holds that the most useful way of capturing the structural regularities of linguistic units is in terms of their patterns of mutual distribution: in other words, the possible combinations of elements with each other. The notion of 'phrase structure' referred to by Brown above is one formalisation of this concept, which in its canonical form is expressed as 'rewrite rules' of the type $S \rightarrow NP + VP$: i.e. a sentence consists of, or can in the first instance be divided into, a noun phrase and a verb phrase. The notion of distribution is in turn predicated on the supposed necessity of describing language forms without reference to meaning, another Bloomfieldian preoccupation accepted without question by Chomsky, but one which is by no means an obvious requirement in the analysis of language.

While this move of Brown's does not represent a real problem, since musical structure must be accounted for somewhere in the model, a more double-
dyed error reveals itself in his distinction between the ‘acoustic’ and ‘vehicle’ modes of music perception. According to Brown, these represent ‘two different modes of perceiving, producing, and responding to musical sound patterns’: the acoustic being the ‘immediate, on-line, emotive aspect’; and the vehicle, the ‘off-line, referential form’ (2000: 272). While on its own this distinction is, in effect, simply a rewording of the commonsense interpretation of music and language as most directly connected to ‘feeling’ and ‘thought’ respectively, it then leads Brown into a distinction between ‘combinatorial syntax’ vs. ‘intonational phrasing’ that betrays further unexamined assumptions.

As I noted above, much linguistic work in the European tradition over the last two centuries could be viewed as a theorisation of principles embodied in the alphabetic orthographies of European languages. While the concept of the ‘phoneme’, or minimum unit of sound, is considerably more sophisticated than that of ‘letter of the alphabet’, it is quite clearly based on similar principles, which, historically speaking, linguistics owes to orthography, not vice versa. One of the effects of what we may call the ‘alphabetic principle’ can be seen in the common distinction in linguistics between segmental phonology, that which can be represented by an alphabetic orthography like that used for Italian, or the International Phonetic Alphabet (IPA); and the suprasegmental phonology of intonation and rhythm, which most orthographies represent partially if at all, Spanish orthography being one of the most complete in this regard. One of the few twentieth century phonological theories to escape from the bonds of the alphabet was the prosodic phonology of J. R. Firth and his colleagues of the so-called London School, significantly based at the School of Oriental and African Studies (SOAS) at the University of London. Linguists of the London School did much descriptive work on African and Asian languages, as well as quite explicitly deriving theoretical principles from the highly sophisticated linguistic traditions of those regions, particularly ancient India and China, and were very sensitive to the dangers of simply accepting the theoretical viewpoint embodied in the use of an alphabetic script.

In this context, not only the source but also the highly debatable nature of Brown’s distinction becomes clear. A ‘combinatorial syntax’ by which ‘phrases are generated through combinatorial arrangements of … unitary elements’ (2000: 273) seems very much like the process of building up words out of letters of the alphabet, or phrases out of musical notes: an emphasis on a basic level of ‘segments’ that clearly reflects the influence of these two writing systems. The secondary level of ‘intonational phrasing’, consisting of ‘modulation of the basic acoustic properties of combinatorially organised phrases’, looks for all the world like the placement of punctuation marks around certain combinations of words, or the putting of a musical slur over a group of notes: something
that both those writing systems also signal as secondary, even omissible. If we think back to the sound phenomenon of music to which all of this is supposed to relate, it is not immediately obvious why we should treat certain ‘acoustic properties’, essentially pitch levels, as ‘basic’, while reserving a secondary status for ‘modulation’ of these ‘properties’ by intonational-type contours – unless, of course, we are prepared to accept the implications of our orthographies, both linguistic and musical, without question.

Indeed the whole notion of pitch ‘level’ as opposed to ‘contour’ plays such a major role in Brown’s theorisation of the common ancestor of music and language which he dubs ‘musilanguage’, and is moreover such a paradigmatic example of the confusion of theoretical postulates with observable properties, that it warrants further exegesis here. Brown constructs an elaborate, but essentially quite simple, argument for the necessity of what he refers to as the ‘creation of a tonal system based on level tones (discrete pitch levels)’ (2000: 279): the sting here is in the parenthesis, an example of a dangerous type of ‘semantic slippage’ which I will come back to later.

Brown prefaces his argument by the claim that although ‘tonal languages … tend to be viewed as oddities by linguists’ (not to this linguist’s knowledge!), a ‘majority of the world’s languages’ are in fact tonal, and therefore the ‘most parsimonious hypothesis is that language evolved as a tonal system from its inception’ (2000: 280–1). Oddly enough, he fails to mention the fact that one of the highest concentrations of tonal languages is in sub-Saharan Africa, where the human species is commonly claimed to have originated, a fact that would seem to support his hypothesis. Even more oddly, although he bases his whole argument on the notion of ‘pitch levels’, he does not cite any examples of such languages, for which again sub-Saharan Africa provides the classic examples. The examples he does cite, and dismisses for his purposes, are those of ‘pitch accent languages’, where tone is not a feature of every syllable, but rather functions more like stress.

In order to understand Brown’s argument, we need to briefly review the use of pitch in the languages of the world. First there are those languages in which pitch is used contrastively in the context of the word and/or phrase, including not only ‘pitch accent’ languages like Swedish, cited by Brown, which contrast high and low word accents, but also ‘stress accent’ languages like English, where pitch is still one of the major components of accent, but is not tied to a particular register. Second are those languages widespread in sub-Saharan Africa, such as Zulu, in which every syllable is in either a high or low register of the voice – what are usually known as tonal languages proper. (Crystal, 1997, gives three examples of tonal register systems in African languages: high, low (Zulu); high, mid, low (Yoruba), and extra-high, high, mid, low (Lushai).) Third, really a subset of the second, are those languages widespread
in South-East Asia, such as Thai or Chinese, which employ pitch contours, usually in conjunction with pitch registers. Thus the Wu ‘dialect’ of Chinese, also known as Shanghainese, has three basic contours – level, rising, falling, plus a fourth which is really an absence of contour for syllables with final stop consonants – in combination with two registers, high and low. (Interestingly, and perhaps unfortunately for Brown’s argument, both the contours and the registers have been shown to be a later historical development from a pre-tonal phase of the language, with the contours deriving from certain final consonants, and the registers from sets of initial consonants, voiceless for high, and voiced for low.)

It is at this point that the effect of the ‘semantic slippage’ mentioned above becomes pernicious. Brown slides imperceptibly between ‘pitch levels’ – in the paradigm case mentioned by Brown ‘high’ vs. ‘low’ – and ‘level pitches’ or ‘level tones’, which would seem to be equivalent to the more or less discrete pitches of many, though by no means all, musical traditions. This simple transposition of an adjective and a noun equates, or seems to equate, two very different phenomena, and thus casts doubt on the whole of his subsequent argument.

It is crucial for Brown’s argument, though not as far as I can see for the musilanguage model as such, that the primaeval form of language should be a tonal language of the second type. Firstly because it seems to make the analogy between ‘tones’ in language and ‘tones’ in music closer: in fact, the sort of linguistic ‘tones’ Brown is talking about are better characterised as ‘registers’, i.e. specific parts of the vocal range, and there is no need for these to be ‘level’ as such. And secondly, because in a similarly unreflective way to Bickerton’s argument, the ‘theory’ says so. The theory in this case is that of ‘autosegmental’ phonology (Goldsmith, 1976), which from a historical point of view has largely rediscovered some of the insights of prosodic phonology of 30 years earlier (Firth, 1948; Robins, 1957). ‘Historically’, as Brown remarks with unconscious irony – the only ‘history’ that matters started in 1957! – ‘there has been a long standing debate in [generative] phonology between a so-called levels perspective and a so-called configurations or contours perspective’. The fact that, according to Brown, ‘[a]utosegmental theory was hailed as a resolution to this controversy’ in favour of levels could, most ‘parsimoniously’, simply be seen as another triumph of the alphabetic principle, favouring ‘segments’ over ‘prosodies’. Indeed it would make just as much, if not more, sense to argue that the pitch contour is the basic phenomenon, and pitch levels, such as those given in the scales or modes of many musical traditions, are simply an abstraction from the contours, used to explain certain features of tonal organisation. The experimental evidence that Brown goes on to adduce, though again failing to include any examples of his paradigmatic ‘tonal’ languages, is vitiated by this
unconscious assumption, an assumption which on the face of it would only be ‘commonsense’ to users of an alphabetic-style musical orthography like that of most Western music. (It would be interesting to speculate what theory of music patterning might be derived from a tablature notation, like that used for the lute in Europe, or the guqin ‘zither’ in China.)

It may seem unfair to blame Brown for all of this. After all, it is surely the linguists’ responsibility to make such things clear, and it is unfortunate that the most visible school of linguistics in the last 30 years has been precisely the one that is most unreflective about the derivation of its own categories, and most inclined to equate theory and phenomenon. As Bickerton points out in the relation to the contribution, or lack thereof, by linguists to debates about the origins of language, ever since the famous ban on the topic by the Linguistic Association of Paris in 1866:

…linguists are poor defenders of territory … Even today one can number on the fingers of one’s hands the serious linguists who are interested in the topic [of language origins]. However, undefended territory does not remain vacant for long, and members of other disciplines (anthropologists, psychologists, biologists) hastened to colonise the area with deplorable consequences. (Bickerton, 2000: 155)

If I may seem to be singling out Brown as one example of someone ‘colonising the area with deplorable consequences’, I am not speaking here as ‘a linguist with a strong sense of territoriality’, a charge Bickerton also defends himself against (2000: 155). I am rather putting in a plea for a greater sense of historical perspective, both within and across the disciplines of linguistics and musicology, and well as for a clearer sense of the distinction between the phenomena that we all have to deal with, and our theoretical ‘facts’. As Goethe said long ago, alles Faktische schon Theorie ist, ‘everything factual is already (part of) a theory’: a phrase used as an epigraph by J. R. Firth in his survey paper ‘A synopsis of linguistic theory, 1933–1955’ (1957) – well worth reading simply as a coherent and comprehensive model of language that declines to accept almost all of the assumptions of Bloomfieldian models – and whose importance is stressed a number of times by John Ellis in his thoughtful critique of contemporary linguistics Language, Thought and Logic (1993: 88, 127). Brown’s attempted synthesis contains much that is suggestive and useful, and no doubt much that, as a non-biologist, I am not qualified to evaluate. However, the linguistic simplifications and over-generalisations which lie at the base of his ‘musilanguage’ model, as well as his lack of what Firth referred to as the ‘renewal of connection’ between theory and data, both seriously undercut the solidity of his theoretical edifice.
4 Cognitive musicology and cognitive linguistics

Having shown some of the unfortunate effects of an over-reliance on the achievements of one branch of linguistics, and an insufficiently critical and self-conscious translation of linguistic notions into the sphere of musicology, we need to look more closely at the sources of these ideas, and the form in which they have become widespread outside linguistics.

For this purpose, the *locus classicus* of linguistically-inspired analyses of music is surely Lerdahl and Jackendoff’s *A Generative Theory of Tonal Music* (1983). Far from being a ‘one-hander’ attempt like Swain’s or Brown’s work, this is a high-level collaboration between a composer and music theorist, Lerdahl, and a cognitive linguist, Jackendoff. The main aim of this book, which as the authors remark in a wonderful throw-away line, was ‘written intermittently along with string quartets and books on linguistic theory’ (1983: xi), is made very clear from the start:

…the theory is intended as an investigation of a domain of human cognitive capacity. Thus it should be useful to linguists and psychologists, if for no other purpose than as an example of the methodology of linguistics applied to a different domain. We believe that our generative theory of music can provide a model of how to construct a competence theory (in Chomsky’s sense) without being crippled by a slavish adherence to standard linguistic formalisms. (Lerdahl and Jackendoff, 1983: xiii)

This programmatic statement contains several themes that deserve elucidation. Firstly, unlike Swain or Brown, Lerdahl and Jackendoff are at pains to stress that they are not proceeding on the basis of analogies like ‘music is a language’, or ‘music has a grammar’. Instead they stress the methodological credentials of their linguistics, ‘the formal nature of the theory’, i.e. that it is formalisable, and thus capable of presenting an explicit description of musical patterning; and secondly, its ‘psychological concerns’, that it is based on a cognitive understanding of meaningful behaviour (1983: 5). As these two features are the main planks of Chomskyan linguistics, and of the claims of the generative enterprise to be ‘scientific’, they are worth exploring more closely.

Chomsky’s making his theory a formal, explicit one – in a term he introduced from mathematics, ‘generative’ – was one of his main innovations, and hailed as such with the publication of his first book, *Syntactic Structures*, in 1957. The explicitness of his theory, as well as his elegant solutions of various descriptive challenges of English grammar, was very attractive, not only within Bloomfieldian linguistics, but in the world of linguistic scholarship more generally. However, like most advances, formalisation turned out to be a two-edged sword, as the functional linguist M. A. K. Halliday explains:
Fifty years after Saussure, Chomksy created a new opposition by calling his own syntagmatic, formal grammar ‘generative’ and claiming that as its distinguishing feature … By generative he meant explicit: written in a way which did not depend on the unconscious assumptions of the reader but could be operated as a formal system. His tremendous achievement was to show that this is in fact possible with a human language, as distinct from an artificial ‘logical’ language. But you have to pay a price: the language has to be so idealised that it bears little relation to what people actually write – and still less to what they actually say. (Halliday, 1985: xxviii)

This theme of the over-idealisation of language within generative linguistics is also taken up by Charles Hockett, one of the many linguists initially attracted to the theory but later critical of its excesses. In his 1968 critique of generative linguistics mentioned above, Hockett points out that Chomsky’s fatal error, again influenced by mathematics, was to conceive of language as a set whose members could be specified by algorithms: in mathematical terms, a ‘well-defined system’ (1968: Chapter 4). Hockett admits that, of course any theory is an ‘approximation’, which is only ‘made possible by leaving some things out of account’. However, as Hockett argues, in this case ‘the things left out of account in order to achieve an approximation of this particular sort are just the most important properties of human language, in that they are the source of its openness’ (Hockett, 1968: 61–2, original emphasis), i.e. what Chomsky refers to as the ‘creative’ aspect of language, in Hockett’s formulation:

The vast majority of the sentences encountered throughout life by any user (= speaker-hearer) of a language are encountered only once: that is, most actually used sentences are novel. This is a central fact for which any linguistic theory must provide. (Hockett, 1968: 38)

While it is true that generative linguists have mostly moved away from Chomsky’s famous characterisation of the grammar of a language as ‘generating all grammatical sequences’ and ‘no ungrammatical ones’ (Chomsky, 1957: 85) – Lerdahl and Jackendoff specifically reject such an aim, settling for the more modest one of providing a ‘structural description’ of sentences, i.e. ‘which strings of words are or are not grammatical sentences’ (1983: 6), this modified aim is still based on the characterisation of a language as an ‘infinite set of sentences’, a characterisation which Hockett very clearly shows to be based on the false characterisation of language as a well-defined system. The spectre of over-idealisation also haunts the methodological practices of generative linguistics (see also the discussion of ‘competence’ vs. ‘performance’ below): we may note that Lerdahl and Jackendoff specify their sources of evidence as coming firstly from ‘contrived examples’, only then followed by ‘the existing
literature of tonal music’ and ‘laboratory experiments’; it seems strange that a cognitive theory of music would not deal, at least in principle, with people’s actual experience of making music.

There is another key feature of generative grammar mentioned in the quotation from Halliday above, that it is syntagmatic: in other words, that it concentrates on the ‘horizontal’ aspect of linguistic organisation – ‘what goes with what’, identifying constituents and how they fit together into constructions. (Another example of the alphabetic principle at work!) In fact Saussure originally coined the term ‘syntagmatic’ as one of a pair of concepts for identifying linguistic elements and assigning them ‘value’ in the system of language, the other being the ‘vertical’ relationship between elements – ‘what appears instead of what’, dubbed by Saussure ‘associative’ but now more usually called ‘paradigmatic’ (Saussure, [1916] 1959: 123). Now while Bloomfieldian linguistics embraced both the syntagmatic and paradigmatic for phonological theory with its notions of distribution and contrast, in its analysis of the meaningful elements of morphemes, words, phrases etc it tended to emphasise mainly the syntagmatic. Syntagmatic co-occurrence was one of the main concepts of the methodology of distributionalism mentioned above, while paradigmatic contrast was less emphasised, since this would have meant explicitly appealing to meaning: something which, as we have noted above, linguists in the Bloomfieldian tradition were loath to do.

Because Chomskyan linguistics, as a development of Bloomfieldian linguistics, was the first to seriously attempt the formalisation of linguistic descriptions, the expectation has grown up that only syntagmatically-based theories are formalisable. While it is easy to point out examples of paradigmatically-based formal theories of language of about the same vintage as generative grammar – for example, Lamb’s stratificational grammar (1962), or Halliday’s systemic functional grammar ([1964] 1976) – it is perhaps more enlightening in the present context to note that Lerdahl and Jackendoff never conceive of musical organisation as being anything but syntagmatic. Thus, in the initial characterisation of their grammar, it is simply assumed that syntagmatic structure is the main variable to be accounted for:

> Each rule of musical grammar is intended to express a general intuition about the organization that the listener attributes to the music he hears. (Lerdahl and Jackendoff, 1983: xii)

While the concept of ‘organisation’ as such doesn’t necessarily entail ‘structure’, the authors later make it clearer that that is the intended interpretation:

> The fundamental concepts of musical structure involve such factors as rhythmic and pitch organization, dynamic and timbral differentiation,
and motivic-thematic processes. These factors and their interactions form intricate structures quite different from, but no less complex than, those of linguistic structure. (Lerdahl and Jackendoff, 1983: 7)

From this it follows that, for a cognitive theory of music, the only factors that require explanation are structural ones:

One commonly speaks of musical structure for which there is no direct correlate in the score or in the sound waves produced in performance ... Insofar as one wishes to ascribe some sort of 'reality' to these kinds of structure, one must ultimately treat them as mental products imposed on or inferred from the physical signal. In our view, the central task of musical theory should be to explicate this mentally produced organization. (Lerdahl and Jackendoff, 1983: 3)

Thus what we see here is a largely unconscious conflation of three things that are in principle quite separate: syntagmatic structure as the main descriptive variable; cognitive organisation as the main locus of explanation; and formalisation as the main means of representation.

Neither theoretically nor historically is it inevitable that these three features should be conflated in a single model. As a historical background to the introduction of their own theory, Lerdahl and Jackendoff provide a brief survey of theories of music, one which is worth quoting at length:

Given a theory of music, one can then inquire as to the status of its theoretical constructs. Medieval theorists justified their constructs partly on theological grounds. A number of theorists, such as Rameau and Hindemith, have based aspects of music theory on the physical principle of the overtone series. There have been philosophical bases for music theory, for instance Hauptmann's use of Hegelian philosophy.

In the twentieth century these types of explanations have fallen into relative disfavour. Two general trends can be discerned. The first is to seek a mathematical foundation for the constructs and relationships of music theory. This in itself is not enough, however, because mathematics is capable of describing any conceivable type of organisation ... The second trend is to fall back on artistic intuition in constructing a theory. But this approach too is inadequate, because it severs questions of art from deeper rational inquiry; it treats music as though it had nothing to do with any other aspect of the world. (Lerdahl and Jackendoff, 1983: 2–3)

This reads suspiciously like a list of straw men. Theology? Hegelian philosophy? How could anyone take them seriously in this day and age? More up-to-date alternatives are dismissed with little argument: mathematics because its scope is...
too broad; artistic intuition, because its scope is too narrow. The clear implication is that the only possible approach consistent with ‘deeper rational inquiry’ is a psychological one.

This fairly perfunctory attempt to deal with – or rather dismiss – alternatives essentially passes over two other ‘aspects of the world’ which could be possible locuses of explanation for music: the physical and the social. The physical is given a brief side-glance – although as its proponents include two composers of the stature of Rameau and Hindemith one might expect it to be worth at least some consideration – but is dismissed, along with theology and philosophy, as out-of-date. The social, however, doesn’t get a look in at all, an omission which surely warrants some explanation.

Because more scholars these days, including linguists, read about Saussure than actually read his work, it is often not remembered that Saussure, like Chomsky, placed linguistics under the general umbrella of psychology, but in the first instance in social psychology. His justification for such a placement is given in a famous, and seminal, passage:

* A science which studies the role of signs as part of social life is conceivable; it would be part of social psychology, and consequently of general psychology; I shall call it *semiology* (from the Greek *semeion*, ‘sign’). Semiology would show what constitutes signs, what laws govern them. Since the science does not yet exist, no-one can say for certain what it would be; but it has a right to exist, a place staked out in advance. Linguistics is only a part of the general science of semiology; the laws discovered by semiology will be laws applicable to linguistics, the latter will circumscribe a well-defined area within the mass of anthropological facts. (Saussure, [1916] 1959: 16, original emphasis)

Saussure, in his characterisation of the ‘linguistic sign’, made it quite clear that he saw the main site of linguistic organisation as the human psyche: both halves of the sign, the ‘concept’ and the ‘sound pattern’ are defined as psychological entities. However, because he also recognised that ‘languaging’ (what he called *le langage*) could only be carried on in a collectivity, he made the abstraction of *la langue*, rendered in Harris’s translation as ‘linguistic structure’, and which was for him the main object of specifically linguistic research, a socially-based concept. Saussure’s programme for the ‘study of signs’ was most comprehensively taken up by the Danish linguist Louis Hjelmslev in his *Prolegomena to a Theory of Language* ([1943] English translation, 1953), and the American linguist Sydney Lamb, who was one of the few linguists to take seriously, and to formalise, Saussure’s conception of language as a network of relations (1962, 1999). The other half of the programme, ‘in social life’, has been most thoroughly developed by the British linguist M. A. K. Halliday, as witnessed by the

Later linguists, especially in the United States, considered what they saw as Saussure's close linking of linguistics with psychology as unhelpful or unnecessary. Both Bloomfield and Harris were familiar with Saussure's work – according to Hockett, Bloomfield 'knew, liked and was profoundly influenced' by it (Hockett, 1968: 11) – and although Bloomfield called on psychology, specifically behaviourist psychology, to explain linguistic meaning (1933: 23–7, 142–4), he did so partly in order to justify the exclusion of meaning from linguistic study proper. When Chomsky, particularly in his 1965 work *Aspects of the Theory of Syntax*, brought psychology back into linguistics – or linguistics into psychology – he did so in a specifically non-social, mentalist form, linking linguistic organisation to innate, genetically determined capabilities of the human brain.

In this regard, it is important to note that Chomsky's distinction between 'competence' and 'performance' (1965: 4), although often equated with Saussure's distinction between *langue*, 'linguistic structure' and *parole* 'utterance', is really quite different, both ontologically and methodologically. As is convincingly argued by Thibault (1997: 9), Saussure did not assign any ontological status to these notions at all, they were simply methodological points of view; to use a more modern terminology, the point Saussure was making in this distinction was that one can look at language either from the point of view of the system which lies behind linguistic utterance, or in terms of the texts which the users of that system produce. Although it is true that Saussure recommended a concentration on the system, and his followers largely (mis)interpreted this to mean at the expense of the text, he did so mainly on the grounds that only by a concentration on the system could one understand language from a specifically linguistic point of view. The history of modern conversational analysis, which draws on anthropology, ethnography, and psychology, as well as linguistics, in a sometimes uneasy collaboration, largely bears Saussure out in this contention; and it should be remembered that, while his lectures dealt with *une linguistique de la langue*, a linguistics of the system, Saussure also promised his students *une linguistique de la parole*, a linguistics of the text, a promise which he did not live to carry out.

In contrast with the very clearly instrumental and methodological nature of Saussure's complementary perspective of system and text, Chomsky claims for his 'competence' what seems like a spurious reality, with it somehow residing in the brain of the individual without reference to the collectivity, and relegates 'performance' to the status of a methodological dustbin, where are to be placed any features of the utterance that interfere with our access to competence (1965: 10, 15). That Chomsky sees this distinction as a dichotomy,
rather than a complementarity, is shown by the fact that he explicitly denies the possibility of a ‘linguistics of performance’ except on the basis of a prior ‘linguistics of competence’.

What is also striking is how Chomsky, and most cognitive theories following him, deny any role at all to the social, or see it as being in antithesis to the psychological. For Saussure there was no contradiction between the two, and his langue-parole complementarity was partly designed to capture a social-individual distinction (the latter being both psychological and physiological – for a critique of this conflation of system with social and text with individual, see Jakobson ([1942] 1990). As I explained above, Bloomfield rejected the social along with the psychological mainly because he wanted linguistics to stay away, at least in the short term, from the difficult and ‘unscientific’ question of meaning; and Chomsky, while bringing back the psychological, was enough of a Bloomfieldian to be equally averse to dealing directly with meaning, and because he followed a specifically mentalist, individualist version of psychology, to see no role for the social either.

From a historical point of view, it is easy enough to understand the different stances of these scholars, each aligning his theory with prestigious scientific models of the day: Saussure with Durkheimian sociology; Bloomfield with behaviourist psychology and logical-positivist philosophy; Chomsky with mentalist philosophy and an innatist evolutionary genetics. But this does not mean that current scholarship should be confined by these same alignments, or should feel obliged to accept one approach over another.

To return to the confluence of these historical and ideological currents in the work of Lerdahl and Jackendoff and others influenced by them, we can see that a whole number of dichotomies are set up, with one side of each dichotomy assigned to the uninteresting or the uninsightful:

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What this leaves us with, in relation to the cognitive exploration of music, is an impoverished model in which, for all its detail, the main aim of description is, in effect, pattern recognition, with little or no cognisance being taken of how these structural patterns relate to their expressive meanings. Furthermore, the complexity and dynamic nature of musical texts – musical parole if you like – are almost completely neglected in favour of identifying the so-called ‘underlying’ regularities of the system; and in that metaphor, and in its source in Chomsky’s distinction between ‘surface structure’ and ‘deep structure’, we see one of the most superficially attractive as well as pernicious idealisations of all.
When Lerdahl and Jackendoff remark, almost in passing, that mere ‘analysis’ is ‘of value insofar as it enables one to express insights into particular pieces’ (1983: 1), but does not compare in explanatory power with a ‘theory’ of musical structure, they are not only privileging the ‘deep’ over the ‘surface’, but also depriving themselves of one of the main sources for understanding the system of music itself, and of the only means of showing how instances of ‘musicking’ affect the system and thus bring about changes in it.

This monistic approach to the study of music and cognition also falls into the trap of circularity, as pointed out forcefully by a musicologist interested in questions of musical innateness, in a paper from the same collection as Bickerton and Brown:

All generative theories of human competence have an implicit reasoning that could become circular through a drift of psychologism or cognitivism. Concerning [Lerdahl and Jackendoff’s *Generative Theory of Tonal Music*], the virtual circle is as follows: the existence of an innate musical competence is postulated that allows the listener to understand an infinite number of musical sentences. This competence is construed in terms of a model based on the tonal system with its hierarchical grouping structure. A listener’s understanding of music is interpreted in these terms, and corresponding mental operations are attributed to the listener that are taken to be the product of the competence we started out with. Nothing is easier to prove than the psychological reality of this tonal grammar: it is enough to apply experimental paradigms well established in psycholinguistics to tonal music, which is what most research in vogue today does. (Imberty, 2000: 453)

This problem was also pointed out above in relation to Brown’s work, and it should make us doubly wary of inflated or unmodulated claims to being ‘scientific’ from any school of thought which does not recognise the contingent nature of its own concepts.

### 5 Taking stock: a plea for methodological scepticism

The Austrian psychologist of language, Karl Buehler, another scholar who deserves to be more widely known in the English-speaking scholarly world, in his major work *Theory of Language* (*Sprachtheorie*), had this to say about the work of Saussure:

Ferdinand de Saussure’s *Course in General Linguistics* is anything but an account of results. Instead, the *Course* reflects throughout, and excitingly, the methodological scepticism of a researcher who knows his craft and its results just as well as any other does, but cannot refrain from carrying out his own
version of the purifying test of Descartes’s *Meditations* on the linguists’ findings. (Buehler, [1934] 1990: 7)

If I have emphasised, as Buehler also does, the fact that Saussure did not actually write the book to which his name is attached, it is because Saussure has too often been taken as making programmatic statements rather than exploring possibilities. As Buehler goes on to point out:

> His lectures, which were posthumously rounded out to form a book, must have been guided tours through the working drafts of a creative mind of great stature, one still struggling with the problems. I am convinced that we are only at the beginning of the historical influence of Saussure’s work, of his sketches on the topic of language theory. (Buehler, [1934] 1990: 8)

From a contemporary vantage point, it seems ironic that Buehler, writing in the 1930s, should claim that linguistic scholarship is ‘only at the beginning of the historical influence of Saussure’s work’. Having had the misfortune to be dubbed ‘Father of modern linguistics’, Saussure, like many such ‘fathers’ has been honoured more in the breach than the observance: judged on the basis of a work compiled from students’ notes which, as his editors acknowledge, ‘he probably would not have authorised’ ([1916] 1959: xvi); criticised for creating dichotomies – *langue* and *parole*, diachronic and synchronic – where he saw only complementarities; having had the nooks and crannies of his discourse subjected to a highly partisan and out of context deconstruction by Derrida; and so little read these days that, in a recent otherwise very insightful book combining Peircian semiotics and neurophysiology (Deacon, 1997), he was even criticised for viewing language as a nomenclature, the very misconception he argued against! The fate of Saussure, or rather as de Beaugrande suggests (1991: 6), ‘Saussure’, in other words the published version of his ideas (Harris, 1987), should provide a salutary reminder of the dangers of taking over ideas without a clear understanding of their original context.

But while Saussure’s ideas, as given in the *Course* with all its problems, and as explained in insightful and sympathetic commentaries such as those of Harris and Thibault, still deserve more serious attention than they usually receive, what we perhaps even more need to learn from Saussure is what Buehler calls his ‘methodological scepticism’ as a ‘creative mind of great stature … struggling with the problems’. One of the reasons Saussure never got round to writing up his book was, as he himself confessed in a letter of the 1890s to his student Meillet, that he felt ‘the general difficulty of writing even ten lines about language’ and that he was convinced of the ‘utter inadequacy of current terminology, the need to reform it, and in order to do that, to demonstrate what sort of object language is’ (quoted in Culler, 1986: 23–4).
Saussure too worked in an era in which linguistic scholarship thought it had solved the main problems, an era in which, to paraphrase Bickerton, linguists felt they had ‘learned more about language in the last century than in the previous two millennia.’ It should no longer be possible for anyone, linguist or non-linguist, to make such claims uncriticised. We need to understand the basis of the ‘current consensus’ in its historical roots, we need to look at what has been ignored by the ‘mainstream’, as well as what has been emphasised. Most of all, we need to be deeply and positively sceptical of any one school of thought claiming to hold all the answers. I hope that in this paper I have shown the sources and some of the problems of the current orthodoxy, and at least encouraged researchers on music not to take the claims of the linguists at face value.

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