

Rethinking Linguistics

Edited by
Hayley G. Davis
and Talbot J. Taylor

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RETHINKING LINGUISTICS

This book argues that we need to rethink the aims and methods of contemporary linguistics.

Some chapters are concerned with the progress made in the area of integrationism. They incorporate lay views of linguistic matters and discuss normativity and reflexivity. Other chapters are concerned with why and how certain standard theories of orthodox linguistics, such as linguistic creativity and child language acquisition, need to be rethought. For example, orthodox linguists' discussions of linguistic form fail to exemplify how language users are language makers. The application of integrationist theory and other critical perspectives is used as a solution to this basic problem within general linguistics.

The book is aimed at an interdisciplinary readership comprising students, teachers and researchers in the humanities and social sciences, including linguistics, philosophy, sociology and psychology.

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RETHINKING LINGUISTICS
Edited by Hayley G. Davis and Talbot J. Taylor

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To our dear friend George Wolf
1950–2002

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INTRODUCTION: WHY RETHINK LINGUISTICS?

Hayley G. Davis

Rethinking Linguistics is an updating, expansion and substantial revision of an earlier book, *Redefining Linguistics*, (Davis and Taylor, 1990). In the opening chapter of *Redefining Linguistics* I wrote ‘The academic discipline of linguistics is at a critical stage of development. Whatever consensus there may have been fifteen or even ten years ago is fast disappearing’ (1). A decade on these words still ring true. For example, Tom McArthur in the 1996 edition of *The Oxford Companion to the English Language* defines linguistics as

The systematic study of language. Its aim is to look at language objectively, as a human phenomenon, and to account for languages as they are rather than to prescribe rules of correctness in their use. It therefore has a twofold aim: to uncover general principles underlying human language, and to provide reliable descriptions of individual languages.

(McArthur 1996: 558)

whereas Kirsten Malmkjær in *The Linguistics Encyclopedia* writes

This volume demonstrates the many-faceted face of linguistics. Its history begins longer ago than we know, along with its very subject matter, and will continue for as long as that subject matter remains. Having language is probably concomitant with wondering about language, and

so, if there is one thing that sets linguistics apart from other disciplines, it is the fact that its subject matter must be used in the description. There is no metalanguage for language that is not translatable into language, and a metalanguage is, in any case, also a language.

(Malmkjær 1991: xi)

Both linguists above have very different perspectives on the aims and purposes of linguistics, and this chapter will be addressing both views presented in these descriptions.

This is a timely revision, as following the establishment of IAISLC,¹ its first international conference held in 2000, and the recent publication of a number of integrationist books², many issues are in need of taking into account the comments about and developments within general linguistic theory. The contributors to *Rethinking Linguistics* deal with the need to rethink the aims and methods of contemporary linguistics. This rethinking takes many forms. Some chapters are concerned with why and how certain ‘standard theories’ in orthodox linguistics – structural linguistics (Chapter 3: Love), linguistic creativity (chapter 5: Joseph), child language acquisition (chapter 6: Taylor and Shanker), – need to be rethought. Other chapters are concerned with the progress made in the area of integrationism. Chapter 1 (Davis) discusses the need to incorporate laypeople’s views and experiences of linguistic matters; Chapter 2 (Harris) considers metalanguage and the non-compartmentalisation principle as a way of demythologising linguistics and advancing our understanding both of what language is and of the part it plays in our lives; Chapter 7 (Harris) considers co-temporality and repetition as a way of demonstrating that integrational linguistics ‘is not just a minor offshoot from the mainstream tradition but a radical departure in philosophy of language’ (this volume). Chapter 4 (Taylor) looks at the nuts and bolts of communication: ‘the ability to participate in reflexive discourse is a *prerequisite* for engaging with and contributing to the communicational worlds in which we live’ (Taylor, this volume). Although not all the contributors to this volume would necessarily agree with all my proposals for rethinking linguistics, all would concur with the recommendation that the technical terms informing traditional linguistic theory need to be

extensively scrutinized. What will be examined in the chapters in this edition are the implications arising in the course of attempting a complete overhaul of the linguistic/metalinguistic divide.

1:1 Reordering first- and second-order linguistic constructs

This overhaul is needed to demonstrate that there has been a gross confusion by orthodox linguists between first- and second-order linguistic constructs, which has prevented linguists from arriving at a proficient and practical understanding of communication. Orthodox linguists tend to treat languages as autonomous first-order objects which pre-exist their use by speakers. For such linguists, particular languages do exist regardless of what the speakers believe about them and consequently ‘linguistic scientists’ investigate the objective existence of linguistic facts. However, integrationists argue that the orthodox linguists’ talk of *words*, *grammar*, *meaning* is just an extension of lay metalanguage. The difference between laypeople’s and the professional linguists’ metalanguage is that most orthodox linguists feel the need to fix, codify and systematise such second-order concepts in order to explain how communication works: so that on this view speakers become communicators by virtue of knowing how to *use* this determinate object. The orthodoxy, in its endeavour to make language a scientific object of enquiry, segregates first- and second-order abilities and posits an idealized system, a ‘fixed code’ – in order to explicate how language makes communication possible. This code-theory model, inherited from Saussure’s speech-circuit model of communication, is derived, as Taylor (1992) shows, from attempting to place common-sense views of language on a scientific footing. And it is precisely because of its mundane appearance that code theory is such a powerful form of intellectual discourse. But as many of the contributions in this edition show, the explanatory power of this idealization is zero. Harris argues that the postulation of a fixed code rules out the possibility both of innovation in language and learning a new word (***) . Joseph, (Chapter 5, this volume) wishes to see interpretation as something not automatically determined by the text but as a creative act by the hearer/reader. Taylor and Shanker show in their discussion of child language

acquisition (chapter 6, this volume) first- order and second-order aspects of language development are integrated. ““First-order” abilities concern the production of particular kinds of linguistic acts the characteristics of which are reflexively maintained by means of the culture’s everyday discourse about what one does and can do in verbal interaction’ (Taylor and Shanker). The assumptions lying behind code theory run counter to certain features of the everyday linguistic experience of members of the societies concerned. What any individual understands by any metalinguistic term will depend on that individual’s reflection on that particular term on that particular occasion (Davis 2001).

Even though second-order constructs may well influence and feature widely in speakers’ first-order linguistic behaviour, that does not suffice to make such constructs any more ‘right’ or ‘real’ for the speakers. And, by the same token the professional linguist is not in a better position than the lay person to evaluate the linguistic behaviour of individuals. The orthodox linguist, in generalising about such second-order constructs as *words, language, meaning* etc. is abstracting from immediate particular linguistic events. Even though any attempt to come to terms with communication and language is bound to involve a process of recontextualization, all context-free signs are still artefacts of analysis and therefore contextualized by the procedures and purposes of the analysis in question. ‘what constitutes “saying the same thing” depends on the kind of sameness required’ (See Love, this volume). How events are interpreted or analysed will depend upon the individual attempting to make sense of, or justify, or manipulate such events (*I dumped her because she didn’t speak my language, I can’t take him to meet my parents, his language is too foul, I won’t get into Oxford, I speak the wrong sort of language. . .*). And how any individual makes sense of communicative events is a highly personal process.

This goes part of the way to explaining why the concepts ‘right’ and ‘wrong’ (linguistic beliefs) figure extremely highly in the metalinguistic repertoire of laypeople. A language is a second-order construct which arises from an idea about first-order utterances – namely that they are repeatable. The allegedly repeatable items may then become institutionalized and treated as constituting *the* language of a certain community. But this abstraction does not

thereby become a first-order linguistic reality. For the integrationist, any metalinguistic concept (*language, word, grammar, meaning...*) is based upon prior analyses and interpretations of communicative events. And there is no prior assumption that all will command and use the same metalinguistic concepts to make sense of their own linguistic experience. Since any and every linguistic sign is created by the context, and the context in part by the participants, then words, gestures, paintings, music, poems and so on, may mean different things to different people and also to the same person in different contexts. A piece of music, painting or poem may seem 'romantic' when experienced with one particular person, but when viewed, experienced or heard with other participants, may be derided as 'old-fashioned', 'banal', 'kitsch', or 'pedestrian'.

What language is, or even what a linguistic fact is, cannot be incontrovertibly given. Nor is it possible to differentiate uncontentiously between the linguistic and the non-linguistic. The controversy over what has become known as 'non-verbal communication' is a case in point. The entanglements linguists encounter in attempting to circumscribe language are tortuous, as Poyatos explains:

Lyons (1972) distinguishes, in the first place, vocal and nonvocal signals, according to whether the signals are transmitted in the vocal-auditory 'channel' or not (by this term implying the two end-points, sender and receiver), and then considers language as made up of verbal (exactly [sic] lexical) and nonverbal (prosodic) components, the latter of course, being still vocal: while he is inclined with Abercrombie to apply the controversial term 'paralinguistic' to features playing a supporting role, such as gestures and eye movements, and to include both prosodic and paralinguistic phenomena with nonsegmental ('linguistic,' therefore, subsuming for him verbal and prosodic) ...

Laver and Hutcherson (1972), in the introduction to a volume of readings on face-to-face interaction, identify verbal with actual words, nonverbal with vocal or nonvocal conversational behaviour (for them paralanguage) apart from words, thus distinguishing: vocal-verbal (words), vocal-nonverbal (intonation and paralanguage), nonvocal-verbal

(written or printed language), nonvocal- nonverbal (kinesics: facial expression, gesture, and posture); four means of communicating that offer linguistic, paralinguistic (for them nonlinguistic, nonverbal phenomena, both vocal [tone of voice] and nonvocal [kinesics, proxemics and related modalities]), and extralinguistic features ('by definition non-verbal, non-linguistic and non-paralinguistic': either vocal [biologically, psychologically and socially-based voice quality] or nonvocal [as the style of dress]) (12–13).

(Poyatos 1983: 67–8)

It is clear that these theorists see verbal signs – ‘actual words’, not vocal signs, ‘tone of voice or intonation’ – as constituting the backbone of language; the rest, although communicationally important, is relegated to the ‘nonlinguistic’, ‘extralinguistic’ or ‘paralinguistic,’ such features merely performing a ‘supporting role’. Any ‘linguistic feature’ such as tone of voice, intonation, facial expression, that has not been codified, taught or learned is not seen to be part of ‘language proper’. This is because orthodox linguists have tended to treat (while refusing to admit it) the written sign as the prototype of all language. Cameron, in *Working With Spoken Discourse* (2001) mentions some problems for the discourse analyst when this ‘scriptist’ perspective is adopted:

Anyone who works with talk needs to bear in mind that meaning may lie in prosodic and paralinguistic features as much as in words (again, this is a point many people do not fully appreciate because of their tendency to treat writing as the prototype of all language). These features have to do with pitch, stress rhythm, pace, loudness, voice quality, and so on. . .

Clearly, nonverbal aspects of communication cannot be investigated systematically unless visual as well as verbal information is recorded and transcribed. This is a complex undertaking, however: the transcriber must not only find conventional ways to represent participants’ bodily movements and the direction of their gaze, s/he must also articulate the visual with the verbal information, showing

for example how the timing of a speaker's movements relates to the timing of their own or others' utterances.

(Cameron, D. 2001: 37–9).

These issues add further support to what André Martinet wrote back in 1984.

If asked point-blank what the object of their science is, I assume that few professional linguists would hesitate to answer that it is 'language'. But if asked what they mean by 'language' serious divergences would soon appear.

(Martinet, A. 1984: 31)

What in a given situation counts as language and what does not is not to be decided by reference to a predetermined analysis of any situation. The notion of *a* language, however, arises with the perception or idea that utterances are repeatable and, with the onset of literacy, such an abstraction may become fixed. But at no point does the language become fixed for its users. Even though individuals may see certain utterances as embodying some underlying sameness, this perception is not a prerequisite for the use of language. To understand utterances, we do not need to relate them to any underlying, fundamental or essential abstractions – abstractions will always vary depending on the viewpoint of the speaker/hearer.

So far, it is clear that we do not have a clear statement on what language is – unless we understand it to involve some representation by the written word. But what is more disconcerting is that segregational linguists have, in the past, been reluctant even to consider the written word as part of 'language proper'. Although most in western literate societies would agree there are what are called 'languages', what any individual understands by them will be dependent on contextualization (see Harris' discussion of the metalinguistic takeover of Cobol, Algol, Basic etc., this volume).

It is not, *pace* segregationists, that speakers demonstrate their ability to speak by selecting from a pre-determined set of options: rather the context of the linguistic act *creates* the linguistic sign. And here, I am using the word 'context' in a broad sense, to

include the assumptions a speaker may hold about a hearer's intentions.

1:2 Linguistics as constituting its own subject matter

Linguistics has been shown to be a subject which cannot be mapped out in advance by direct appeal to the concept 'language' since that very concept has been shown to be part of the subject matter under investigation (see Davis 2001). It is useless to base linguistics on some *a priori* concept of 'a language.'

For the integrationist, any concept of 'a language' that human beings entertain must ultimately be the product of reflection on linguistic experience, i.e. on the first-order communicational activities of speaking, listening, understanding or failing to understand, etc. It is in this sense that, in integrational linguistics, appeal to 'languages' is neither dismissed out of hand nor taken for granted, but placed on the linguist's list of explicanda.

(Harris 1998: 55)

The problem for segregational linguistics is that language is both a means of communication and a topic of inquiry. This is what makes rethinking Linguistics an enterprise of a different order from rethinking Astrology, French or Yoga. For these subjects, i.e. disciplines, are not open to revision in the same sense. Whatever reassessment is made of their contents, e.g. '*le weekend* is not French', 'that's not a yoga position', the subject itself remains recognisably intact. Many linguists attempt to identify the difference. 'Linguistics is a relatively young social science, in which there has been a massive expansion in almost all areas. It now comprises a large number of flourishing branches, several of them hybrids with other disciplines' (McArthur 1996: 558). This is a rather bizarre claim. Linguistics is arguably the oldest of the social sciences. Robins, in *A Short History of Linguistics* traces the origin of linguistics back to the Greeks in the fifth century BC.

Linguists today are not alone in their achievements, their disputes, and their problems. They are the heirs to more than two millennia of the wonder that the 'strangeness, beauty, and import of human speech' has never failed to arouse among sensitive and enquiring minds.

(Robins, R.H. 1979: 7)

Early works on language did not involve trying to find fundamental truths about language itself, but rather were philosophical discussions about the relation language had to questions of truth, or knowledge, or nature, or the mind etc. Thus language was not treated as an autonomous object. Language study was not a matter of knowing the forms, meanings, syntax, phonology and other aspects of particular languages. It involved questions of a wider significance involving for instance, the origin of language and thought. A major change came in 1786 when Sir William Jones discovered that Sanskrit, Greek, Latin, Celtic, and Germanic all had certain structural similarities and must have been derived from the same source. This observation then led to the writing of comparative grammars and the setting up of a hypothetical ancestor. Linguistic study, in turn was then influenced by scientific models such as those being developed by Darwin's evolutionary thesis and by the introduction of mechanistic physics and other natural sciences. August Schleicher in 1863 claimed that Languages are natural organisms which are born, grow, develop, become old, and die according to fixed laws. Glottic or linguistic science he claimed is therefore a natural science; its method is that of the natural sciences.

But during the late nineteenth century the results of these comparative grammars were placed in an historical perspective largely through the work of the neogrammarians, such scholars thinking that linguistic comparison was worthless and misleading when divorced from history. Hermann Paul wrote

What is explained as an historical and still scientific observation of language is at bottom nothing but one incompletely historical, through defects partly of the observer, partly of the material to be observed.

(1890: xlvi–xlvii)

And so I cannot conceive how anyone can reflect with any advantage on a language, without tracing to some extent the way in which it has historically developed.

(1890: xlviii)

A major difference between these earlier conceptions of linguistics and ‘general linguistics’ involved Saussure’s rethinking and transformation of language study. Saussure posed as his fundamental question not ‘what is language?’ but rather ‘what is linguistics?’. ‘What is it that linguistics sets out to analyse? What is the actual object of study in its entirety? The question is a particularly difficult one?’ (Saussure 1922: 23). The difficulty, for Saussure, is that whereas other sciences study objects given in advance, in linguistics, ‘it is the viewpoint adopted which creates the object’ (Saussure: 1922: 23). So, for Saussure, one could not have a discipline called general linguistics unless one first identified the theoretical viewpoint adopted. He claimed that a unifying theoretical standpoint was needed for describing linguistic data and that the discipline should be self-contained, autonomous, independent of any other sciences, and that the object of study should be ‘real’.

He is the first thinker to issue a radical challenge to the notion that had been prevalent in the Western tradition from Plato onwards: namely, that the core of any language comprises an inventory of names designating things, persons properties and events already given to human understanding in advance of language. The theoretical task as Saussure saw it, was to find an alternative set of assumptions on which it would be possible, at last, to erect a genuine science of language.

(Harris 1989: 190)

It is not the age of linguistics that makes it difficult to define; rather, the difference between Astrology, French, Yoga and linguistics is that, in the case of linguistics, the subject, as Saussure implicitly acknowledged over half a century ago, is also the subject matter.³ What will be discussed in this chapter are the implications of the thesis that linguistics is to be seen as constituting its own subject matter. Any subject matter comes along with its own metalanguage.

Many linguists have ignored this problem of definition, simply saying ‘Linguistics is the science of “language itself”’. This definition has suited scholars, as it has allowed them the prestige of being called ‘scientists’. A sceptic may query, ‘If linguistics is not intended to describe languages scientifically, what else can it do?’ Such questions in part are due to a perceived disciplinary gap, in that ‘linguistics’ has often been seen as being rather late on the academic scene. Other scholars had staked out areas of language study for their own territory (rhetoric, philology, dialectology etc.) leaving an unoccupied space for the study of ‘language itself’, i.e. for its own sake. However, definitions of language in linguistic textbooks demonstrate that what ‘language itself’ is, is by no means uncontroversial. All this should demonstrate that linguistics is not a subject with a clearly (if at all) defined subject-matter. In most other disciplines, the subject matter precedes the subject. The lack of consensus on what language, or a language, is in itself shows that this has not been the case with linguistics. However, this lack of consensus has not prevented orthodox linguists from believing that somehow their postulated language systems consist of determinate linguistic units and relations. But from an integrationist perspective this postulated language system is no more than a mythical idealization.⁴

1:3 Rethinking language users as language makers

The linguistic orthodoxy also fails to see speakers as language *makers*. We are not types who communicate by choosing from a fixed range of units and combinations; rather we are makers of meaning and linguistics. As Harris succinctly puts it elsewhere

The integrational perspective sees us as making linguistic signs as we go; and as having no alternative but to do this, because language is time-bound. For the integrationist, we are time bound agents, in language as in all other activities. There is no way we can step outside the time-track of communication. Once this is conceded, it follows that there is no such thing as a contextless sign. A sign cannot exist except in some temporally circumscribed context. That contextualization is a foundational condition of the very

existence of the sign. And that is why there is no question of giving any *general* account of ‘what else’ A and B need to know, apart from their fixed code.

(Harris 1998: 81)

Harris’s quotation well covers the following incident in my (‘typical’) household.

ZOE: ‘Mummy, look Ella’s swearing. That’s swearing isn’t it?’

MUMMY: ‘No, that’s waving two fingers in the air’.

On this occasion I can only come up with a contextual interpretation of what was happening: that Zoe wanted to get her sister Ella into trouble because Ella was performing an act of defiance (behind my back). This interpretation would be influenced both by my knowledge of Zoe and Ella’s current behaviour (Zoe had been complaining because Ella would not ‘share’ her gameboy game) and by my knowledge of their similar behaviour in the past. At the time, whether or not Ella’s gestures constituted swearing was both irrelevant and unanswerable. I would hazard a guess that Ella chose this action to annoy Zoe because it did not leave her vulnerable to being accused by me of ‘swearing’ or ‘being rude’.

We can also compare this episode with a report on BBC News Online concerning French Connection UK’s advertising poster ‘*fcuk* fashion’. Profits apparently soared after the poster first appeared in 1997 but the Advertising Standards Authority said that the latest poster was ‘irresponsible’ and ‘brought advertising into disrepute’. It was also alleged that it was both ‘kinky’ and ‘another swearword’. How *fcuk* could be labelled as ‘irresponsible’, ‘kinky’ or a ‘swearword’ is remarkable for orthodox linguists as *fcuk* is not an English word and therefore does not have any assigned meaning. That being so, how can it be a swearword? In the same way as Ella, French Connection UK could also, if accused, deny that they were being ‘obscene’ or rude. Both Ella’s V sign and *fcuk* for orthodox linguists are not even words.

What is more extraordinary is that these two examples would not even be contemplated by segregational linguists. What is interesting from an integrationist perspective is that the ‘linguistic forms’ used

have not been treated in any mainstream linguistic textbook to my knowledge. The study of such forms would not be considered, as it is difficult to see how they could be seen as falling within the scope of linguistics as it is defined today. And both the communication of Ella and French Connection UK involved the use of creative linguistic signs by using ‘forms’ which could not conclusively be described as true ‘linguistic forms’. In addition, both cases share some similarities: they both involve a number of integrated processes. Ella, in deciding to annoy Zoe could have used a variety of verbal or non-verbal forms. French Connection UK could have chosen many different advertising slogans (pictorial and or verbal). The advertisers did not decide to choose from any range of pre-existing forms. And both the ASA and Zoe interpreted the communicational act reflexively. But what is obvious in these cases is that Ella and French Connection UK used linguistic signs and the ‘forms’ used could come under the study of linguistics – they communicated both to me, and to others and involved reflexive discourse. Not only is language constituted by a number of different modes of communication but also the relationship between language and communication is highly intricate and multifaceted.

The linguistic sign, whether spoken, written or manifest in any other medium, is not an object, or a permanent property of an object. It has no fixed or determinate semiotic value. It becomes a sign as and when used as such, and its significance is a function of that use.

(Joseph, Love and Taylor 2001: 211)

Saussure himself acknowledged the difficulty of defining an ‘object’ for linguistic study (Saussure 1922: 23–5). His ‘synchronic’ approach was ostensibly intended to capture the perspective of the lay language user. But if one takes the language-user’s own perspective seriously, it soon becomes apparent that it yields no determinate view of what belongs to *la langue* and what does not. It will vary from speaker to speaker and from occasion to occasion. The context of the linguistic act, which includes the assumptions the speaker may hold about the hearer’s intentions, creates the sign. The linguistic sign, *pace* Saussure, is not given in advance of the

situation. Linguistic communication is inevitably influenced by what orthodox linguists call the ‘extra-linguistic’ – knowledge of the world, power relationships, memory limitations. . . This presumably has always been so. As language does not consist of any fixed set of facts or events open to inspection and analysis, it is up to the layperson and linguist to develop perspectives from which sense can be made of the communicative events.

1:4 Linguistics rethought

It is only by making verbally explicit their own reflexive understandings that lay speakers are able to impose regularities and constraints upon language use. And this folk theoretic approach helps us to see such regularities in the making. Linguistics is, and can only be, the study of what is assumed to be linguistically pertinent. For some, linguistics may be the study of the hidden assumptions underlying linguists’ models, showing, for example, why such models are historical constructs rather than pointing to immutable truths about the nature of language. For others, linguistics may amplify and deepen the answers that we already have about language. Language is a process of making communicational sense of verbal behaviour, and the point of departure is always the individual linguistic act in its communicational setting. Integrationism is not a theory of the speaker in place of a theory of language, rather it is an exploratory investigation of the integrational character of communication. – whether it be verbal, gestural, pictorial or even communicative silence. Rethinking linguistics involves examining how we interpret and construct our day-to-day communicational acts, what views of language are held by certain individuals, and the source and roles that these views play in our living and learning experience. Such a perspective, precisely because it is a perspective, subject to outside influence and in constant interplay with the perspectives of other human activities, must be endlessly rethought.

Notes

- 1 The International Association for the Integrational Study of Language and Communication.

- 2 Davis H.G. (2001), Harris, R. (1996, 2000, 2001), Harris, R. and Wolf, G. (eds.) (1998), Toolan, M. (1996).
- 3 Saussure identified one of the aims of linguistics as being to delimit and define linguistics itself (Saussure 1922: 20).
- 4 'A myth is a cultural fossil, a sedimented form of thinking that has gone unchallenged for so long that it has hardened into a kind of intellectual concrete' (Harris 2001: 1).

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2

ON REDEFINING LINGUISTICS

Roy Harris

Introduction

An American philosopher once wrote of Renoir's painting:

The nudes of Renoir give us delight with no pornographic suggestion. The voluptuous qualities of flesh are retained, even accentuated. But conditions of the physical existence of nude bodies have been abstracted from. Through abstraction and by means of the medium of color, ordinary associations with bare bodies are transferred into a new realm, for these associations are practical stimuli which disappear in the work of art. The esthetic repeals the physical, and the heightening of qualities common to flesh with flowers ejects the erotic. The conception that objects have fixed and unalterable values is precisely the prejudice from which art emancipates us.

(Dewey 1934: 95)

The descriptive linguist, if we are to believe some accounts, is a Renoir operating in the field of speech. What the linguist's descriptions show us is language stripped of its mundane values, but accentuating its bare essentials: language unclothed, but divested too of the 'practical stimuli' which would ordinarily accompany it. Such a description leaves us free to delight in the aesthetics of language structure, its symmetries and complexities, purified of any plebeian communicational interpretations.

It might perhaps be objected to the view of Renoir quoted above that it reduces Renoir's nudes to examples of still life. He might just as well have painted pineapples: the philosopher's comments, if they are valid, would still apply. And much the same objection carries over to the parallel account of linguistic description. But at least Renoir never claimed to be giving a scientific account of the female body; whereas the linguist commonly claims to be giving a scientific account of the structure of speech. Perhaps, on the other hand, a linguist would insist that the conception of speech as merely a chain of physical events is precisely the prejudice from which linguistics emancipates us. But what the philosopher does not sufficiently allow for is that our view of Renoir may depend on our own definition of painting. And that applies equally in the case of language.

The rise of linguistics

Like painting, language may be viewed – and has been viewed – in different ways at different times in history. Since the end of the eighteenth century, two radical changes of perspective have taken place in language studies in the western intellectual world.

The first of these changes accompanied the establishment of linguistics itself as an independent branch of inquiry in the early decades of the nineteenth century. This came about when scholars began to realize that relationships between languages could be studied as a subject in its own right. The term '*linguistique*' was admitted to the dictionary of the French Academy in 1835 and there defined as 'the study of the principles and relationships of languages'. But the new discipline was also known as 'comparative grammar', a term proposed by Schlegel in 1808; and that is the designation which perhaps described better than any other the new orientation of linguistic scholarship. Prompted by the rediscovery of Sanskrit, European linguists embarked on the comparative analysis of the whole family of Indo-European languages. This marked a clear break with the academic grammatical and philological studies of the previous century, which had been directed primarily towards commentary on and elucidation of important ancient texts. Comparative grammar brought to light many resemblances between the various Indo-European languages. Seeking historical reasons

for these resemblances, linguists were inevitably led to attempt to reconstruct the linguistic developments which had given rise to them. Thus by the end of the century, linguistics had become a branch of scholarship defined by its focus on the history of related languages.

The second major change of perspective, and the first redefinition of linguistics itself, came at the beginning of the present century. It was associated primarily with the revolution in linguistic theory ushered in by the work of the Geneva school, led by Saussure. Its effect was to redirect attention away from the history of languages towards the analysis of the synchronic linguistic system, considered as a subject of study in its own right, irrespective of its antecedents and irrespective of its relationship to other such systems. Redefining linguistics in this way gave the discipline the essential academic profile which it has retained down to the present day.

The Geneva school structuralists conceived linguistics as a science of speech communication based on two theoretical principles. These two principles were called the 'principle of arbitrariness' and the 'principle of linearity'. Their adoption resulted in a linguistics which investigated the human language faculty at one remove; that is to say, not directly through the analysis of particular linguistic acts, but indirectly through the analysis of postulated systems underlying them. It was for these postulated systems that Saussure reserved the term '*langue*'. Each such language (*langue*) was envisaged as an independent, self-contained object of knowledge, known to its users. In each such system the minimum linguistic sign consisted of a string of one or more phonemes, associated with a given meaning, and sentences consisted of strings of such signs meaningfully arranged in syntagmatic combinations.

Today this is still the portrayal of speech communication which linguistics offers its students. Writing is discounted as a mere second-order representation of speech, and speech is defined, implicitly or explicitly, as the use by individuals or communities of oral systems of the type just described. It is a remarkable fact that no major school of twentieth-century linguistics-structuralists, distributionalists, glossematians, generativists, tagmemists, or stratificationalists – ever, as a school, renounced Saussure's twin principles. No major linguistic theorist ever called them in question.

Even those who refused to subscribe to the bi-planarity of the linguistic sign did not query arbitrariness or linearity. This is not to say that these two basic principles were never the subject of attack or controversy. Nevertheless, in spite of the divisions and changes of emphasis that have marked the development of linguistics throughout this century, the basis of linguistic theory has remained in all essentials unchanged since it was first laid down in Saussure's Geneva lectures of 1907–1911.

Whether or not we agree with Saussure's view of language, that is the ultimate testimonial to his theoretical acumen. He redefined linguistics in such a way that even those who disagreed with him were forced to accept that definition, and work within it or around it. Any new redefinition, therefore, is still an enterprise – if anyone wishes to attempt it – which must begin from the original Saussurean thesis. The task is itself defined by reference to that theoretical position, which has dominated the academic study of language for most of the present century.

It has also dominated the view of language taken in neighbouring disciplines, where it was welcome because it relieved those disciplines of the burden of undertaking their own linguistic investigations, while allowing them to make use of results obtained in linguistics. If linguistics was a science, as the Geneva school claimed, then all empirical linguistic questions could safely be passed on to the linguist for an expert answer. Furthermore, by implicitly limiting the range of questions the linguist was competent to handle, the orthodox view did not threaten any encroachment upon the academic territory of others. The result was to establish a division of labour in which the field of linguistics was demarcated from such adjacent fields as anthropology, sociology, psychology, physiology, philosophy, and literary studies, all of which claimed some professional interest in language. This whole division of labour rested on the premiss that a viable definition of linguistics had been reached and had been provided with a sound theoretical foundation.

What in fact that definition amounted to was a decision to restrict the concept 'language' in a particularly narrow way. In the first place, it restricted language to speech; and then it restricted speech to the production of determinate strings of phonemes, segmentable into determinate substrings, each identifiable as the manifestation of

a determinate linguistic sign. Each linguistic sign was assumed to have a determinate form, a determinate meaning, and a determinate capacity for linear combination with other linguistic signs. In brief, it was a linguistics which could handle the phenomena of speech only in so far as a speaker's vocalization was reducible to a set of determinate phonological forms with determinate meanings and a determinate combinatorial pattern. Any aspects of speech not reducible to this schema were simply ignored.

The basic contention of the present paper is that the fundamental error in contemporary linguistics is still the fundamental error of Saussure's original thesis. It involves a crude process of abstraction by which certain phenomena are segregated from the continuum of human communication, and these segregated phenomena are then, rather capriciously, set up for academic purposes as constituting the *linguistic* part of communication.

The mistake, in other words, was already inherent in Saussure's first theoretical move, which was to segregate manifestations of language from all forms of non-linguistic communication by the exclusive identification of the former with signals contained in the auditory flow of speech. This single stroke of Saussure's segregational axe simultaneously separated language from non-language and linguistics from all other investigations dealing with human behaviour. Modern linguistics proceeded to demonstrate its indebtedness to Saussure by remaining profoundly segregationalist both in its methodology and in its attitude to neighbouring disciplines. It conceived its own scientific brief in narrowly segregationalist terms, and accordingly took its primary objective to be the construction of an internal systematics of relationships between units identifiable exclusively within the flow of speech.

The expression 'speech communication' thus acquired an interesting ambiguity. For the lay person it continued to mean simply communication by means of verbal utterances, as distinct from communication by writing, by gestures, or by other means. For the academic linguist, on the other hand, it meant communication restricted to the processes of *parole* as identified by Saussure, a far narrower interpretation. This ambiguity itself worked in favour of the establishment of a linguistic orthodoxy which represented the latter as giving a scientific account of the former.

Lessons from the ‘history of linguistics’

The proclamation of linguistics as a new ‘science’ did not long precede the date of Saussure’s birth. His generation was the first to be brought up on this notion, and he was the first of his generation to address seriously the question of how linguists could produce a theoretical validation of that claim. As is well known, Saussure rejected the academically accepted view of his day, which assumed that linguistics could be founded on the empirical discovery of laws of linguistic evolution, including the famous laws of ‘sound change’ for the Indo-European languages, discovered in the nineteenth century.

However, once a new ‘science’ is proclaimed, it inevitably and immediately acquires a history. Its birth is seen as the outcome of earlier views, which are retrospectively resurrected as progenitors. Thus it was with linguistics. Once a subject has retrospectively acquired a history, its practitioners are expected to situate their own practice by reference to it. Thus, again, it was with linguistics. And Saussure, having marked a turning point in that history, automatically validated it. ‘Pre-Saussurean’ came to be both a chronological and a doctrinal designation; rather like the more familiar expression ‘BC’. *Anno Domini* for modern linguistics is established by the date of publication of Saussure’s *Cours de linguistique générale*.

Once an orthodoxy is set up, those who are dissatisfied have only two choices. They can seek to set up a privileged version of that orthodoxy; or they can become heretics. This process operates in linguistics in much the same way as in religion, politics, philosophy, and other areas of human endeavour which require an explicit statement of beliefs. But not all self-proclaimed heretics are sufficiently heretical to acquire historical title to that status. Furthermore, heresy may often take the form of claiming to be more strictly orthodox than the orthodox.

It is interesting to reflect that those linguists who, at one time or another, appeared to be offering the most serious theoretical objections to Saussure in the end turned out to be offering no challenge at all. One thinks particularly here of two major figures in American linguistics: Leonard Bloomfield and Zellig Harris. Both, interestingly enough, thought they were redefining linguistics

because they held certain views about the nature of science and about the human mind. And indeed they disagreed fundamentally with Saussure both on philosophy of science and on philosophy of psychology. But in retrospect it became clear that disagreeing with Saussure either about the way the human mind works, or about the goals and methods of scientific inquiry, was not in the end going to make much difference to academic linguistics. These were marginal considerations, not central ones; and Saussure would doubtless have been amused had he been able to witness the theoretical posturings of American linguistics during the 1930s and 1940s. By the late 1950s, when at last his posthumous treatise had been translated – or mistranslated – into English, Saussurean views were beginning to appear more congenial to a new generation of American linguists, particularly in so far as those views related to semantics. For it is fundamental to Saussurean thinking that the linguistic sign cannot be defined without reference to what it means. And both Leonard Bloomfield and Zellig Harris had presumed to emphasize the study of formal structures at the expense of the study of linguistic meaning.

It is ironic that Bloomfield in particular never learnt the lessons explicitly drawn in his own criticisms of Saussure. In his review of Saussure's posthumous work in the early 1920s Bloomfield accused Saussure of having 'no psychology beyond the crudest popular notions, and his phonetics are an abstraction from French and Swiss-German which will not stand even the test of an application to English' (Bloomfield 1923: 64). Saussure, therefore, for Bloomfield, scores zero both in psychology and in phonetics. Nevertheless, in the same review, Bloomfield bestows on Saussure the highest of academic accolades. Saussure, says Bloomfield, 'has given us the theoretical basis for a science of human speech' (1923: 65). How is that possible, if Saussure is an ignoramus as regards both phonetics and psychology? For do not sounds and their meanings jointly exhaust the domain of human speech? Bloomfield answers his own conundrum as follows, and thereby defines his own theoretical position. Saussure, he says, 'exemplifies in his own person and perhaps unintentionally, what he proves intentionally and in all due form: that psychology and phonetics do not matter at all and are, in principle, irrelevant to the study of language' (1923: 64). The

autonomy of linguistics has not often been asserted more trenchantly.

Ten years later, Bloomfield had become a convert to a then fashionable behaviourism, and gives Saussure only a passing mention on page 19 of his book *Language*, the *magnum opus* of Bloomfieldian linguistics. By that time Bloomfield could not afford to admit that in the study of language psychological theories were irrelevant. So Saussure had to be demoted.

Under Bloomfield's leadership, linguistics in America became even more adamantly segregationalist than the European variety. For the Geneva school, the speech circuit had always included a conceptual component. But for Bloomfield and his followers the only speech phenomena which the linguist could deal with scientifically were those physically present in the articulation of a vocal utterance. Concepts or meanings were not present in this sense, and thus were excluded. In this way the segregation of language from non-language came to be interpreted for all practical purposes as the segregation of the audible sound sequence from everything else.

Where the behaviourists went wrong – wrong, that is to say, had they wished seriously to undermine Saussure's concept of linguistics – was that they accepted as harmless, and even necessary, Saussure's celebrated distinction between synchronic and diachronic linguistics. They misinterpreted this distinction, or at least Bloomfield did; but that is not the point. The point is that once Saussure is allowed his distinction between synchronic and diachronic – however innocuous that concession may seem – then it will be found at the end of the day that the only trump card there was to play against Saussure's game has already been thrown away. It is no good then to take issue on psychological questions. For such questions are already involved in -and presupposed by- the distinction between synchronic and diachronic. That is what Bloomfield failed to see.

A later transatlantic challenge which collapsed was that of transformational-generative grammar. What at first appeared to be novel and un-Saussurean about this approach was its emphasis on algorithmic procedures, an emphasis borrowed from mathematical logic and boosted by the advent of computer-age technology. But this apparent novelty turned out to be merely superficial. Its tacit

basis was still the Saussurean linguistic sign, a given string of phonemes with a fixed meaning. Grammar, for the generativists, was a system of relations between forms and meanings, and speech a set of operations by which those systemic relations gave rise to sequences of uttered sounds. Once it was realized that stating the grammar of a language as a set of generative rules is simply one possible formalization of Saussurean syntagmatics, the generativist bid to redefine linguistics faded. Heresy was overnight transformed into orthodoxy. Generativism simply became the currently fashionable framework within which to pursue an analysis of language which was still based on the principles of arbitrariness and linearity, and remained as exclusively segregationalist as Saussure's.

This particular chapter in the history of ideas therefore suggests a useful maxim for those who set up in the business of redefining linguistics. First, make sure you understand why Saussure's principles seem unassailable. If you do not understand that, then your challenge to orthodox twentieth-century linguistics is not only bound to fail, but will be swept along, as those of Bloomfield and others were, in the mainstream of orthodoxy itself.

Speech as a telementational process

These reflections on the history of linguistics lead directly to a key question. Why did the adoption of the principles of arbitrariness and linearity produce a segregationalist linguistics? Was that inevitable? No, it was not inevitable; but it became inevitable once the principle of arbitrariness and the principle of linearity were conjointly wedded to one particular theory of human communication. The theory in question is telementation; that is to say, the theory which explains communication as the transference of thoughts from one person's mind to another person's mind. Saussure adopted telementation as his theory of communication, although he does not designate it by that term. Nevertheless, the adoption is explicitly spelled out in Saussure's account of what he calls the 'speech circuit'.

Saussure's speech circuit envisages the archetypal speech act, reduced to its bare essentials, stripped of all possible ramifications. There are just two participants, A and B, who in turn take on the roles of speaker and hearer. A says something to B, and B in return

says something to A. That constitutes one completed lap of the speech circuit. This simple scenario assumes that A and B are speaking the same language. If the only language known by A were Catalan and the only language known by B were Cantonese, this exchange would not constitute a speech circuit in Saussure's sense, regardless of how A and B eked out their oral utterances and their mutual tolerance with gestures, facial expressions and other varieties of non-verbal communication.

Let us suppose, then, for the sake of argument, that A and B are both speaking Kalaba. What happens in the speech circuit, according to Saussure, is that certain ideas occur to A. A wishes to transmit these ideas to B. Because A is a speaker of Kalaba, these ideas trigger in A's mind the phonetic image of certain Kalaba words, which A then proceeds to utter. B hears A's utterance and, being also a speaker of Kalaba, is able to interpret the Kalaba sounds heard as having precisely the meanings which correspond to the original ideas in A's mind, which A intended to transmit to B. Speech communication, on this view, is essentially a process of telementation, or thought-transference. The same thoughts may be transferred from A's mind to B's or from B's mind to A's via exactly the same linguistic procedures; in this case, the procedures laid down by and constitutive of the Kalaba language. So B may then reply to A by using exactly the same sequence of procedures, which will be a selection of those known to both A and B in virtue of their being both speakers of Kalaba.

Before analysing this Saussurean scenario further, it may be as well to offer some documentation of the fact that this still remains the basic scenario which linguists of the present generation endorse. Here are four of the many example which might have been chosen. (None of these, it should be noted, contains any explicit reference to Saussure. They are presented as simple accounts of self-evident facts about speech communication.)

- 1 The first thing the speaker has to do is arrange his thoughts, decide what he wants to say and put what he wants to say into *linguistic form*. The message is put into linguistic form by selecting the right words and phrases to express its meaning, and by placing these words in the

correct order required by the grammatical rules of the language. This process is associated with activity in the speaker's brain, and it is in the brain that appropriate instructions, in the form of impulses along the motor nerves, are sent to the muscles of the vocal organs, the tongue, the lips and the vocal cords ... The movements of the vocal organs generate a speech sound wave that travels through the air between speaker and listener. Pressure changes at the ear activate the listener's hearing mechanism and produce nerve impulses that travel along the acoustic nerve to the listener's brain ... We see, therefore, that speech communication consists of a chain of events linking the speaker's brain with the listener's brain ... At the listener's end of the chain, the process is reversed. Events start on a physical level, when the incoming sound wave activates the hearing mechanism. They continue on the physiological level with neural activity in the hearing and perceptual mechanisms. The speech chain is completed on the linguistic level when the listener recognizes the words and sentences transmitted by the speaker.

(Denes and Pinson 1963: 4-7)

- 2 The speaker's message is encoded in the form of a phonetic representation of an utterance by means of the system of linguistic rules with which the speaker is equipped. This encoding then becomes a signal to the speaker's articulatory organs, and he vocalizes an utterance of the proper phonetic shape. This is, in turn, picked up by the hearer's auditory organs. The speech sounds that stimulate these organs are then converted into a neural signal from which a phonetic representation equivalent to the one into which the speaker encoded his message is obtained. This representation is decoded into a representation of the same message that the speaker originally chose to convey by the hearer's equivalent system of linguistic rules.

(Katz 1966: 103-4)

- 3 Language enables a speaker to transform configurations of ideas into configurations of sounds, and it enables a listener within his own mind to transform these sounds back into a reasonable facsimile of the ideas with which the speaker began.

(Chafe 1970: 15)

- 4 *A* has in his head some sort of message (or idea), and he wishes *B* to form in his head the same message. This message is transformed ultimately into a series of neural impulses that are sent to the muscles responsible for the actual production of speech, which follows immediately ... The listener, *B*, must decode *A*'s message by converting the sounds into a semantic representation.

(Cairns and Cairns 1976: 17–18)

Saussure died in 1913. The four accounts just quoted are in chronological order, and the earliest of the four dates from 1963. Whether these accounts were directly or indirectly influenced by Saussure is not the point. The point is that once any theorist adopts a telementational theory of communication, and attempts to graft on to that theory the principles of arbitrariness and linearity, the inevitable result is that it leaves only one option open for explaining what a language is. The only option open is to construe a language as a fixed code, the fixed code known to both *A* and *B*.

Languages as fixed codes

By a 'fixed code' is meant one which remains invariant from speaker to speaker and from occasion to occasion within the sphere in which it operates. It is fixed in the sense in which the institutionalized rules of a game such as chess are fixed. It is no coincidence that chess was Saussure's favourite analogy for explaining and illustrating how language works. If the rules of chess could change unpredictably during the course of a game or a tournament, then chess would become unplayable. The players would have no guarantee that their moves would bring about the intended results. On the contrary, the results produced might well be the opposite of those intended. But in

chess as we understand it, two players are always bound by the same rules, like it or not. The chess code is thus a fixed code, even though different versions of the rules may replace earlier versions in the course of time. It is in a sense exactly analogous to this that when A and B converse in Kalaba, Kalaba is construed as a fixed code.

Why is this theoretically necessary? The answer is that construing a language as a fixed code is demanded by the internal logic of Saussure's speech circuit. Unless the code is fixed, then invoking linguistic knowledge simply does not explain how speech communication works. Given any utterance by A, it is essential that B must not only recognize this utterance as an example of the words A intended to pronounce, but must also attach to those words the same meaning as A does. Otherwise speech communication between A and B necessarily breaks down. This in turn follows from the telementational theory of communication, according to which it is both a necessary and sufficient condition of communication that the ideas which A intends to convey are identical with those which B receives as a result of hearing what A said. Just as in chess, A and B must be following the same rules in order to guarantee that each correctly understands what the other is doing. The fixed code is essential to the concept of a synchronic system. Once change is introduced we are no longer dealing with the original synchronic system but with its diachronic successor.

As in the case of the theory of telementation, Saussure's successors also adopted the theory of the fixed code as a basis for linguistics. This might have been predicted, since the two theories are complementary. The preferred form which the fixed code theory takes in contemporary linguistics involves the postulation of a 'completely homogeneous' speech community. But whether one speaks of fixed codes or completely homogeneous speech communities in the end it amounts to the same thing. The effect in both cases is to eliminate from theoretical consideration the problem of establishing uniformity across individual speakers and communication situations.

To summarize, then, if speech communication is a telementational process, it demands a fixed code which A and B share. If A and B do not share this fixed code, or erroneously suppose they share it when in fact they do not, then speech communication between them must

at some point break down, even though the breakdown may not necessarily be obvious to either party, or have pragmatically serious consequences in any particular instance. So the theoretical assumption must be that, somehow or other, those who manage to communicate with each other via speech share and operate a fixed code, even if they do not realize that this is what they are doing. The fixed code is their common language. In this sense, languages take priority over speakers, and over speech: linguistics is thus envisaged as a science primarily concerned, both in general and in particular cases, with analysing languages, which in turn are assumed to be the fixed codes underlying all successful speech communication.

Next let us consider the connection between the postulated fixed code and the twin principles of orthodox linguistics. The relationship is different in the two cases. There is no logical necessity by which a fixed code has to consist of arbitrary signs. But if languages were systems of non-arbitrary signs, then it would become incumbent upon the linguist to identify what natural or causal principles determine the relationships between forms and meanings. In the absence of any identifiable principles of this nature, the only alternative for linguistics is to opt for the thesis that the linguistic sign is arbitrary. However, if the linguistic sign is indeed arbitrary, then it becomes all the more essential to insist that speech communication between A and B depends on A and B using the same fixed code. For, in the absence of natural principles of any kind, there is no *other* way A and B could arrive at identical interpretations of the messages they exchange.

Nor, on the other hand, does a fixed code have to consist of linear signs. But, again, the operation of a fixed code in which signs are arbitrary demands that A and B share some method of identifying individual signs and sign-combinations. The minimal requirement here is to explain how any given complex utterance can be analysed by A and B into its constituent units, on the basis of whatever fixed code is postulated; and the simplest theoretical assumptions to make are that the meanings of individual signs correspond directly to discrete segments of a continuum, and that the continuum has only one dimension. If this continuum is identified with the temporal flow of speech, then it becomes necessary to insist that communication between A and B depends on their knowing how to segment linear

sequences of sounds into linguistically significant units. For again there is no *other* way, given the same sound sequence, that A and B could independently arrive at identical interpretations. In theory it might be possible to hypothesize some non-linear system of analysis; but such a system would be far more complex for A and B to operate, apart from having less intuitive plausibility. Discrete segmentation of a linear continuum emerges as the simplest possible system for the operation of any fixed code using speech sounds as its sole channel of communication.

Orthodox linguistics thus has an internal elegance and harmony which do not become fully apparent until we see how the principles of arbitrariness and linearity are tacitly linked via the theory of telementation and the postulation of a fixed code. At first sight it might seem that arbitrariness and linearity have been selected merely because they are universal characteristics of speech. But if this were the reason other 'design features' (for example, productivity) would seem to have an equally strong claim. The rationale underlying Saussure's original choice might perhaps be stated most concisely as follows. If we define speech as oral communication, and communication as telementation, then the task of linguistics is to provide a theoretical framework for explaining what makes speech possible and how it may be systematically analysed. Given that we cannot discover any natural principles which explain how the forms of speech are determined by their meanings, then the simplest hypothesis would be that speaker and hearer share a fixed code of arbitrary signs, in which determinate meanings attach to determinate discrete segments in the flow of speech. Arbitrariness and linearity, therefore, are the two additional postulates which are necessary to put the linguist in a position to proceed immediately with the analysis of speech, once it is assumed that speech communication is a telementational process based on fixed codes. Arbitrariness and linearity are both crucial methodologically, because they constrain very rigorously the otherwise limitless possibilities for setting about the analysis of an unknown code.

'But what is wrong with this rationale?' it may well be asked. 'On what other theoretical basis could linguistics possibly proceed?' If linguistics is to be redefined, these are the two questions which must next be tackled.

Languages as synchronic systems

What is wrong with a linguistics which defines itself in such a way as to set up a primary task of analysing postulated fixed codes whose only significant units are assumed to occur in linear sequences audibly manifested in the temporal flow of speech is that the enterprise is doomed in advance to fall foul of its own internal contradictions. Some of these had already occurred to Saussure, as is evident from the attempts he makes to deal with them. In spite of these attempts, and those of his successors, it remains difficult to avoid the conclusion that orthodox linguistics has failed to justify its own claim to be a science of speech communication. In assessing that claim both words in the phrase 'speech communication' are important. Orthodox linguistics did not claim to be a general science of speech *tout court* (which would have involved the anatomy and neurophysiology of the articulatory and auditory apparatus). Nor did it claim to be a general science of communication *tout court* (which would have involved the study of other signs than verbal signs). But it did claim to analyse speech in so far as speech was a form of communication, and communication in so far as communication was conducted by means of speech.

One general objection to a linguistics thus defined is that the fixed-code theory leads straight to what may be called the 'paradox of inquiry'. This arises in the following way. For any given word, either A and B share the same fixed code, in which case they will both assign the same meaning to that word; or else they will not assign the same meaning, in which case they do not share the same fixed code. Suppose, for example, the word is *quadrilateral*. A asks 'How many sides has a quadrilateral?' and B replies 'Four'. If A and B share the same fixed code, then A must already know the answer to the question; whereas in the alternative case A's question is one which it is impossible for B to understand correctly. It makes no difference in principle whether or not 'four' is the right answer, or how the word *quadrilateral* is defined. The point is that a fixed-code theory of speech communication must attribute exactly the same linguistic knowledge to A and B if communication is to be successful. On this theory, therefore, it is impossible for anyone to come to know the meaning of a word by asking another person. But

this conclusion is paradoxical, since asking the meaning of a word is commonly held to be a normal and unproblematic function of speech communication; and furthermore this function is generally regarded as essential for the usual processes of language-learning.

A second objection is that if speech communication is indeed based on a fixed code shared by speakers and hearers it becomes extremely difficult to explain in any plausible way how the fixed code comes to be established in the first place. Every individual undergoes a unique apprenticeship to language, which is shared in full by no one else. Even within very restricted linguistic communities, such as the family, no two members are uniformly exposed to exactly the same learning experiences. The larger the community the less chance there is that any two individuals will have had the same opportunity to acquire exactly the same set of correlations between forms and meanings for purposes of communication. This, precisely, is one reason why it is sometimes argued that fixed codes could exist only at the idiolectal level. For languages are not like legal systems in which a central authority lays down rules and penalties to which all are subject, whether they like it or not. In other words, the fixed code with which A operates is presumably the unique product of A's individual linguistic experience, while the fixed code with which B operates is likewise the unique product of B's individual linguistic experience. But this conclusion contradicts the telementational account of speech communication itself; for we are left without the essential guarantee that A and B share one and the same fixed code. Saussure's *langue* is, very explicitly, an attempt to bridge the gap between individual and collectivity, and thus to resolve the apparent contradiction between the uniqueness of one's own linguistic psychohistory and the apparent facility with which one communicates with other members of the same linguistic community whom one has never previously met. But this attempt merely generates at one remove the no less intractable problem of accounting for how *la langue* comes into existence. Saussure turned his back on the question and simply denied that this was a problem linguists were required to deal with. But it is difficult to see how they can avoid it.

A third objection is that if the speech circuit depends on the operation of a fixed code then innovation becomes a theoretical

impossibility. If A attempts to introduce a new word, B will certainly fail to understand it since *ex hypothesi* the word is not part of the code they share. On the other hand, if either A or B can introduce innovations which are communicationally successful, then the code is not fixed. This conflict between the demands of a fixed code and the possibility of linguistic change Saussure attempted to resolve by introducing a rigorous separation of synchronic from diachronic linguistics. However, this gives no explanation of how a transition from one synchronic system to its diachronic successor is possible for the language-users themselves. Thus the problem of innovation is dismissed, but not solved. The failure to deal with it has a particular irony, since the development of linguistics has been heavily dependent on the introduction of new terminology, and Saussure's *Cours* itself is a case in point. The work should have been incomprehensible if the theory of communication it advances is correct.

A fourth objection to a linguistics based on the fixed-code theory is that even if A and B *were* using the same fixed code they would never be able to be sure of this. For if B wishes to verify that the ideas A wished to convey are indeed those which B interprets A's utterance as conveying, B must either elicit further utterances from A or else assess A's reactions to further utterances by B, or both. The snag is that these tests will encounter verification problems of exactly the same order as raised by A's original utterance. Consequently the tests available to A and B to determine whether both are using the same linguistic system turn out to be either regressive or circular. This conclusion in itself does not automatically invalidate the fixed-code hypothesis; for nothing in that hypothesis entails that A and B realize, let alone could prove, that they share a common linguistic system. Nevertheless, there is something manifestly awkward about an explanation of any human social activity which leaves the participants theoretically unable to grasp what it is they are doing. For social activities are above all those in which the participants' intentions and interpretations of one another's behaviour are crucial factors in determining the course of the activity and the form which it takes.

A fifth objection is perhaps more powerful than any of these, at least as far as the academic status of linguistics is concerned. If

linguistics deals with synchronic speech-systems (or *états de langue* in Saussurean terminology), and these systems are fixed codes, then they do not correspond to ‘languages’ in the everyday sense in which English, French, and German are reckoned to be the languages typically spoken by most people born and brought up in, say, the United Kingdom, France, and Germany. These are *not* fixed codes, whatever else they may be, because they are manifestly not uniform. Smith’s English may not be the same as Brown’s English. The French spoken by Dupont may differ from the French spoken by Duval. Such differences may affect not only pronunciation but grammar and vocabulary as well. Yet Smith is a native speaker of English, just as Brown is; and Dupont is a native speaker of French, just as Duval is. Thus it appears *prima facie* either that linguistics cannot deal with languages like English, French, and German; or if it does it cannot be dealing with fixed codes.

The orthodox strategy for dealing with this objection was again initiated by Saussure, who conceded that everyday language-names such as *English*, *French*, and *German* do not in practice correspond to those synchronic systems underlying speech communication which it is the business of linguistics to analyse. Instead, he claimed that each of these language-names designates a large and vaguely defined group of synchronic systems, historically related to one another. A more recent version of the same strategy is to claim that languages, in the sense in which English, French, and German are languages, are merely publicly constructed social artifacts; whereas the genuine object of linguistic analysis is the ‘internalized grammar’ of a competent speaker, which underlies those social artifacts.

Strategic manoeuvres of this kind parry the objection, but again do not solve the problem. For the question remains as to how descriptive linguists are to identify the existence of whatever system they are supposed to be describing. Saussure’s own answer was that synchronic systems existed at the dialectal or subdialectal level: that is to say, although English as spoken in the year 1900 did not in its totality constitute a synchronic system, nevertheless ‘British English’ or – better still – ‘educated Southern British English’ in 1900 might well qualify. Later linguists saw even that restriction as itself problematic, and instead proposed that

synchronic systems existed ultimately at the idiolectal level of the individual speaker.

This, however, is no solution either, since the descriptive linguist has no guarantee that the speech of any individual informant is always self-consistent. Smith's English may not be the same English on all occasions; it may depend on how Smith feels, who Smith is speaking to, what Smith wants, where Smith is, and a host of other factors. Worse still, if synchronic systems exist only at the idiolectal level, then *ex hypothesi* if Smith and Brown ever manage to engage in successful communication it will be sheer good luck. The identification of synchronic systems with idiolects is theoretically self-defeating for orthodox linguistics. It is no good for Smith to have a fixed code which is shared with no one else.

In short, the fixed-code theory lands linguistics in a dilemma. The business of the descriptive linguist is supposedly to analyse linguistic systems; but that task becomes impossible if the systems themselves cannot be reliably identified in use. The impossibility is on a par with that of describing the rules of various unknown games if the observer cannot be sure which game is being played on any given occasion. It has sometimes been suggested that the way out of this impasse is for the linguist as observer to describe the one language which does not present these difficulties of access, namely the linguist's own. But it then becomes unclear how linguists are supposed to check the internal consistency of their own linguistic practices or the accuracy of their own privileged observations.

The objections summarized above implicitly set certain goals for any proposed redefinition of linguistics. A redefined linguistics needs a theoretical basis which, as its minimal condition of viability, can be shown not to lead directly back into the theoretical morass of intractable orthodox problems.

Idealizations in linguistics

Although the objections mentioned so far already seem sufficient in themselves to call in question the validity of orthodox linguistics, nevertheless they are sometimes dismissed by defenders of the orthodox doctrine as irrelevant, on the ground that no one, from Saussure onwards, had ever seriously supposed that the conditions

laid down in the fixed-code account of speech communication were those which normally obtain in real-life situations. The fixed code and the homogeneous speech community, it is claimed, are merely theoretical idealizations, which it is necessary for linguistics to adopt, just as other sciences adopt for theoretical purposes idealizations which do not correspond to the observable facts. Thus, for example, geometry postulates such idealizations as perfectly parallel lines and points with no dimensions; but these are not to be found in the world of visible, measurable objects. Nevertheless, it would be a mistake to protest on this ground that the theoretical foundations of geometry are inadequate or unsound. Analogously, it is held, idealizations of the kind represented by the fixed code are not only theoretically legitimate but theoretically essential in linguistics; and those who object to them simply fail to understand the role of idealization in scientific inquiry.

Unfortunately, this defence of the orthodox doctrine is based on a false comparison. Broadly speaking, two different types of intellectual idealization may be distinguished. In the exact sciences, and also in applied sciences such as architecture and economics, idealizations play an important role in processes of calculation. Any such idealization which was in practice discovered to be misleading or ineffectual when put to the test by being used as a basis for calculation would very soon be abandoned. In the humanities, by contrast, idealization plays an entirely different role. The ideal monarch, the ideal state, and the ideal mother are abstractions not set up in order to be used as a basis for calculation, but as prescriptive stereotypes on which to focus the discussion of controversial issues concerning how human beings should conduct themselves and how human affairs should be managed. But the ideal speech community, the ideal language, and the ideal speaker-hearer turn out to be neither one thing nor the other. They are neither abstractions to which items and processes in the real world may be regarded as approximating for purposes of calculation; nor are they models held up for purposes of exemplification or emulation. In fact they are, more mundanely, steps in a process of explanation; and as such subject to all the usual criticisms which explanatory moves may incur (including, for instance, that they fail to explain what they purport to explain).

What is particularly damning in the case of orthodox linguistics is that its idealized account of speech communication not merely fails to give a verifiable explanation of what passes for speech communication in the world of every day, but actually makes it theoretically impossible for a linguist proceeding on the basis of this idealization to come up with any linguistic analysis at all. Paradoxically, therefore, linguistics emerges as a ‘science’ in which the scientists have to ignore their own theoretical principles in order to be able to practice. Doubtless there are many disciplines in which, for practical purposes, theoretically illegitimate short cuts are taken every day. But there cannot be many which retain their status as sciences if the *only* way to proceed is for practitioners to flout established theory all the time.

Speech and writing

If one were to rest content with making a merely negative, even if devastating, criticism of modern linguistics, there would be no need to proceed further. What has already been pointed out is the gross disparity – indeed contradiction – between the advertised aims of the descriptive linguist and the theoretical basis on which it is claimed these aims may be pursued. But there are other reasons, not so far mentioned, which call for a redefinition of linguistics.

First, a linguistics which confines the linguistic activities of speaker and hearer to the production and interpretation of arbitrary vocal signals articulated in a simple linear concatenation manifestly fails to deal with the reality of language in all its complexity. The segregationalist attempt to limit the analysis of language to its manifestations in speech is an attempt which fails because the limits it imposes are too narrow. But it is not merely that less stringent limits would be welcome. Rather, the primary theoretical issue to be faced is whether *any* a priori attempt to delimit language as a well demarcated field of inquiry can possibly be successful.

The reasons for rejecting the possibility of an a priori delimitation of language are overwhelming. Consider first the theoretical implications of the cultural fact that some societies have developed language in ways which are totally unknown in other societies. The most obvious historical example is the emergence of writing. Now if

writing is possible, then so may be other potential forms of linguistic communication which now remain unexplored because they do not at present answer to human social needs or opportunities. But at some time in the future, possibly with the advent of new technologies, these potential forms of language may well come to be realized. We already know in advance, however, what the reaction of orthodox segregationists will be. They will simply deny that any form of communication other than speech is language. For this has consistently been the position maintained by orthodox theorists throughout the present century. Bloomfield put it bluntly and unequivocally: 'Writing,' he asserted, 'is not language'. (Bloomfield 1935: 21) This is rather like denying that the piano is a musical instrument on the ground that its invention is comparatively recent and its use confined to a certain range of cultures. To deny the linguistic status of writing in turn invites an answer to the question of what writing actually is. Bloomfield's answer was equally dogmatic. For him, writing is 'merely a way of recording language' (Bloomfield 1935: 21). The trouble with this answer is that it is patently wrong. Language for Bloomfield is speech; and while it is true that writing may indeed be used to provide a record of speech, that is by no means its sole function, or even its major function. The notion that all written texts are records of words spoken prior to their inscription is manifestly absurd. Bloomfield was presumably as well aware of all this as anyone else. Why, then, did he refuse to accept it as relevant to the task of defining linguistics? The reason is not far to seek. Once it is theoretically conceded that language is not confined to oral expression but may also be expressed visually then the principle of linearity has to be abandoned as a foundational principle of linguistics. For visual signs are not necessarily linear.

Few critics have thoroughly examined the question of why Saussure selected linearity as his second principle of linguistics. The roots of the answer probably lie in Lessing's distinction between the spatial and the temporal arts, with which Saussure was no doubt familiar. According to Lessing:

if it is true that painting and poetry in their imitations make use of entirely different means or symbols – the first, namely of form and colour in space, the second of

articulated sounds in time – if these symbols indisputably require a suitable relation to the thing symbolized, then it is clear that the symbols arranged in juxtaposition can only express subjects of which the wholes or parts exist in juxtaposition; while consecutive symbols can only express subjects of which the wholes or parts are consecutive.

(Lessing 1766: 91)

This passage presents a generalized theory of meaning as model of reality, well in advance of Wittgenstein.

Writing is a potentially awkward case for semiotic theories of this kind, because its external form belongs to the pictorial – i.e. visual – mode of expression, whereas its content is merely verbal. (At least, this appears to be the case from a European perspective. From an oriental point of view, the affinity between writing and painting is much closer.) The obvious way to accommodate writing, therefore, is to claim that in spite of its visual form it is merely a derivative representation of speech; and this is the classic segregationalist move.

Like Lessing, Saussure saw the basic theoretical issue as being how to distinguish language from pictorial art; and there is a case for saying that this has remained on the hidden agenda throughout the history of modern linguistics. Now to claim that the linguistic sign is arbitrary, even if that is true, is not enough. For pictorial representation in certain respects is also arbitrary. We do not begin to drive a wedge between the two until we insist on the fact that arbitrariness does not extend as far in both cases. ‘Juxtaposition’, to use Lessing’s term, is arbitrary in speech, but not in painting. Whether the painter depicts one object to the left or to the right of another object is not an arbitrary matter; at least, not for European painting before Picasso. The left-right relationship is taken as signifying the actual disposition of the objects depicted. But if one word precedes or follows another word in speech, it cannot be taken as indicative of the spatial relationship of the objects designated by those words. It is in this sense that the principle of linearity is required to support the more general principle of arbitrariness. The two have to be taken together; and together they implicitly define language as a form of representation in which the sign is not merely

arbitrary but linearly arbitrary; that is to say, spatiotemporally undetermined.

Now a redefinition of linguistics which abandons the principle of linearity no longer has any reason for insisting that speech is the sole or quintessential form of linguistic expression. Viewed in orthodox terms, the problem then becomes how to draw any theoretical distinction between language and pictorial art. But it can be argued that linguistics can indeed be redefined in such a way as to sacrifice both the principle of linearity and the principle of arbitrariness, but nevertheless leave room for distinguishing linguistic representation from pictorial representation in so far as that distinction is relevant or necessary. This is the breakthrough on the hidden agenda of theoretical linguistics which orthodox theorists have so far declined to contemplate.

There are in any case independent reasons for recognizing that although there are linguistic connections between speech and writing, writing cannot be considered merely a functional extension of speech. One reason is simply that it is possible to learn to write a language without knowing how to speak it. Another is that there are written languages used for communication by people unable to understand one another's speech, even though the same written language can be used to transcribe both. A third reason has to do with the psycholinguistics of reading. According to the orthodox segregationalist view, the essential difference between reading the words 'Open the door' and hearing the words 'Open the door' is that the reader has learnt a set of audiographic correlations which make it possible to reconstruct from a series of alphabetic characters the abstract phonetic image of the spoken words. In short, written communication between A and B is exactly the same as spoken communication except that in addition to the triggering of concepts by phonetic images and vice versa there is an extra process by which phonetic images trigger corresponding configurations of visible marks and vice versa. This, allegedly, explains why A and B can dispense with vocalization in written communication, but nevertheless transmit exactly the same message as would have been transmitted by speech. However, there is nowadays general agreement among psychologists who have made a special study of reading, and in particular of various forms of dyslexia, that this

theory, which in effect postulates that we read and write by matching sounds to graphic symbols and vice versa, is far too simplistic to account for the complex cognitive processes associated with literacy. Yet this is the only theory which strictly squares with the segregationalist account of speech and its relationship to writing. In other words, it attributes to A and B a second fixed code of audiographic correlations, which is based on and supplements their primary fixed code of correlations between sounds and concepts. Anything less simplistic automatically raises problems for the segregationalist thesis that writing has no linguistic status of its own.

Verbal and non-verbal signs

The controversy surrounding the relationship between speech and writing has a long history (Harris 1986). The point which deserves emphasis here is that the case of literacy is only one example – although a particularly clear and well documented example – of the more general problem of discerning where in human affairs language begins and ends. For this is coterminous with the problem of defining linguistics itself.

That problem would be no less intractable if writing had never been invented; for it arises no less acutely in the case of pre-literate societies than in the case of societies which have developed forms of writing. In neither case is any solution afforded by the arbitrary decision to restrict linguistics to the study of speech phenomena. For the problem then becomes how to segregate speech phenomena from their embedding in a continuum of communication. Speech, clearly, cannot be simply equated with phonation. Many of the sounds which register on a spectrograph or an oscilloscope do not belong to ‘speech’ in the sense in which that term has been appropriated by orthodox linguistics. On the other hand, much of what orthodox linguistics dismisses as non-verbal is highly pertinent to the interpretation of the sounds speakers make when they engage in speech communication.

The problem was long ago recognized as one pertinent to linguistic theory; for example, by J.R. Firth. Did not Firth, in the wake of Malinowski, recognize the limitations of a segregationalist

approach to language when he wrote of the 'interlocking' of speech and non-speech? Witness, for instance, the following quotation:

The meaning of any particular instance of everyday speech is intimately interlocked not only with an environment of particular sights and sounds, but deeply embedded in the living processes of persons maintaining themselves in society.

(Firth 1952: 13)

In these words, there appears to be a clear recognition that speech cannot be divorced from its integration into a non-verbal context. Nevertheless, recognizing a truth is not to be equated with realizing its theoretical implications. Firthian linguistics never fully realized those implications, in spite of the great emphasis it laid on the importance of contextual factors. Firth himself, as a recently published critique of his work shows (Love 1988), although rejecting Saussure's *langue*, pioneered a form of linguistic analysis which in practice compromises with Saussurean orthodoxy all along the line. In short, Firthian methodology is inconsistent with Firth's proclaimed principles.

What Firth, in common with other critics of the segregationist approach, in the end shied away from was the prospect of redefining linguistics in such a way as to take account of a simple fact of everyday experience. In linguistic communication, what people do *not* say is just as important as what they *do* say. If A utters the words 'Open the door' and B in response says nothing but simply opens it, then B has exhibited a knowledge of English which *pro tanto* not only matches A's but is no less clearly demonstrated than in A's utterance. This is not to say that opening the door is the only way in which B might demonstrate a linguistic proficiency adequate to this particular communication situation; nor that it is the ultimate criterion of B's relevant linguistic knowledge. Both these assumptions would lead to a linguistics defined on a narrowly behaviouristic basis.

Nevertheless, B's behaviour in this situation *is* communicatively relevant, and here the theoretical baby must not be thrown out with the behaviouristic bathwater. For there is no reason to deny that B's

opening the door is a *linguistic* act, just as much as A's utterance. That is to say, opening the door *in this particular situation* is a response which qualifies as 'linguistic' no less indubitably than A's vocalization. It is not to say that every time a door is opened linguistic communication is taking place; any more than it is to say that every time a given sequence of sounds is produced linguistic communication is taking place. But to acknowledge that in opening the door B makes a linguistically appropriate contextual response to A's utterance is to recognize that B's actions also are signs, on an equal footing with the signs expressed vocally by A. By so acting, B in turn signals to A; and, specifically, signals an interpretation of A's own utterance. It is this reciprocity which is constitutive of communication. By focusing exclusively on A's utterance, orthodox linguistics introduces an asymmetry which simply ignores the mutual dependency of A's and B's communicative acts.

The theoretical implications of this mutual dependency are far-reaching. Both A's verbal act and B's non-verbal act have to be seen as integrated constituents in an interactive continuum of communication. Divorced from that continuum, neither has any communicational significance whatsoever. Any attempt to give a systematic analysis of what A does independently of what B does rests on a misconception of what is going on. It is like trying to describe a game of tennis as if it were being played by only one person, and the player on the other side of the net did not exist. Such a description, we may be sure, however exhaustively it appears to cover the actions of the single player selected for attention, cannot make any sense of the game being played.

There are various corollaries for the linguist which follow from this. Some will be dealt with in the remainder of this paper. But before proceeding to those the first essential is to realize how radical a redefinition of linguistics is called for simply in order to accommodate the integration of spoken and unspoken signs in this very elementary type of case.

Orthodox linguistics has no way of dealing with the relationship between A's utterance and B's actions because it cannot deal with the description of non-vocal signalling. Even if it waived the requirement of vocalization, it cannot analyse what B does in terms of the segmentation of a linear sequence into units which match the

three verbal signs A uttered. If it could, then there would be no hindrance to arguing that a systematic description based on the principles of arbitrariness and linearity could be extended to cover the integration of A's utterance and B's non-verbal response. But this would only work if the structure of linguistic communication were much simpler than in fact it is. It might work, for instance, if languages were signalling systems no more complex than highway traffic lights; or if A and B were the builder and his assistant described in the opening paragraphs of Wittgenstein's *Philosophical Investigations* (Wittgenstein 1953). The trouble is that speech is underpinned by non-verbal communication which is vastly more complex than these simple cases exemplify.

If an integrational linguistics starts from the premiss that not all linguistic signs are vocal signs, then immediately it must reject most of what has passed for linguistic analysis in western universities for the past fifty years. For those forms of analysis were predicated precisely upon the assumption which an integrational approach must disavow; namely, that the systematicity of speech is self-contained and can be described without reference to what lies outside the speech circuit. There can be no question of retrospectively accepting work based on that assumption as having provided a preliminary ground-clearing operation, on which it is now possible to proceed to construct fully integrated analyses. An integrational linguistics cannot be built on segregationalist foundations. To cherish that hope would be to make all over again exactly the same mistake as has already been made in the history of the subject. Tinkering with the orthodox paradigm must not be confused with redefining linguistics.

An integrationalist programme

By rejecting a telementational model of communication and substituting an integrationalist model (i.e. one in which the sign is not given in advance of the communication situation but is itself constituted in the context of that situation by virtue of the integrational role it fulfils) the foundation is laid for an entirely new approach to the study of language. It becomes possible to treat linguistic communication as a continuum of interaction which may be manifested both verbally and non-verbally. This requires a

theoretical basis which is quite different from the theoretical basis of orthodox linguistics. But exactly how?

An integrationalist redefinition of linguistics can dispense with at least the following theoretical assumptions: (i) that the linguistic sign is arbitrary; (ii) that the linguistic sign is linear; (iii) that words have meanings; (iv) that grammar has rules; and (v) that there are languages. This last point, despite its paradoxical appearance, follows from the first four. In effect, to dispense with the first four assumptions is, precisely, to say that linguistics does not need to postulate the existence of languages as part of its theoretical apparatus. What is called in question, in other words, is whether the concept of 'a language', as defined by orthodox modern linguistics, corresponds to any determinate or determinable object of analysis at all, whether social or individual, whether institutional or psychological. If there is no such object, it is difficult to evade the conclusion that modern linguistics has been based upon a myth.

A demythologized linguistics (that is to say, a linguistics liberated from the telementational fallacy) would sponsor a type of programme which will be briefly outlined in what follows. Once a telementational model is rejected, one can reject along with it the orthodox constraints on what a language is conceived to be. These constraints flow from the fact that the telementational model requires the assumption of a predetermined plan or fixed code which ensures in advance that the hearer, if all goes well, will be able to receive exactly the message that the speaker intended to convey. It would not do to have a system under which the utterance conveying A's message is interpreted by B as conveying some different message. Such a system could not be a language, according to the telementational theory, because it would fail to ensure communication between A and B.

This being so, the telementational model automatically imposes what may be called an 'invariance condition' on the language. Whatever may vary as between one speaker and another, or between the conveyance of a given message on one occasion and the conveyance of the same message on another occasion, cannot count as part of the language. This is why a segregationalist linguistics, although restricting linguistic communication to speech, cannot deal with all the information for which speech acts as a vehicle. It must

exclude, for example, what the speech signal may convey concerning the speaker's sex, emotional state and personal identity. This is because, in order to ensure the possibility of communication throughout the community, the linguistic 'code book' which any member of the community uses for sending and receiving messages must be exactly the same as that used by every other member, at least in theory. To the extent that in practice the invariance condition is not fulfilled, communication between members will be at best partial or faulty.

It follows from this that the telementational model requires the ideal community to have a language in which all the basic units are determinate and all the rules which govern their combinations and interpretations are determinate. For otherwise there is no possibility of a common code book for the whole community. This stipulation is central to the orthodox language myth concerning synchronic systems. It does not in itself resolve the problem of deciding how much of the communicational process is accounted for by knowledge of the language. But what it does settle in advance is that the expressions of a language have to be determinate both in respect of 'form' and in respect of 'meaning'. There must be fixed rules for deciding whether a given sequence of sounds does or does not represent an expression of the language and, if it does, for identifying that expression; and there must also be fixed rules for assigning the right interpretation to any expression thus identified.

In this way, before any decision has been made about what in principle is to count as 'form' or as 'meaning', the communication model already imposes a bi-planar structuring on the language system, in the sense that no expression with a determinate meaning can fail to have a determinate form corresponding to that meaning, and no expression with a determinate form can fail to have a corresponding determinate meaning. Although for orthodox descriptive purposes the two planes of form and meaning may be considered independently, it is the network of specific correlations between them which makes communication possible. It would be of no avail for the language-user to know all the forms of the language, and all the meanings as well, but not to know which forms had which meanings.

It was Saussure who first carried through this consequence of adopting a telementational model to its logical conclusion by

actually defining the linguistic sign in terms of the psychological association between form and meaning. In orthodox linguistics, not only the individual sign but the whole structure of grammar is likewise tacitly envisaged in terms of telementation. 'The function of a grammar,' we are told, 'is to link meaning with sound' (Bartsch and Vennemann 1973: 3). In other words, grammatical rules are devices for ensuring a consistent correspondence between the form of an utterance and the ideas it conveys. Saussure's distinction between synchronic and diachronic is above all required in his theory in order to provide the logical space within which a telementational model may operate.

In order to redefine linguistics successfully it is essential to reject the whole mythology of language structure which derives from a telementational model of communication. An integrationalist redefinition is in a position to do this because it adopts a perspective which, in Saussurean terms, is neither synchronic nor diachronic but panchronic. It considers as pertinent to linguistic communication both the integration of simultaneously occurring events and also the integration of present events with past events and anticipated future events. This integration is governed by a single 'principle of cotemporality', which postulates a chronological parity between linguistic and non-linguistic events in human experience.

This principle, which orthodox linguistics fails to recognize, is of basic importance if we wish to have a theory of language which can explain how and why communication invariably proceeds on the assumption that every linguistic act is integrated into the individual's experience as a unique event, which has never before occurred and will never recur. Without this principle the theorist cannot even begin to explain the basic and universal metalinguistic concept of 'repetition'. To repeat what was said is perhaps the most general and primitive mechanism involved in the transmission of linguistic information in all societies of which we have any record. It is the mechanism on which the function of the messenger depends in every pre-literate culture.

The orthodox 'principle of linearity' derives what validity it possesses from being a special case of the more general principle of cotemporality. Once we recognize the latter we can dispense with the former. Furthermore, this replacement enables us to give theoretical

recognition to linguistic signs which are non-linear in structure, even though their use is governed by the principle of cotemporality.

The orthodox 'principle of arbitrariness' is, from an integrationalist point of view, simply an irrelevance. It makes no difference whether a given sign is arbitrary (in the orthodox sense) or not; for its significance is always a function of its integration into a particular communication situation. To insist on arbitrariness as a defining feature of the linguistic sign is a futile attempt to draw the boundary between the linguistic and the non-linguistic at a point where it cannot be drawn. This, in turn, is the result of treating the linguistic sign as a decontextualized unit, having a form and a meaning whose relationship can be considered in isolation from the actual employment of the sign in any given situation.

But this is not all. From an integrationalist point of view it might be said that the principle of arbitrariness is doubly irrelevant. For it presupposes that each sign has a meaning; whereas this is an assumption which the integrationalist neither needs nor endorses. By denying that words, or other signs, have meanings what the integrationalist is rejecting is the orthodox claim that there is some invariant semantic value which attaches to a linguistic sign in all circumstances, and from which its interpretation is derived by those who use it. This is the myth of meaning institutionalized in dictionaries, and it is logically required by the telementational account of how speech communication works. For purposes of an integrational analysis, however, the concept of meaning may be dispensed with and replaced by that of communicational function. The crucial difference is that the communicational function of a sign is always contextually determined and derives from the network of integrational relations which obtain in a particular situation.

Likewise, from an integrational point of view, grammar is contextually determined and therefore cannot be stated in terms of decontextualized rules. Since the notion of a rule which varies from one occasion to another is a notion which lacks coherence, it follows that for an integrationalist grammar has no rules. Nor is the province of grammar limited to the narrow domain it occupies in orthodox linguistics. Thus, for instance, if A asks a question and B gives a non-verbal response, there is for the integrationalist a grammatical relation between the question and the non-verbal response, just as

there would be for the orthodox linguist between a question and a verbal answer. In other words, the domain of grammar is the whole domain of combinatorial relationships which are contextually relevant to establishing communicational sense. A raised eyebrow may be as relevant to what is said as the intonation contour of a sentence, or the length of the pause between one sentence and the next.

Finally, it follows that integrationalism has no theoretical place for the concept of a language in the narrow sense recognized by orthodox linguistics, where languages are construed, precisely, as sets of decontextualized rules, not only grammatical, but phonological and semantic rules as well. On the other hand, the integrationalist recognizes the lay metalinguistic distinctions that are drawn between one language and another, for such distinctions are among those in terms of which lay members of a linguistic community construe their own linguistic experience. For it is not that lay people are mistaken or misguided when they classify some words as 'English', others as 'French', others as 'German', and so on. What is mistaken is the way in which orthodox linguistics has treated an explicandum as a theoretical postulate. Starting from the postulate that linguistics deals in the first instance with determinate rule-based systems called 'languages' has the effect of standing the problem on its head. That makes it almost impossible to arrive at any clear understanding of how such concepts arise and what role they play in our conceptualization of the communicational space in which human beings live. Languages are functions of communicational processes, not vice versa.

What, then, is the integrationalist programme for linguistics? Integrationalism redefines linguistics as a mode of inquiry into the construction and articulation of our linguistic experience. It inquires not into the hypothetical structure of abstract linguistic systems, nor into their even more hypothetical representations in the human brain, but into the everyday integrational mechanisms by means of which the reality of the linguistic sign as a fact of life is established. For this purpose, in contradistinction to all previous linguistic programmes, it rejects any a priori attempt to circumscribe the phenomena of language or to draw a distinction between language and non-language which will be valid in each and every case. Instead, it

delimits its own sphere of investigation by reference to dimensions of communicational relevance which apply to all forms of sign behaviour in human communities. Such an inquiry may conveniently distinguish between three different scales or levels of relevance, depending on the mode of our involvement in communicational processes. One scale, which may be termed 'macrosocial', deals with factors which situate any given communication in its particular historical and cultural context. A second, which we may term 'biomechanical', deals with factors of a physiological and physical nature which determine the parameters of communication within that situation. The third scale is the integrational scale itself, concerned with communication as a function of the individual's experience in the context of a given situation.

Any episode of communication, in its totality, will call for analysis on all three scales. To the macrosocial scale belong factors of the kind which orthodox linguistics relegates to such subdisciplines as dialectology and sociolinguistics. To the biomechanical scale belong factors of the kind dealt with in articulatory, auditory, and acoustic phonetics, along with others which relate to sight and other sensory capacities. To the integrational scale belong factors relating to the psychohistories of the individual participants, which affect how they negotiate and make sense of the episode in question. The episode itself is the unique integrational product of all these factors.

Accordingly, an integrational linguistics will focus on typically different questions from those which have preoccupied and still preoccupy the orthodox linguist. Investigations which are at most of marginal interest within the framework of linguistic orthodoxy become central. Whereas the attempt to give a mathematically precise formulation to rules of grammar can tell us nothing about how most people construct and articulate their own linguistic experience, we may on the contrary learn a great deal about this by asking what everyday metalinguistic vocabulary they use. Grammatical formalizations reveal more about the grammarian than about the language which the grammarian claims to be formalizing. Even the pursuit of abstract linguistic universals will teach us less about the human mind than studying how, in specific situations, human beings combine verbal and non-verbal signalling for purposes of communication, and how they apportion the

communicational load between verbal and non-verbal devices. The number of parts of speech a language has (however a linguist decides to count them) cannot be more important than distinguishing the different integrational functions that different types of word fulfil in discourse. In short, the strategies and assumptions people bring to bear on the communicational tasks of daily activity, tasks they are obliged to deal with by whatever means they can, are all an integrational linguistics needs to study in order to advance our understanding of what language is and the part it plays in our lives.

An integrational approach thus makes possible a thoroughgoing demythologization of linguistics. The first step in the demythologizing process is simply to convince linguists that no disastrous consequences ensue from abandoning the hallowed assumptions of orthodox linguistic theory. The case is roughly parallel to the demythologization of economics which was accomplished earlier this century by theorists led by John Maynard Keynes. The prevailing economic myth which the Keynesians attacked was, as it happens, one which bears striking similarities to the currently prevalent myth of orthodox linguistics. These similarities are not merely coincidental, but to trace the historical interconnections falls outside the scope of the present paper. They are similarities which hinge on a common concept of 'value'. Just as orthodox linguistics treats sounds as having meanings by standing for concepts or for objects and persons in the external world, so the basic idea of economic theory which the Keynesians called in question was the idea that a pound note had a value by standing for a quantity of gold. Confidence in the so-called 'gold standard' went hand in hand with the popular view that pound notes in themselves were worthless because they were 'only pieces of paper'. The linguistic parallel here is the view that speech itself is meaningless because spoken words are just vocal noises. It was widely feared that if the Keynesian view prevailed and the so-called 'gold standard' was abandoned, then the honest savings of millions of poor working people would automatically become valueless. So the first task in the Keynesian demythologization of economics was to argue that nothing disastrous would follow from coming off the 'gold standard', because the so-called 'gold standard' was itself part of a myth about finance.

One naïve reaction to arguments against the telementational model is to ask what alternative explanation can be proposed of how it is possible for human beings to convey their ideas to one another. But those who demand this alternative account merely demonstrate how completely bemused they are by the language myth in question. It is rather like asking a Keynesian what alternative standard should replace the ‘gold standard’: perhaps a ‘silver standard’, or a ‘copper standard’? The point of the term *demythologization* here is that there is no question of an alternative account. Myths do not have alternatives. To ask for an alternative is a sign of having failed to recognize the myth as a myth.

There is one last point worth making in connection with demythologization. Myths cannot be shown to be false, because myths are never founded on propositions which were demonstrable in the first place. Keynesian economics did not demonstrate that ‘gold standard’ economics was wrong, but merely that faith in the ‘gold standard’ was unnecessary, unhelpful, and in various ways obfuscating and harmful. The Keynesian strategy is to point out that the assumption that currency notes are pieces of paper standing for quantities of precious metals fails to make sense of economic reality, where in practice money functions as a complex of mechanisms which facilitate the distribution of goods and services. Money does not in addition need to ‘stand for’ anything. Analogously in the linguistic case, once we see that language can be treated as a complex of mechanisms for facilitating communication there is no need to insist that linguistic signs ‘stand for’ anything else in addition. Nor need anyone fear that a linguistics which abandons the writing of grammars and dictionaries has abandoned linguistic inquiry altogether. On the contrary, it is only when linguistics has advanced beyond the grammar and the dictionary that the serious business of linguistic inquiry will have begun.

Postscript

There seems to me little that needs altering if I were re-writing this essay today. I would wish to reiterate what I describe as its ‘basic contention’, i.e. that the fundamental error in contemporary linguistics is still the mistake that Saussure made about the possibility

of segregating *faits de langue*. I therefore prefer to leave the original text as it stands, and add a few endnotes. (A fuller discussion of some points can now be found elsewhere. On integrationism and the structuralist legacy, see Harris 1999. On speech and writing, see Harris 1995 and Harris 2001a. On telementation and the role of the language myth in the Western tradition, see Harris 2001b.)

1. *The term 'integrational'*. Since the paper first appeared, rather more than a decade ago, I have given up using the term *integrational* to designate the third of the three scales or parameters pertinent to the analysis of human communication, because it now seems to me potentially misleading. While retaining *biomechanical* and *macro-social* for the other two, I now prefer to call the third dimension *circumstantial*. I define circumstantial factors as those which 'relate to the specifics of particular situations' (Harris 1998: 29). Thus, for instance, *when* and *where* an interaction is taking place would be circumstantial factors that cannot be ignored by the analyst. Some circumstantial factors are continually subject to change as a given communication situation develops: e.g. what was just said may, and often does, affect what will be said next. By contrast, biomechanical and macrosocial factors tend to remain more stable throughout the duration of a communicational episode. (So-called 'code-switching' in bilingual conversations would be an exception.)

2. *An objection to the 'non-compartmentalisation' principle*. The greater part of the above paper is devoted to spelling out the reasons behind the integrationist 'non-compartmentalisation' principle (Harris 1981: 165), i.e. arguing that, contrary to what was assumed in orthodox linguistics for most of the nineteenth and twentieth centuries, there is *no* defensible dividing line between the linguistic and the non-linguistic, whether drawn in biological, developmental, cultural or any other terms. The unpalatable consequences of this principle for orthodox linguistics are obvious enough. At one stroke it invalidates the orthodox assumption that linguists can – and should – get about their business by investigating what context-free synchronic 'systems' of forms and meanings underlie the linguistic behaviour of members of a linguistic community. The integrationist counterclaim is that there is no way of identifying 'linguistic behaviour' which distinguishes it, systematically and across all variations in conditions, from its non-linguistic or pre-linguistic

environment. There is not even a plausible ‘standard case’ scenario, valid for most communication situations, by approximation to which linguistic behaviour might be defined.

Recently, however, I have encountered an objection to the ‘non-compartmentalisation’ principle which takes roughly the following form. *Objection*: ‘Surely most intelligent people both can and do routinely distinguish between (i) using words and (ii) communicating by other means, even when verbal and non-verbal communication are taking place concomitantly. And that alone justifies linguists in treating the distinction between language and non-language not only as a real distinction (as distinct from an artifact of analysis) but as the indispensable foundation for any science of linguistics.’

The kind of example that is sometimes produced in support of this objection is the following. Take any well-known song. Most of us will have little difficulty in sorting out the words from the music. We can write down the lyric for anyone interested, without thereby identifying the tune. Conversely, we can also – if we are skilled musicians – write down the tune in musical notation, without thereby giving any indication of the words. The two operations reveal totally separate components, one linguistic and the other non-linguistic. Furthermore, the objection continues, these two components are cognitively distinct. Remembering how the tune goes is not the same as remembering the words, nor vice versa. So in practice the non-compartmentalisation principle is contradicted by the plain facts of everyday experience.

This would be a powerful anti-integrationist argument *if it were relevant*. But taking the case of song completely misses the point. Separating out the words from the music is fine as far as it goes, whether we do the separation in the mind or on paper, and regardless of whether we ‘store’ the two in separate compartments of the memory; but it is a separation that destroys the cultural phenomenon itself, i.e. the song. Although it is doubtless right in some sense to say that the words will be what is left of the song if the music is subtracted, and likewise for the music when the words are suppressed, that is rather like saying that if we take the oxygen out of water what will be left is hydrogen, and if we remove the hydrogen what will be left is oxygen. But either subtraction leaves us with nothing recognizable as water in one case or song in the

other. The properties of water are not a combination of the separate properties of hydrogen and oxygen. Water is not just an unusual form that hydrogen and oxygen sometimes take. Nor is a song just speech delivered with curious but pleasant distortions of pitch and syllabification. In other words, as regards our first-order experience of both water and song, they *do not exist* other than as integrated phenomena: the integration is essential to their being what they are. It is to phenomena of this kind (in our cultural world) that the non-compartmentalisation principle applies. And what the integrationist claims is that it applies to language.

The dogged objector, not yet satisfied, may well point out that poems, which start life as words, are often set to music, and so become songs. The author of the lyric and the composer of the tune may be different individuals. Is this not ontogenetic proof that the resultant song is rightly considered an authentically bi-partite entity?

But again this misses the point. It makes no difference whether the lyric-writer fitted the words to the tune, or whether the writer of the score set a pre-formed lyric to music, or whether it was all put together *ambulando* by some process of collaboration or trial and error. *We are not trying to reconstruct the original process of composition.* What makes the song a song is the way words and music are integrated, and that integration is something more than a matter of matching and delivering simultaneously two independent items, i.e. a sequence of syllables and a sequence of notes. To suppose that *that* was the sum total of the art would be at best to conflate the song with the singing. But singing a song requires a quite different performance, as every trained singer knows. In song, the notes audibly *embody* the words, and by so doing verbalize the tune. This is not a simple matter of ‘associating’ certain words with a certain tune, or vice versa, but of fusing the two into a single creative form. Or, as an integrationist might prefer to put it, the sign of song is a sign *sui generis*: it is not the mathematical combination of a linguistic sign plus a musical sign, even though the song involves both verbal and musical forms of expression.

The mistake in taking the song as a counterexample to the non-compartmentalisation principle involves a confusion of analysis with synthesis. It is rather like supposing that because it is possible with a camera to take a black-and-white photograph of the *Mona*

Lisa, this demonstrates that Leonardo's painting just *is* the configuration revealed in black-and-white, only with colour added. Or like supposing that because it is possible to identify the magnitude of the angles of a triangle without reference to the length of the sides, and vice versa, it follows from this that drawing a triangle involves executing two separate geometrical operations, viz. drawing the angles plus drawing the sides.

The sceptic might nevertheless press the question: 'But if there is *no* possibility of extracting bits of language from the non-linguistic substrate in which they are embedded, how is it that we manage so easily to identify the words of the song as distinct from the music, and show no tendency at all to confuse the two?'

The integrationist answer is that recognizing the words of a song is analogous to recognizing 'the same face' or 'the same shape' under various projections or visual distortions (as e.g. in caricature). Whether it is indeed 'the same' is a moot point, for the recognition is actually one of family resemblances (Wittgenstein 1958: §67). Similarly, what we are exhibiting when we identify the words of the song, as distinct from the music, is our (macrosocially trained) ability to classify certain auditory patterns as 'the same words', whether they are spoken or sung. Apart from begging a crucial question about 'sameness' (and some of us find it far from easy to identify 'the words' as screamed by pop groups currently in vogue), that is a far cry from proving that singing the song consists of two activities, one linguistic and one non-linguistic.

Presumably pattern-recognition does not in any case supply *eo ipso* a sufficient condition for distinguishing one mode of communication ('language') from all others. For if that were so, then the theorist would need to consider every participant as a special case, since pattern-recognition varies across individuals; with the result that the distinction between language and non-language would have to be defined differently for Smith and for Jones. But *that*, patently, is the last conclusion that orthodox linguistics would be willing to welcome, since it undermines the basis of fixed-code communication.

3. *Bridging the gap*. Some linguists try to play down the problem involved in distinguishing between language and non-language by setting up various kinds of intermediate buffer-zone. Thus

pragmatics, according to some, is a field immediately adjacent to linguistics, where one studies how linguistic knowledge and extralinguistic knowledge interlock in the production of successful communication. Speech-act theory is a similarly intermediate domain.

A whole intermediate category sometimes postulated is ‘para-language’, variously defined by different authorities, but always designed to accommodate a recognition that the sound signals of the human voice in speech could not plausibly be claimed to be carrying verbal information *alone*. Thus a tone of voice or manner of delivery indicating the speaker’s surprise, contempt, or some other attitude, was typically treated as not ‘linguistic’ but as ‘paralinguistic’. Such a distinction, however, obscures the compartmentalisation problem rather than resolving it. As in the case of song, the fact that the speaker might, if asked, repeat ‘the same words’ but without the ‘surprise’ or ‘contempt’ effect does not somehow prove that the original utterance must have comprised two acts, i.e. a linguistic act plus a paralinguistic act. Are there utterances which realize ‘linguistic’ sounds only and nothing else? It would be interesting to know exactly what such ‘pure’ unadulterated utterances sound like, and where one can hear them; just as it would be interesting to know how to write without having your ink make marks of any particular colour, or thickness, or size; or how to make eye contact with another person without thereby signalling anything to that person at all.

From an integrationist perspective, all such attempts to bridge the gap between language and non-language leave the basic problem unresolved.

4. *Other ‘redefinitions’ of linguistics.* Integrationists do not claim to be the *only* faction calling for a fundamental rethink of linguistics. But it is important not to confuse the integrationist position with that of neo-positivists, such as Yngve. Yngve (1994), in the wake of many others, casts serious doubts on the possibility of defining such basic terms in the orthodox vocabulary as *phoneme*, *noun*, *verb*, *word*, and *sentence*. He himself holds that the term *utterance*, as used by linguists, reflects an ‘illusion’ which arose by ‘normal processes of projection and externalization, perhaps also partly under the influence of writing’. But, whatever the source of

the illusion, he is convinced that ‘there is no such thing [as an utterance] in nature’. What exists in nature is ‘only the physical sound waves themselves and the people producing, sensing, and interpreting them’. Having spotted the illusion, Yngve is led ‘to doubt the scientific validity of the entire conceptual framework and system of grammatical terminology developed to talk about utterances, their parts and their properties’. He thinks linguists should ‘consider giving up the traditional concepts and seek a new more scientifically acceptable foundation for linguistics.’

With much of this an integrationist might feel inclined to agree. But in a way it is all *déjà vu*, because Saussure himself complained (i) that linguists are condemned to working on the basis of concepts originally devised by (traditional) grammarians, and (ii) that language (*langage* as opposed to *langue*) is unknowable (*inconnaissable*) because it involves taking in so much more than a single discipline can possibly encompass. These observations already voice proto-integrationist concerns. But, like Saussure, Yngve never gets as far as identifying the problem underlying *all* the traditional and current metalanguage deployed in linguistics, i.e. reluctance to recognize the context-dependence of its applications. For integrationists, Yngve’s assumption – that what exists linguistically ‘in nature’ is only ‘physical sound waves’ plus various operations conducted on those sound waves by human beings – is a pointer leading to further blind allies in linguistics. (One might as well declare a glorious new start in the theory of painting by declaring that all that exists ‘in nature’ is a conglomeration of splodges of paint on a surface, plus how these are ‘seen’ by viewers.)

5. *The boundaries of language.* Those linguists who have in the past been uneasy about the arbitrariness with which theorists tried to distinguish language from non-language did not come up with anything which approximates to a non-compartmentalisation principle. Occasionally one finds an admission that ‘there is no sharp distinction between language and non-language’ (Lyons 1972: 77). But what the right hand seems to offer is immediately snatched back by the left. For then we are told that, in spite of the absence of any sharp distinction, nevertheless certain properties such as grammatical complexity are unique to language. ‘If we decide to make the possession of these properties a defining characteristic of

what we will call “language”, we will then say, and correctly, that non-verbal communication is “radically”, or “fundamentally”, or “qualitatively” different from language’ (Lyons 1972: 77). The question of whether we opt for this is dismissed as ‘purely definitional’.

As an example of trying to eat one’s cake and have it, this could hardly be surpassed. First, we are told that there ‘is’ no distinction (or at least no ‘sharp’ one). But next it appears that there is after all a distinction (even a ‘sharp’ one) if we choose to make it. But why anyone should choose, or might choose, to make it is a question never pursued. The consideration of possible reasons for making such a choice is not allowed to arise. Worse still, if we are to take seriously the initial claim that there ‘is’ no such distinction, it is difficult to see how it is possible to identify properties such as grammatical complexity as being ‘unique to language’. At most one could recognize them as being unique to certain forms or modalities of communication. The leap from this to a retrospective decision by the linguist to erect these properties into *criteria* for language in general is a move that baffles comprehension.

6. *Language versus languages.* Setting up linguistics as an autonomous discipline in the nineteenth century required – and presupposed – an autonomous subject matter. From Max Müller onwards, scholars committed to this thesis of academic autonomy struggled with the task of explaining exactly what this subject matter (‘language’) was, and how it differed from the detailed one-by-one examination of the various languages known to have been used, at different times and places, in different quarters of the globe. Max Müller once said, in a famous lecture, that linguists *qua* linguists do not seek to know *languages*, but to know *language*.

What exactly was the difference? The Neogrammarian answer was to define the domain of linguistics as that within which certain historical regularities could be detected. The regularities in question were claimed to be correlations between ideas and sounds. Thus Hermann Paul could say that only two exact sciences were necessary ‘as the basis of the science of language, viz. psychology and physiology, and of the latter certain portions only’ (Paul 1891: xlii). (Paul’s apparent assumption that in the 1880s psychology already qualified as an exact science is itself astonishing. Unless we

interpret this as a caveat which implies that a science of language will only be possible when psychology itself qualifies as a science.) Paul had no qualms in admitting that his view of linguistics by no means included ‘all the physiological processes which belong to linguistic activity’. It excluded, for example, ‘the excitement of the motor nerves whereby the organs of language are set in motion’. He thought that this deliberate restriction to correlations between ideas and sounds gave linguistics a great advantage over a more complex subject such as ‘economic science’, where the investigator had to deal with the ‘reciprocal operation of the entirety of physical and psychical factors with which mankind enters into any operation whatever’. In such a science, he added, ‘the most earnest endeavours will never succeed in expounding with absolute accuracy the part played by each single one of these factors in the process’ (Paul 1891: xliii). Paul is here trapped in the web of his own logic. For if it is true that in human life a whole host of factors enters into ‘any operation whatever’, that must necessarily apply to linguistic operations too (howsoever these latter might be defined). Therefore to refuse to consider more than a restricted number of factors was already to cut linguistics off from any possibility of becoming a ‘science of language’ in the fullest sense. One might just as well – and just as self-defeatingly – have decided to restrict economics to considering only the correlations between income and expenditure. Or agriculture to the correlations between crop yield and sowing. But the Neogrammarians tacitly assumed that, provided there were enough attested cases that supported postulating evolutionary ‘laws’, that in itself was enough to back the claim that linguistics was already a successful science (on a par with physics, geology, etc.).

Paul’s implicit argument is clearly reductionist in spirit: i.e. we can *make* linguistics scientific by deliberately restricting it to the consideration of certain correlations, which we (linguists) will determine. The integrationist response to this – as to any latter-day versions of this kind of thinking – is simple: go ahead and impose any restrictions you like, but don’t tell us that makes your enterprise ‘science’. A science constructed by choosing to wear blinkers is going to leave the world poorer, not richer, than having no science at all. We shall understand less about language, not more, by refusing

to consider whatever is not amenable to rules, laws and other forms of metalinguistic systematization allegedly characteristic of 'scientific' thinking. Why? In the first place, because focussing on the supposedly 'scientific' systematizations and explanations will in practice impede a grasp of language which would be the surer if left unhampered by all the mumbo-jumbo that invariably accompanies them. But in any case because most of what has passed for a science of language since Paul's generation is actually the updated recycling of an ancient language myth that goes back to the beginnings of the Western tradition.

7. *Language as inductive generalization.* Another possible answer to the conundrum about 'language' versus 'languages' was to say that the difference was no more than a matter of inductive generalization. In other words, having studied many particular languages, the 'general' linguist would then be in a position to enunciate truths that held for them all. Such generalizations were later called (by some) 'universals'. A well-known theorist who held this inductivist position was Leonard Bloomfield, from whom we have the much-quoted pronouncement: 'The only useful generalizations about language are inductive generalizations. Features which we think ought to be universal may be absent from the very next language that becomes accessible' (Bloomfield 1935: 20). This envisages a situation (perhaps permanent?) in which the linguist is never quite sure that the tally of 'languages' is complete. A 'rogue' language, newly discovered, can overturn any number of previous assumptions about 'language'. This point of view, however, presupposed a philosophy of science that became less and less acceptable to many as the twentieth century proceeded.

One reason why it became less acceptable can be traced to the underlying metaphysics involved in Bloomfield's notion of 'a language'. Bloomfield stated baldly: 'Writing is not language, but merely a way of recording language by means of visible marks. [...] A language is the same, no matter what system of writing is used to record it, just as a person is the same no matter how you take his picture' (Bloomfield 1935: 21). This disconcerting analogy between a language and a person exposes the reification of languages which lies at the basis of Bloomfieldian thinking. Languages become 'objects' available for inspection, having an existence of their own,

over and above the existence and activities of the linguistic communities which support them.

8. *Language and the computer.* As linguistics today tries desperately to adapt to the realities of life in an increasingly computer-dominated culture, it is interesting to observe how the current generation sees the defining characteristics of ‘language’. Probably most people living in the computer age are now aware of the metalinguistic takeover by which Cobol, Algol, Basic etc. were overnight promoted to the status of ‘languages’. (There is an anecdote to the effect that one senior American academic, having failed a ‘foreign language’ test, was allowed by the university to substitute a qualification in computational languages.) This promotion trades on, and simultaneously reinforces, the notion that all languages are simply fixed codes. Simultaneously, there was much talk of one machine ‘communicating’ with another. In effect, the distinctive contribution that human beings bring to communication was being taken off the ‘scientific’ agenda. French and Fortran were now on an equal footing.

Given that equality, one property that comes up for discussion is what Drucker calls linguistic ‘fungibility’. What is drawn to our attention is the way the computer has extended the application of this concept (and hence, by implication, our notions of what language is or might be).

The nature of language is such that information stored in linguistic form is basically fungible; that is, the form the information takes is interchangeable and mutable. A statement rendered in manuscript hand, in letterpress type, or in typewritten or electronic script can essentially contain the same linguistic message – with the major qualification that the material form in which written language appears encodes that message. The ‘information’ quotient of the material can vary from negligible to highly significant. (A handwritten grocery list and a typewritten one will probably result in the same items being bought while a hand-written stop sign has a completely different status from an officially produced and sanctioned one.)

(Drucker 1998: 221)

Here it is already obvious that there is no longer room for the fuddy-duddy notion that writing is not language. (Integrationists duly applaud.) On the contrary, the pendulum has swung to the opposite extreme: the properties of writing are regarded as characteristic of language. (Integrationist applause subsides.) The passage quoted continues as follows:

But the way this fungible character of writing functions is transformed dramatically in the age of electronic media. This is because for the first time, the encoding of a linguistic message into writing does not have any material stability. Texts were always fungible it's true – there was always a moment in which texts slipped from one form to another, from manuscript to print, from one edition to the next. And there was a moment of suspended existence when the language was held in the mind of any fresh compositor outside of written material form. But the actual documents had a material character: a sentence rendered in handset Garamond foundry type and printed letterpress would retain that material information as part of its linguistic existence until it was re-rendered or transformed. But every written instance of the message would bear within it a whole history of its execution in the codes of the material in which it was embodied. In an electronic environment, entering a sentence in Garamond, Times, Matrix or Hobo is in no way a permanent aspect of its existence as information. The keystroke commands, transformed into electronic code, are fungible in a whole new way: the mutability of the form the written language takes is increased radically. There is, simply, no longer any necessary relation between the input form of the written message and its output. There is nothing that links the characteristics of input and that of output in a material support or form. Stored electronically, the materiality of written language becomes a fungible factor – up until the existence of electronic media, the materiality of written language was part of what allowed it to function culturally.

(Drucker 1998: 221–2)

Integrationist readers will doubtless wince here at the question-begging deployment of such terms as *code* and *information*. Clearly the old ‘sender (encoding) – receiver (decoding)’ model of communication flourishes vigorously in its new IT habitat. But more relevant for our present purposes are the uses of the terms *language* and *linguistic*.

‘Language’ is here envisaged as being an abstract system which has no fixed form of its own, but nevertheless permits the formulation of messages in an endless variety of material realizations, and which, in some magical way that remains unexplained, maintain an absolute linguistic identity from one form to the next. Although the writer of the passage shows no sign of realizing it, the conceptual model here deployed was anticipated half a century ago by Hjelmslev, who lived in a pre-computer age. The theoretical problem with Hjelmslev’s model is obvious enough (Harris 1998: 111–15) and in linguistics one might have thought it had long been laid to rest; but here it is popping up again with the powerful backing of the new electronic media. Powerful because here, it would seem, we see that model actually *working* in mechanical form. It is no longer mere theory, but actuality.

But it is really? The interesting question is this: what is it about the mechanics of electronic writing that would lead anyone to posit ‘fungibility’ as a basic characteristic of *language*? The answer is already clear from the passage quoted above. It is nothing more than that *physically* the machine does not need to preserve written texts in anything like the visual format in which they were originally entered. (In fact, if it had to do that, it would not be a computer.) But this proves nothing about language at all (other than that, trivially, conventional writing takes a visual form); for it applies equally to *any* visual material the machine is capable of dealing with. In short, here we have another case where the properties of the medium are confused with those of the message. It remains only to add that this has been happening with language for centuries: the structure of alphabetic writing being attributed to speech remains the classic example.

In any case, there is nothing new here. What there is was already recognized by proponents of ‘information theory’ in the 1940s–50s. Shannon’s probabilistic account of ‘communication’, although

doubtless satisfying the requirements of electrical engineers, assumed *ab initio* that ‘messages’ could be transformed by some unspecified process into ‘signals’ which retained no *physical* properties of the items into which they were originally ‘encoded’. *Plus ça change, plus c’est la même chose.*

9. *Rethinking linguistics.* Rethinking linguistics means rethinking language. Rethinking language means rethinking languages. Rethinking both means rethinking metalanguage.

Integrationism proposes a precise theoretical basis on which all this rethinking might be done, i.e. the axioms of integrational semiology (Harris 1996a: 154). How much of the rethink integrationists have so far succeeded in doing is another question. But it is already clear that it cannot be accomplished by piecemeal tinkering with the terminology already in use. The interest that attaches to the terminology already in use is what it reveals about assumptions underlying the Western perspective on communication (Davis 2001).

I have explained elsewhere (Harris 1996b) why I do not think anyone can outlaw the metalinguistic games we all play with language. Nor do I think integrationists can propose a ‘superior’ or ‘more scientific’ metalinguistic game to play. I hope readers will pardon me for concluding immodestly with self-quotation (an interesting example of the potentialities of linguistic reflexivity), but I cannot at the moment think of any better way of putting the point I wish to make. The aim was ‘never to call a halt to the metalinguistic games we play when we inquire into language, but to prevent the fact that we play them as we do from giving rise to metalinguistic illusions’ (Harris 1996b: 148).

The reason why professional linguistics needs rethinking, from top to bottom, is that in its current form it not only allows, but actively propagates, pseudo-scientific illusions of every kind. These illusions are the stuff that so many academic careers in Western university departments of linguistics are made of. (I also believe that some of these illusions promote or support socially and ethically objectionable ideas, but that is an issue which cannot be pursued here and is in any case not essential to the integrationist critique of orthodox linguistics.) A persistent, self-serving refusal to question publicly what one was taught: that is the ultimate *trahison des clercs*. Not only in Western civilization, but in all civilizations.

By insisting that linguists must face the challenge of recognizing that human beings do *not* live in a communicational world that is neatly and permanently compartmentalised into language and non-language, and by holding it important to inquire into the various educational, social and political purposes which attempts at compartmentalisation subserve, integrationists automatically incur the wrath, scorn or dismissal of many of their academic colleagues. Whether that condemnation is deserved is another question, to which history will doubtless in due course provide one kind of answer. What integrationists are saying is that only by taking on board the full implications of non-compartmentalisation can linguistics avoid going the way of alchemy, phrenology and similar prestigious studies that made the mistake of claiming to be sciences when they were not.

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RETHINKING THE FUNDAMENTAL ASSUMPTION OF LINGUISTICS

Nigel Love

In *Linguistic Form*, C. E. Bazell makes some remarks on the relation between structural linguistics and linguistic description:

It is commonly supposed that one of the chief aims of structural linguistics is that of providing new techniques of grammatical description. It was not left to the twentieth century structuralists to discover the means of describing a language adequately enough for all practical purposes, including the practical purposes of the professional linguist: good grammars of the last century do not convey less information, or even less relevant information, than their most modern counterparts. But it is held that there was something gravely amiss in the method of presentation. The method was not *uniform*.

Indeed a good many methods were used. One was the analytic method, which was pursued with tolerable consistency down to the level of the word (an undefined unit). Below this level some analysis (for instance, the stem-suffix divisions in the chapter on word-formation) was undertaken. More often however the student was presented with 'paradigms', i.e. with tables of related words arranged after their semantic or syntactic function, on the model of which other words could (within stated limits) be formed. For

exceptional formations there was neither analysis nor paradigm, but simply an important example or two, with the indication that others might be expected. Finally one was warned that grammar did not replace the reading of texts, which were generously cited in the grammar itself.

This was all very useful, but it did not conform to the modern notion of a scientific grammar. Different levels were not carefully distinguished, and the same fact might turn up, in rather diverse guises, at distinct stages of the description. This was not found to be confusing at the time: it was left to a later age to discover that there was any confusion at all. Is it not the task of structural linguists to dispel the confusion?

But surely it is more important to distinguish between the tasks of descriptive grammar and those of structural linguistics, lest there may be confusion here. An interest in linguistic structure implies an interest in the description of language-structures, though it is not quite the same thing. But the task of descriptive grammar is not that of describing a language-structure; it is quite simply that of describing a language. A language is not necessarily to be regarded as a single structure, or even as a complex of structures similar enough to be all described in the same way.

One man is interested in city-structure, and another is interested in the description of cities. One may learn from the other. Very often no such learning is necessary: if a city is built in the form of a square, this is not more likely to escape the eye of the descriptionist than that of the structuralist. If the squareness of the city is merely approximate, the structuralist may notice this sooner. This is the sort of thing he is looking for . . .

. . . Part of our city might show no well-defined structure at all. One might have to rest satisfied with indicating the positions of the individual buildings. The describer of a language, in similar cases, refers to the dictionary; while the structuralist can have little to say

(Bazell 1953, pp. 102–104)

Twentieth-century structural linguists have very often proceeded on the basis that in analysing linguistic structures they are describing languages, or at any rate elaborating methods of linguistic description. Some structural linguists are even commonly called 'descriptivists'. Bazell's reminder that these are two different enterprises may be taken as the thin end of a wedge which, if driven in far enough, might point the way to a more satisfactory conception than has so far been offered by any school of structural linguistics of the relations between languages, linguistic structures and the human beings who must on any account of the matter be somehow implicated in creating and reproducing them. For if a language is not necessarily to be regarded as a single structure, or a complex of similar structures, and may in parts have no well-defined structure at all, it is natural to ask how this can come about, granted the usual assumption that to speak a language is, precisely, to implement the structure that the structuralist perceives in the utterances he takes as data. In fact, it raises the possibility that what a language is, and what it may be to speak one, have been altogether misconceived.

To be sure, the conflation of the two enterprises has sometimes been seen to require a special conception or technical definition of a language (for example, Saussure's *langue*), in order to eliminate from consideration those aspects of linguistic phenomena unamenable to the analytic techniques employed, and this may require the theorist to concede that, strictly, nobody actually speaks or knows what has been defined as 'a language'. But even then it has usually been felt necessary to bring languages and the stipulatively defined objects of structuralist study into some plausible relationship, for instance by claiming that the latter are scientifically necessary abstractions from languages, or features of languages as viewed from a certain angle. (*Langue*, after all, is just the ordinary French word for 'a language', and sometimes has to be understood in that sense in the very text in which it is explained and used as a technical term.) In short, it is taken for granted that the structures structural linguistics deals with are in some sense real features of real languages as spoken by real people, or, at the very least, in some intimate relation with these realities.

The structures in question are units of linguistic form (let us call them 'structural units') and their patterns of distribution in

utterances taken to belong to some (spoken) language. At both levels of articulation a language consists, or may be taken to consist, of structural units. Structural units at the meaningful level of articulation are commonly characterised in the two dimensions of ‘form’ and ‘meaning’.

The structuralist’s most fundamental claim, then, is that utterances recurrently instantiate structural units in recurrent distributional patterns. But all utterances are unique. There must therefore be some sense or senses in which superficially different utterances count as ‘the same’. One structuralist who recognised the foundational nature of this point was Leonard Bloomfield. Indeed, he took it to be ‘the fundamental assumption of linguistics’.¹

Bloomfield’s fundamental assumption is on the face of it more worthy of attention than the foundational assertions of Saussure, who talks of ‘un système de signes où il n’y a d’essentiel que l’union du sens et de l’image acoustique’ (Saussure 1922 p. 32),² located ‘dans chaque cerveau, ou plus exactement dans les cerveaux d’un ensemble d’individus; car la langue n’est complète dans aucun, elle n’existe parfaitement que dans la masse’ (p. 30), or of the early Chomsky, who says that ‘linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly’ (Chomsky 1965 p. 3). Eschewing such idealisations, Bloomfield starts from a simple statement allegedly about real speech-communities, and presents it as a proposition whose ‘general truth is presupposed not only in linguistic study, but by all our actual use of language’ (Bloomfield 1935 p. 145). Assertions about communal minds or ideal speakers are only worth entertaining if they are provisional stand-ins, steps on the way to saying something about real minds and real speakers. Whether those who have founded linguistics on such assertions could even in principle make good on the implied promises is doubtful. By talking from the outset of ‘our actual use of language’, Bloomfield bypasses epistemological uncertainties of this order. Moreover, he seems to promise an approach to language structure that starts by establishing what the structural units actually are, rather than demanding that they fit into the Procrustean bed of a general *a priori* definition.³

The fundamental assumption makes the first of two appearances in *Language* in the course of a discussion of phonology:

Speech-utterances are infinitely varied. Everyday experience tells us that different persons speak differently, for we can recognize people by their voices. The phonetician finds that no two utterances are exactly alike.

Evidently the working of language is due to a resemblance between successive utterances. Utterances which in ordinary life we describe as consisting of 'the same' speech-forms – say, successive utterances of the sentence *I'm hungry* – evidently contain some constant features of sound-wave, common to all utterances of this 'same' speech-form. . . The phonetician, however, cannot be sure of these constant features, as long as he ignores the meaning of what is said. . . Suppose, for instance, that he had records of an utterance which we could identify as representing the syllable *man*, spoken on two different pitch schemes. If the language of these utterances were English, we should say that both contained the same speech-form, namely the word *man*, but if the language were Chinese, the two records might represent two different speech-forms, since in Chinese differences of pitch-scheme are connected with different meanings . . .

. . . the working of language depends on our habitually and conventionally discriminating some features of sound and ignoring all others. . .

To recognize the distinctive features of a language, we must leave the ground of pure phonetics and act as though science had progressed far enough to identify all the situations and responses that make up the meaning of speech-forms. In the case of our own language, we trust to our everyday knowledge to tell us whether speech-forms are 'the same' or 'different'. . . Thus, we find that the word *man* spoken on various pitch-schemes is in English still 'the same' word, with one and the same meaning, but that *man* and *men* . . . are 'different' words, with different meanings. . .

. . . Phonology involves the consideration of meanings. The meanings of speech-forms could be scientifically defined only if all branches of science . . . were close to perfection. Until that time, phonology and, with it, all the

semantic phase of language study, rests upon an assumption, the fundamental assumption of linguistics: we must assume that *in every speech-community some utterances are alike in form and meaning*.

(Bloomfield 1935 pp. 76–78)

What exactly does the assumption mean? We can easily dispose of some trivial difficulties of interpretation. For instance, the word ‘likeness’ is perhaps not well chosen; as the context suggests, ‘likeness in form and meaning’ is not to be construed in such a way that utterances of, say, the English words *car* and *cart* would count as ‘alike’ in the relevant sense. Likeness, here, is not merely a matter of phonetic and semantic similarities. The likeness in question is in fact sameness: two utterances are ‘alike’ when they are ‘the same’. Then again, ‘some utterances’ does not imply that there are other utterances that are unique in being ‘unlike’ any others. On the contrary, it means that every member of the (infinite) totality of possible utterances in a speech community is ‘like’ some other members, i.e. is the same as some other members, in respect of form and meaning.

But these are just ungainlinesses of expression.⁴ There are deeper problems, epitomised in the passage quoted by incompatible uses of the term ‘speech-form’. When Bloomfield talks of ‘utterances which in ordinary life we describe as consisting of “the same” speech-forms’, this seems to be his equivalent to talking of utterances instantiating the same structural units. That is, a speech-form is an abstract invariant manifest in speech in indefinitely many non-distinctively different utterances. Speech-forms are more abstract than utterances. But then he remarks that ‘we trust to our everyday knowledge to tell us whether speech-forms are “the same” or “different”’. Now it seems that utterances just are speech-forms. Because if speech-forms are structural units, the question whether they are the same or different cannot arise. By definition, the members of any set of structural units in a language are ‘different’ from one another: the whole point of recognising them is to articulate a system of distinctive differences. You can ask whether two phones are the same (i.e. only non-distinctively different) or different (i.e. contrastive). But to ask whether two *phonemes* are different makes no sense.

These two readings of 'speech-form' yield two quite different fundamental assumptions. If a speech-form is a structural unit, then the fundamental assumption is, simply, that there are structural units, manifest in the utterances that are alike (the same) in both form and meaning. On this reading, the fundamental assumption implies that the first step is to disengage from utterances samenesses of form that are correlated with samenesses of meaning, and thereby establish what the (meaningful) structural units are. But if an utterance is already, in and of itself, a speech-form or forms, the fundamental assumption has to be read as being merely that speech-forms (that is, structural units considered in their formal aspect) are systematically associated with meanings. On this reading, some utterances (wearing on their faces their identity as the *forms* of structural units) are the same in meaning as well as in form.

On the first reading neither the formal nor the semantic analysis of utterances is given. All utterances are *at least* non-distinctively different from one another in form and in meaning: the task is to say which utterances are *only* non-distinctively different in *both* form and meaning, and thereby establish the structural units. On the second reading the formal analysis of utterances is given. All utterances are non-distinctively different in both form and meaning, but which differences of form are distinctive is already known. In this case the fundamental assumption is that if utterances are only non-distinctively different in form, they will be only non-distinctively different in meaning too.

The first-reading assumption is weaker, in that it assumes less about the structural analysis (*qua* identification of structural units) that follows from it. The second-reading assumption is much stronger. It incorporates the first and, additionally, assumes that structural units (*qua* forms) come to us already identified in utterances themselves. Let us refer to them as the w- and s-assumptions respectively.

But perhaps all this is being erected on a mere lapse of terminological care on Bloomfield's part. Outside the passage in question he rarely uses 'speech-form', and in fact it is difficult to see what terrain is left for it to occupy alongside the more frequent, overtly defined terms 'phonetic form' and 'linguistic form'. A phonetic form is 'any combination of phonemes that occurs in a language, and is *pronounceable* in this language', while

A phonetic form which has a meaning, is a *linguistic form*. Thus, any English sentence, phrase, or word is a linguistic form, and so is a meaningful syllable, such as, say [mɛl] in *maltreat*, or [mʌn] in *Monday*; a meaningful form may even consist of a single phoneme, such as the [s] which means ‘more than one’ in plural-forms like *hats*, *caps*, *books*.

(Bloomfield 1935 p. 138)

‘Speech-form’ as used to give grounds for the weak reading of the fundamental assumption might as well be replaced by ‘utterance’, while in its s-assumption sense it seems equivalent to ‘linguistic form’. But in any case, the question which fundamental assumption Bloomfield is making arises irrespective of which of various collocations or compounds containing the term ‘form’ he uses on a particular occasion. For, as will emerge, his concept of a (meaningful) form is radically problematic.

So what *is* Bloomfield’s fundamental assumption? No sooner has it made its second appearance in *Language* (p. 144) than he is conceding that it is not strictly true. What can be gleaned from his reasons for saying so?

The alleged stumbling block is the existence of homonyms:

Since we cannot with certainty define meanings, we cannot always decide whether a given phonetic form in its various uses has always the same meaning or represents a set of homonyms. For instance, the English verb *bear* in *to bear a burden*, *bear troubles*, *bear fruit*, *bear offspring*, can be viewed as a single form or as a set of two or perhaps even more homonyms. Similarly, *charge*, in *charge the cannon with grapeshot*, *charge the man with larceny*, *charge the gloves to me*, *charge him a stiff price*, can be viewed in several ways; *the infantry will charge the fort* seems to be different. The quality *sloth* and the animal *sloth* probably represent a pair of homonyms to some speakers and a single meaning to others. All this shows, of course, that our basic assumption is true only within limits.

(Bloomfield 1935 p. 145)

On the w-reading, it is hard to see why homonyms, and the fact that different speakers may have different views as to where there is homonymity, should show that the assumption is true 'only within limits'.⁵ Utterances with phonetic shapes transcribable as [sləʊθ] and produced in contexts where lazy torpor is in question will *pro tanto* be utterances alike in form and meaning. Zoological utterances containing a form transcribable in the same way will likewise be utterances alike in form and meaning. This applies irrespective of any view speakers may take as to whether there are two homonymous words *sloth*. Bloomfield's dilemma seems to be whether to say that there are two distinct sets of 'like utterances' here, in that the meaning of *sloth* is different in the two kinds of context, or only one, in that *sloth* has a 'single meaning'. But if this is a problem, it is not one that arises from the w-assumption itself.

There are steps missing from the argument, as is apparent from the immediately preceding statement (p. 145) that

our fundamental assumption implies that each linguistic form has a constant and specific meaning. If the forms are phonemically different, we suppose that their meanings also are different... We suppose, in short, that there are no actual *synonyms*. On the other hand, our assumption implies also that if the forms are semantically different... they are not 'the same', even though they may be alike as to phonetic form.

(Bloomfield 1935 p. 145)

Now we are confronted by a number of puzzles. First, the w-assumption is about utterances, not linguistic forms; secondly, it is not clear from the definition of a linguistic form that linguistic forms are required to be determinate with respect to homonymity.

The first puzzle can be solved, or at least made some sense of, if we invoke Assumption 2 of the 1926 postulates: 'Every utterance is made up wholly of forms' (Bloomfield 1926 p. 17), which is not overtly stated in *Language*. This suggests that the sameness of utterances alike in form and meaning is a matter of their containing or consisting of the same (linguistic) forms. The second puzzle is trickier. A linguistic form, according to the definition, seems to be a

meaningful phonetic form. [sləʊθ] is a meaningful phonetic form, however many distinguishable meanings one may want to associate with it. Alternatively, if the emphasis is supposed to be on the indefinite article in the definition of a linguistic form as ‘a phonetic form which has *a* meaning’, then in so far as [sləʊθ] is a phonetic form with more than one meaning, it is not a linguistic form. What Bloomfield seems to want to say is that a homophonous phonetic form ‘is’ as many different linguistic forms as there are different (‘constant and specific’) meanings associated with it. It follows that indeterminacy with respect to how many different meanings are associated with a given phonetic form will make for difficulty in deciding how many linguistic forms we are dealing with. But this would not call in question the w-assumption. For the w-assumption is merely that some speech-utterances are alike as to form and meaning. In fact, why any of this is presented as having a bearing on the fundamental assumption is unclear, on the w-reading. That ‘each linguistic form has a constant and specific meaning’ is not, *pace* Bloomfield, an implication of the w-assumption, but simply (one way of reading) the definition of a linguistic form itself.

On the s-assumption, however, this is all much clearer. On this reading, where the assumption is that an utterance is in itself (what is now called) a linguistic form (i.e. an item already identified in respect of what structural unit it is the form of), that each linguistic form has a constant and specific meaning is simply a reformulation of the assumption itself. On this reading, the only problem of structural-unit analysis is indeed the problem of homonymy. The last three phonetic segments of an English utterance ‘this is a dog’ exemplify the phonetic form [dɒg]. This is a linguistic form in so far as it has a constant and specific meaning. The phonetic form [si:l], found in ‘this is a seal’ is not on the face of it a linguistic form because it has no constant and specific meaning: sometimes it means ‘phoca’ and sometimes ‘sigillum’. But Bloomfield nonetheless wants to count it as a linguistic form. This is the problem with homonyms, and it arises from his insistence that linguistic forms should be observable in the surface phonetic structure of utterances. Bloomfield never himself says that utterances are (concrete) *instantiations* of (abstract) units:⁶ forms are, for him, observably there in utterances themselves. Form and meaning are not

equipollent facets of a structural unit, as they are for Saussure: for Bloomfield it is the form that wears the trousers. The trouble with homonyms is, precisely, that different linguistic forms have no distinct form. Hence the need, unacknowledged by Bloomfield, for a more abstract conception of a linguistic ‘form’.⁷ What Bloomfield actually alleges to be the trouble with homonyms – that speakers may disagree as to when sameness of form is to be associated with a single meaning – is a red herring: his notion of ‘meaning’ is such that for the most part speakers do not know meanings anyway.

On the other hand, leaving the passage on homonyms aside, the w-reading seems implicitly argued for by the mutual imbrication of the problems of identifying form and identifying meaning, as Bloomfield presents them. Form depends on meaning, but meaning would seem to depend on form. We appear to have no independent way in to either.

On both its appearances in *Language* the fundamental assumption is presented as required, at any rate *pro tem*, to circumvent difficulties in the study of semantics, as Bloomfield envisages it. ‘The statement of meanings is . . . the weak point in language-study, and will remain so until human knowledge advances very far beyond its present state’ (p. 140). What are the difficulties? The meaning of a linguistic form is ‘the situation in which the speaker utters it and the response which it calls forth in the hearer’ (1935 p. 139). On the other hand, we are almost immediately told that ‘the ordinary meaning of the English word [= structural unit] *salt* is “sodium chloride (NaCl)”’. It is clear that the definition of meaning just cited must be immediately revised to read something like ‘the features of the situation in which the speaker utters it relevant to its being uttered. . .’. But there are still problems: for one, it seems that we need further and better particulars of what is meant by a ‘situation’ here. On the most obvious interpretation of ‘situation’ salt is not necessarily or perhaps even all that often a feature of situations in which ‘salt’ is uttered. This is apparently Bloomfield’s own interpretation, for he immediately acknowledges the point: salt-free ‘salt’ utterances are what he calls ‘displaced speech’. But instead of trying for a more adequate account of ‘situations’, such that the meaning of *salt* did indeed emerge as what was common to situations, displaced or not, in which the word was uttered (which

would no doubt oblige him to postulate mental events or objects of some kind), Bloomfield simply adds displacement to a catalogue of reasons for giving up on linguistic meaning as intractably hard to deal with.⁸ We have ‘no way’, he says, ‘of defining most meanings and of demonstrating their constancy’, and so ‘we have to take the specific and stable character of language as a presupposition of linguistic study, just as we presuppose it in our everyday dealings with people. We may state this presupposition as *the fundamental assumption of linguistics...*’ (p. 144).

We start with the suggestion that we are inclined to recognise recurrent ‘sames’ in the utterances we produce and hear. The task Bloomfield sets himself is to explicate the sameness of sames in terms of objectively observable features of utterances themselves (i.e. their form) and of the situations in which they are uttered (i.e. their meaning). But what is objectively observable is liable to vary from instance to instance, as regards both form and meaning. Bloomfield, supposes, nonetheless, that there must be something that remains constant in spite of the variation. In the case of form, there must be some ‘constant features of sound-wave’ (p. 76). In the case of meaning, there must be some constant features of ‘situation’.

In order to identify the constant features of sound-wave that make utterances the same in form, we have to attend to meaning. If we know that a given range of phonetically different utterances all have the same meaning, we can abstract from the variation to establish constancy of form, which will consist in what those utterances have phonetically in common. But when we attend to meaning, we find it impossible to identify the constant features in the circumstances of utterance that would make different utterances semantically ‘the same’. Identifying forms depends on identifying meanings, but we are unable to identify meanings. So how does the enterprise get airborne? By actually making the s-assumption, which, by assuming that one side of the form/meaning complex is given, allows us to break the circle.

But we still need an explanation of why the circle should be broken by assuming that it is form that is less problematic than meaning, rather than vice-versa. Meanings are so problematic, in Bloomfield’s terms, that there is little or nothing to say about them. But if the identification of forms depends on meaning, that ought to

be no less problematic. On the contrary, in *Language* and elsewhere Bloomfield has a great deal to say about forms.

In the case of meaning, Bloomfield concedes that science will have to make great strides before it can identify the physical features of 'situations' that make for recurrent semantic samenesses. But why does he not find himself obliged to take a similar line in the case of form? Why is it not, apparently, the case that phonetics will have to advance very far beyond its present state before we can identify recurrent formal samenesses?

Bloomfield's answer seems to be that phoneme theory offers a way of solving problems of form that has no counterpart in the case of meaning. Given a range of utterances assumed to have the same meaning, the phonemicist undertakes to abstract the distinctive phonic features. But given a range of utterances assumed to have the same form, there is no corresponding technique for abstracting the distinctive semantic features. This asymmetry is rigged, though: here we see the significance of Bloomfield's assumption that 'there are no actual synonyms'. The point of this is to guarantee to the phonemicist in advance that, for his purposes, utterances assumed to have the same meaning are already given as utterances of the same structural unit. In other words, the constancy of form that is supposed to establish different utterances as utterances of the same structural unit can only be detected by assuming what the constancy is supposed to show – that they are indeed utterances of the same structural unit. There is no question of trying to demonstrate the constant form underlying [fə:z] and [gɔ:s], however convinced we may be that the meanings are the same.

That seems to clinch the case for the s-reading of Bloomfield's fundamental assumption. But a linguistics based on it is problematic, both in connection with Bloomfield's enterprise as he seems to understand it, and more generally.

First, it drastically weakens the claim that linguistics is a physicalist science, based strictly on attending to what is observable and postulating no mental entities. For in so far as we are already in a position to use our everyday knowledge to identify the structural units of our language, their sameness, in respect of both form and meaning, must in fact, and *pace* Bloomfield, be an abstract, psychological sameness. There is no physical constancy of form that

we as yet know of, and even if there might in principle be a physical constancy of situation that we can call ‘meaning’, that is evidently not what we go by in perceiving utterances as ‘the same’ in meaning, because according to Bloomfield nobody, not even the linguist, can as yet say very much about the alleged constancies in question. Of course, reliance on our everyday knowledge is a temporary expedient, a *pis-aller* imposed by the currently deficient state of science, but it would make no sense to think of pressing ‘everyday knowledge’ into service in that role if we could not safely assume that advances in science will tend to confirm its deliverances. Either linguistics has to wait for science to get its act together, or, if linguistics is already feasible on the basis of attending to our everyday knowledge, Bloomfield’s official epistemological commitments must be mistaken, or at any rate irrelevant.

But what does Bloomfield mean, in this context, by our ‘everyday knowledge’? Can we in fact ascertain the structural units of our language by attending to what we would say in ‘ordinary life’ as to when speech-forms are ‘the same’ or ‘different’?

I, for one, find that I have no fixed view on whether when you say [i:ðə] I understand you to be uttering something ‘different’ from when I say [aiðə]; whether this also applies to [təmeirou] vs [təmə:təu]; whether the case of [ə'lu:mɪnəm] and [æljʊ'mɪnjəm] is different again, because of the different spellings; whether *it is raining* is ‘the same’ as *it's raining*; whether I acknowledge the somewhat higher level of abstraction at which *didn't you finish it?* might be ‘the same’ as *did you not finish it?*; whether the modal auxiliary *will* is the ‘same’ word as the lexical verb *will*; whether *I didn't leave because I was angry* when it means ‘because I was angry I didn't leave’ is a different structural unit from *I didn't leave because I was angry* when it means ‘it wasn't because I was angry that I left’; whether I think that *croissant* as uttered in an English-speaking context is a French word momentarily intruding into English (i.e. not a structural unit of English at all, but of French) or see it as an English loan from French (i.e. a separate structural unit from the French one); whether it is supposed to make any difference to my judgement on the status of *port* (drink) and *port* (harbour) as homonyms that I happen to know that port was originally shipped from Oporto; whether *father-in-law* is three words or one; whether

holpen is a variant of *helped* or a different unit in a different system; indeed, whether *man* and *men* are different words or different forms of the same word. On all such questions I find (i) that they hardly ever arise in everyday life; (ii) that in so far as they do, different answers or interpretations might be appropriate in different situations; (iii) that it is an open question how far, in any particular case, other English-speakers would concur with my judgements;⁹ (iv) above all, that *there is no context-neutral fact of the matter*. If this is what is meant by consulting our everyday knowledge, a linguistics based on that knowledge would be very different from anything Bloomfield envisages.

It must be that he has in mind, not my everyday knowledge of *my* language, but my everyday knowledge of a cultural construct called 'English'. In that case some but by no means all questions of this sort can be decisively answered by reference to the texts in which this cultural construct is enshrined. If this is the knowledge in question, I know, for instance, that [təmeɪrou] and [təma:təu] are variants of the same entity *tomato*: the English dictionary tells me so. On this interpretation (the only one that might be workable) Bloomfield is espousing a linguistics that establishes the structural units of a language like English by simply reproducing the analysis already set forth in grammar books and dictionaries. In the case of any such language the s-fundamental assumption of linguistics is superfluous – or, rather, it is: assume that the work of analysing utterances in terms of structural units has already been done by descriptive grammarians and lexicographers.

The w-assumption may be weaker as an *assumption* but, just for that reason, is more interesting to explore as the starting-point for linguistics. The question now is whether one can find any basis for recognising context-neutral 'sames' in languages at all. On the s-fundamental assumption, where forms are given, the queries that arise have to do with competing abstractions, or alternative levels of abstraction, in terms of which forms are to be understood. Hence the question whether [təmeɪrou] and [təma:təu] is each (the formal side of) its own structural unit, or whether one should identify a supervenient abstraction of which these are non-distinctively different manifestations. On the w-assumption, the question is on what grounds forms such as [təmeɪrou] or [təma:təu] are identified

in the first place. For each of these is already an abstraction over countless physically and circumstantially different utterances.

How far does our 'everyday knowledge' help us to tell when or whether utterances are 'the same', for purposes of validating the *w*-assumption? That they are *sometimes* taken to be the same is not in doubt: many everyday metalinguistic acts depend on it. For instance, the idea of repeating what one says would make no sense otherwise. But what counts as repeating an utterance is not somehow fixed in advance of the diverse situations in which repetition is called for. What constitutes 'saying the same thing' depends on the kind of sameness required.

What kind of sameness are we looking for when it comes to identifying the abstract items of which a language might be held to consist? This specification would seem to rule out the sameness involved in, say, repeating the question 'are you going to cut the grass today?' by saying 'you'll mow the lawn today, won't you?'. It is not hard to at least conceive of an abstraction of which they are variant instantiations. It would be idle to object that they have little in common formally. 'I'm hungry' as uttered by a needy stranger at the door, and by a child who merely wants to put off going to bed, have little in common semantically, at least as Bloomfield thinks of semantics (the example is from Bloomfield 1926 p. 26), but that is no bar to seeing them as the same structural unit(s). The problem, or one of the problems, is that the equivalence between the two utterances is not generalisable beyond the contexts in which it arises. Grasslands are not lawnlans. To cut a cake is not to mow it. What is required is some dimension of equivalence between utterances that holds irrespective of context.

What is required, in fact, is some counterpart in speech of the sense in which if I write *truth*, and then write *truth*, I have written the same thing twice.¹⁰ That sameness is guaranteed not because, as here, it may be manifest *in praesentia* through the visual near-indistinguishability of the two inscriptions as printed, but by the fact that writing involves the deployment of a finite set of graphic units explicitly learned as such by apprentice writers and readers. Whatever variant shapes the letters may have, the Roman alphabet (as ordinarily used by English speaker-writers) has just twenty-six of them. Two instances of any letter or sequence of letters are therefore

bound to be the same in respect of their composition out of units of the system; and that fact can be systematically exploited, if anyone chooses, to imply corresponding samenesses among whatever the letters may be taken to represent. If I give one student an 'A' and another student an 'A', I have given them the same mark. So, if speech has no such counterpart, could it not be imported from writing, by understanding spoken utterances as utterances *of* written forms? If I can get you to understand that the inscription *truth*, whatever other functions it may have, answers to some range of potential or actual English utterances, I have automatically established a dimension in which those utterances are the same. They are the same in corresponding to that determinately identifiable written form.

There is little doubt that acquaintance with the practice of writing, in its role as a communicational analogue of speech, is the basis for any systematic articulation¹¹ of the recurrently instantiated invariants of which a spoken language is held to consist.¹² Which makes it hard to understand the usual claim made by structural linguists, that writing is no more than a convenient ancillary notation for speech. In Bloomfield's case this takes the form of saying that 'writing is not language, but merely a way of recording language by visible marks. . . In order to study writing, we must know something about language, but the reverse is not true' (Bloomfield 1935 p. 21).¹³ We can assess the value of this claim by considering the following statement: 'A morpheme like *John* [dʒɔn] or *run* [rʌn] is really an abstraction, because in any actual utterance the morpheme is accompanied by some secondary phoneme which conveys a grammatical meaning' (Bloomfield 1935 p. 163). If writing is not language, and the written forms '*John* [dʒɔn]' and '*run* [rʌn]' here are not recording any actual utterance, it seems impossible to make anything of them at all.

Let us suppose, in accordance with the practice if not the theory of structural linguists, that writing is not only language, but the only linguistic mode in which the invariant structural units alleged to underlie speech could be made manifest. How far does writing in fact solve the problem of identifying them?

If writing is just an ancillary notation, it may be hard to see why it is not more systematically deployed to eliminate homography. Granted that our everyday knowledge tells us that English *port* is

homonymous, why, in recording this part of the language, do we not use different spellings for the different structural units (*port*, *not starboard*, but *please pass the paught*, *Southampton is an important pourt*)? This might be extended to many constructional homonymities: perhaps *flying planes are dangerous* but *flyinge planes is dangerous*, hence different spellings for *flying planes can be dangerous* in its different senses. The reason why not has to do with the fact that writing is no mere way of recording language (i.e. speech), but a mode of communication *sui generis*.

In the first place, as the *port/paught/pourt* example immediately shows, there is no monolithic 'spoken English' to which written English stands in a consistent and systematic relation: rhotic speakers may not care much for *paught* as a spelling of *port*. Secondly, the question of systematically differentiating homographs only arises given an orthography *stricto sensu*, i.e. an orthodox spelling system – which is by no means a necessary concomitant of the use of alphabetic writing. Thirdly, granted an orthography, changing a spelling can introduce new words. British English writers currently have a problem with the spelling *programme*. Writing *computer programme* may seem intolerably quaint, using American *program* in any other context unacceptably foreign. One solution is *computer program* but e.g. *television programme*. Writers who make this differentiation may come to see *program* and *programme* as homonyms (cf. *flour* and *flower*); after all, there is now a difference of meaning between *computer program* (software) and *computer programme* (plan or scheme in connection with computers). If I dislike [dɔːg] as a pronunciation of *dog*, I can spell it *dawg*, thereby drawing attention to it as something 'different'.

The graphic technics of linguistics have come a long way since Bloomfield's day. (In *Language* he refuses to acknowledge even the need for phonetic as distinct from phonemic transcriptions.) But if writing were just an ancillary notation for speech, it would be hard to know what to make of the pervasive practice of reifying linguistic non-items, and graphic distinctions between linguistic items, that correspond to nothing at all in ordinary speech:

- 1 *The cat is on the mat is acceptable
- 2 *The cat is on the mat* is acceptable

The function of the star in front of (1) is precisely to indicate that this is *not* a sentence of English. And what turns non-sentence (1) into sentence (2) has nothing to do with the spoken language it allegedly records; it is a matter of differentiating the first six words in accordance with a professional convention for using certain graphic devices (italics, inverted commas) whose primary function is simply to draw attention to the items concerned, as can be seen by comparing (3) and (4)

- 3 Curd coagulated with rennet is called cheese
- 4 Curd coagulated with rennet is called ‘cheese’

Do the quote marks in (4) even answer to what the logician calls ‘mention’ (as opposed to ‘use’), let alone to anything in speech? *Cheese* here seems to be somewhere in the borderland between ‘mention’ and simple emphasis.

Consider the sentence *the sun sets in the west*. It would be instructive to find out just what readers of linguistics texts *do* consider when confronted, as they incessantly are, with injunctions of this sort. What makes this, in and of itself, a ‘sentence’, as opposed to, say, a sequence of written English word-tokens or the record of a unique utterance? Granted that it is intended to be taken as a sentence, can it in fact be one? We are officially supposed to consider a spoken item of some kind: *the sun sets in the west* has to be understood as a typographical convenience for [ðəsʌnsetsɪndəwɛst]. But is that not at least two homophonous sentences: *the sun sets in the west* and *the sunset’s in the west*? To suppose that there is such a thing as ‘the (spoken) sentence *the sun sets in the west*’ is simply to reify an entity conjured into existence by the graphic devices employed to cite it.

Writing is to some extent capable of fixing the identification of spoken utterances, in the limited sense of providing them with something other than speech to which they can be referred; and in literate societies it is used to just that. Not being speech itself, writing abstracts from the vagaries of utterances. But written forms themselves are not automatically determinate in respect of what if anything outside themselves they may stand for. They have to be ‘self-identifying’, in the sense in which Searle says that language in

general is self-identifying (Searle 1995 pp. 72–73). Which means, of course, that *we* have to identify them. And the point is not that we may fail to identify them (though we may). The point is that our identifications must ultimately be self-validating. In the case of non-linguistic uses of writing, e.g. a musical notation, any obscurities as to how the notational devices relate to the phenomena they transpose into the written medium can be explored and resolved in language. Language, being language, is on its own. It is interpretatively terminal. The mutual relations between utterance and written form have no grounding outside language itself. The stable and consistent identification of structural units in speech that writing apparently offers is ultimately illusory. But the illusion is powerful.

On the face of it, there are large differences between structural linguistics and linguistic description. Traditional descriptions of languages, on the whole, do not rest on explicitly stated fundamental assumptions. Latin grammars do not start out with an announcement that in ancient Rome some utterances were alike in form and meaning. That something like this must be true is perhaps taken for granted; more likely such propositions are perceived as neither necessary or relevant. The subject matter of the grammar is simply there from the outset. Secondly, structural linguists tend to dwell on the non-meaningful level of articulation. The phoneme is perhaps the most successful structuralist concept, surviving in essence unrevised to this day. In contrast, traditional grammatical descriptions have little or nothing on phonology; latterly there may be an inventory of phonemes, but that is a recent loan from structural linguistics itself. The meaningful side of language has always been more problematic for structuralists: the closer they get to meaning itself the more uncertain they become. There have been periods when semantics has disappeared from the agenda altogether. Doubtless the problem is that to be articulated at all, meanings have to be thought of as (reducible to) verbal items of some kind. Bloomfield was no doubt right, if for the wrong reasons, to steer clear. There is a difficulty attendant on treating form and meaning as equipollent – as, in Saussure's image, the recto and verso of a sheet of paper. This can be seen if we ask why we cannot identify a meaningful linguistic unit by citing its meaning. Why do we not talk of an English word 'any species of *Cygnus*, a genus of large,

graceful, stately, long-necked birds of the duck family', whose form is *swan*? Because the words cited as 'the word' here themselves constitute a form, with a meaning of its own. However abstract one's conception of a linguistic unit, form must have priority: it seems that metalinguistic difficulties leave us with no choice but to cite it as a form, with a meaning attached. Along with the requirement that everything in language must be observable, leading to a reluctance to postulate abstractions, this no doubt has some bearing on Bloomfield's equivocations about 'form'. But language-describers have no problems of this order. For them, the lexicographer's way of stating meanings will do; fretting over what or where meanings really are is best left to linguists and philosophers.

So it may seem odd that the interests of structural linguists and language-describers should ever have been seen as congruent, let alone to the point where the perceived role of structuralists was to rectify the sloppy formulations of descriptive grammarians. The two enterprises seemed to have more in common than they did because structuralists simply took for granted that the words, phrases, clauses, sentences, etc. the grammarians talked about were uncontentiously given. Of course the classificatory *terms* were rethought. Definitions, sometimes new and more precise, were provided for 'word', etc. But how to set about identifying the items that fell within the definitions was not an issue. If the grammarian and the lexicographer agreed that there was an entity *tomato* in English, then there was an entity *tomato*, its existence seemingly guaranteed by that uniquely identifiable written form. Despite claims to provide old-fashioned language description with a theoretical rationale, based on identifying the units and distributional relations actually involved in language-use, structural linguistics has indeed largely been a redescription of what has already been described.

But is linguistic 'description' an apt term? For the question remains: where do units like *tomato* come from? No doubt the linguistic psychology of literate speakers is organised with reference to them, but that seems to be at least in part a matter of acquaintance with a cultural construct. And it seems that those who 'describe' the cultural construct must have a large say in *creating* it.

The citizens move about the city, making use of its amenities. They have widely varying conceptions of it; some may not even

perceive it *as* a city. Some may be unaware of the very existence of certain quarters. The city-describer describes it *as* a city. The city-structure-describer offers to supply a theory to underpin the description: the city, as described, has a structure; and it is the structure of the city that determines the activities of the citizens. But how did the city come to *be*?

Creaky analogies aside, there is something that remains to be explained. What Bloomfield identified as the fundamental (w-) assumption of *linguistics* is really the fundamental assumption of *linguistic 'description'* itself, as undertaken not only by language experts but language-users themselves. Elaborating languages on the basis of that assumption has long since been achieved. One task for linguistics is to explain how.

Notes

- 1 Bloomfield's theory is not founded on any special or technical definition of a language corresponding e.g. to the Saussurean *langue*; in fact he initially defines 'a language' in a way that seems to have nothing to do with the idea of a determinate structure: a language is simply 'the totality of utterances [= 'acts of speech'] that can be made in a speech-community' (Bloomfield 1926 p. 26). It is only when he defines a speech-community a 'a group of people who interact by means of speech' (Bloomfield 1935 p. 42), i.e. by using 'the *same system of speech-signals*' (1935 p. 29; emphasis added), that he overtly states a structuralist conception of a language.
- 2 See Harris 2001 p. 34 for discussion of the authenticity of this wording of Saussure's dictum.
- 3 For example, the Saussurean sign unites a meaning with an acoustic image. Both meanings and acoustic images are abstractions liable to variation in their actual manifestation in speech. So although different utterances of the French word *Messieurs!* may show considerable phonetic differences – 'aussi appréciables que celles qui servent ailleurs à distinguer des mots différents', nonetheless 'on a le sentiment qu'il s'agit chaque fois de la même expression', i.e. the same sign. On the other hand, there is the variation exemplified in the French phrases *le mois de décembre* [ləmwadadəsɔ̃br] 'the month of December' and *un mois après* [œmwazapʁe] 'a month afterwards', where *mois* (allegedly) appears in two phonetic guises, [mwa] and the 'liaison form' [mwaz]. Saussure is quite clear that these must be taken as the acoustic images of two different signs: 'il ne saurait être question d'une unité concrète: le sens est bien le même, mais les tranches de sonorité sont différentes' (p. 147). What seems to emerge here, in brief, is that for Saussure a single sign may vary in form within limits set by the requirement that it cannot have variants with different numbers of segmental phones. This distinction gives results that are unlikely to correspond to the

- 'réalité' for 'la conscience des sujets' that Saussure claims (p. 128) synchronic linguistics to be exclusively concerned with – as he seems to acknowledge by referring to [mwa] and [mwaz] as 'le même mot' (p. 147), and by ignoring the possibility (which makes eminent sense to phonologists but probably not to French speakers as such) that the [z] here is [mwaz] in fact the onset of the first syllable of [apre]. Saussure's hand is forced by his *a priori* conception of the sign. To take English examples, it seems unlikely that if [dɔ:g] counts or can count as the same structural unit as [dɒg], this could not also apply to the pair [sɪŋgə] and [sɪŋə], as versions of *singer*. For Saussure the latter would have to be different signs.
- 4 The wording on p. 144 is slightly different and introduces another ungainliness: '[i]n certain communities (speech-communities) some speech-utterances are alike as to form and meaning'. The substitution of 'certain' for 'some' does not appear to imply that Bloomfield now thinks there are speech-communities where the fundamental assumption does not hold. At any rate, if he does, he has nothing to say about such communities.
 - 5 What exactly this means is problematic. Are these limits to the extent to which the assumption is true in respect of any set of utterances to which it applies, or limits to the range of utterances to which the assumption applies at all?
 - 6 Although he does let slip the word 'represent' in the discussion of homonyms on p. 145: '[pɛə] represents two nouns (*pear* and *pair*) and a verb (*pare*)'.
 - 7 This need also arises in connection with Bloomfield's recognition that morphemes may have various kinds of 'alternant'. For discussion see Love 1990 pp. 58–64.
 - 8 Another reason Bloomfield gives is that 'even when we have some scientific (that is, universally recognised and accurate) classification, we often find that the meanings of a language do not agree with this classification. The whale in German is called a "fish": *Walfisch*...' (p. 139). Some of the more salient things wrong with this are: (i) the whale in German is not called a fish but a *Walfisch*, and indeed sometimes a *Wal*; (ii) if the problem with *Walfisch* is supposed to be that it contains the form *fish*, what that shows, on Bloomfield's own terms, is that *fish* doesn't mean 'fish', for *fish* evidently occurs in situations where no fish or fishes are in question; (iii) come to that, English *fish* doesn't mean (what Bloomfield seems to have in mind when he says the whale in German is called) 'fish' either: consider *cuttlefish*, *silverfish*, *fishplate* etc.; (iv) it is precisely because the meanings of words like *Fisch* and *fish* 'disagree with a classification' that the classifiers never used such words in the first place; the whole issue here follows from the mistake of supposing that *fish(es)* is synonymous with the taxonomic zoological term *Pisces* – a mistake it is especially odd to find Bloomfield making, given that 'we suppose ... there are no actual synonyms' (p. 145) even within languages, let alone translingually (if *Pisces* in its zoological use is taken to be the Latin word *pisces* rather than an item from an international supplementary lexicon to be treated as belonging to the vocabularies of any number of vernaculars); (v) latter-day taxonomists are by no means sure that even *Pisces* should figure in a 'scientific (that is, universally recognized and accurate) classification': goldfish being more

- closely related to goats than they are to sharks, it seems that there is no such thing as a fish anyway. Of course Bloomfield cannot be blamed for missing this last point, but it does highlight the absurdity of supposing that ‘the statement of meanings’ could even in principle depend on ‘a scientifically accurate knowledge of everything in the speakers’ world’ (p. 139).
- 9 See Davis 2001 for a salutary reminder of the diversity of people’s metalinguistic understanding of words.
 - 10 The sense in which they are ‘the same thing’ is *not* that in themselves they are, or represent, the same word. In fact the first *truth* here is the Welsh word so written, meaning ‘falsehood’. Letter-sequences are what they are; *we* have to attach interpretations to them.
 - 11 The point is not that without writing we can have no sense at all of recurrent sames. The linguistic psychology of the non-literate or pre-literate, especially in a world before the invention of writing, may be hard to imagine. But some of us may be able to recall what the incipiently literate made of the purely oral. I grew up in an area of England where the word for calling for a pause or truce in a playground game was [veinɪz]. Or so I first apprehended it. There were many variants, of which the most frequent seemed to be [feinaitɪs]. I recall wondering whether my version was a legitimate alternative or a simple misapprehension. There seemed to be no one to ask. The word was formally quite opaque. It was apparently a noun, as it occurred in the collocation ‘I’ve got [feinaitɪs]’, and I dimly supposed it to be plural, but for all I knew it might (in this form at least) have been a phrase or a compound. The word had no ‘rappports associatifs’, no connections with any other word that might have suggested what to make of it. Above all, it had no written form. Without such anchorage it scarcely had more phonetic stability than a Chinese whisper. Nonetheless, I was fairly sure that there was a ‘same thing’ of which all variants were instances. But what *was* this same thing? Now imagine some enterprising child announcing that the word is in fact *feyknights*, so spelt. Writing pins it down: it immediately comes under metalinguistic control. Now a folk etymologist could tell a story about it.
 - 12 See Love 1998; Joseph, Love and Taylor 2001 ch. 14.
 - 13 On the other hand, ‘in order to make a record of our observations, we need a system of written symbols which provides one sign for each phoneme of the language we are recording’ (p. 85). What feature of non-written language is recorded by distinguishing *eau* from [o] in ‘French *eau* [o] “water”?’ (p. 89).

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LANGUAGE CONSTRUCTING
LANGUAGE: THE IMPLICATIONS
OF REFLEXIVITY FOR
LINGUISTIC THEORY*

Talbot J. Taylor

What if there were no reflexive language? If no language had any reflexive properties? Or to put it another way: What if we did not have and had never developed any metalinguistic vocabulary or metadiscursive techniques for talking about our language and its everyday uses – the language that is sometimes called, our ‘primary’ or ‘object’ language? What would be the consequences?

This paper is intended as a thought-experiment. Within the context of this volume’s topic of ‘rethinking linguistics’, its goal is not one of proving any claim or set of claims, nor even of providing an argument. As a thought-experiment, most of the paper consists in posing counterfactual questions and then speculating about possible answers to those questions. Few if any of the answers proposed could be *shown* to be true, or even given any empirical *evidence*. But that is not the point of posing the questions or proposing speculative answers to them. Instead, my aim is to suggest a different way of thinking about something that is quite familiar and that we assume we already know perfectly well – language. My hope is that the experience of trying to make sense of language in this way can help

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linguistics break free of the rhetorical compulsion to see our familiar account of the properties of language as *necessary*, as the only account that *could* make sense of all that we ‘know’ about language. (For a discussion of this rhetorical method and of the motivation for using it, see Taylor, 1992, chs. 1 & 11). If the result is that the proposed shift of aspect in how we see language strikes many readers as disorienting and strange, so much the better.

Another approach would have been to lay out an argument for the importance of reflexive discourse (or ‘metadiscourse’): its importance to the learning of language, to the evolution of language and its everyday uses, to the sociopolitical issues which it raises, and to its description, analysis, and theorization. I have been presenting this argument, bit by bit, point by point, in various papers and books published over the past 20 years (see especially Taylor 1981, 1986, 1990a, 1990b, 1992, 1993, 1997, and Shanker and Taylor, 2001). But while my goal here is both more speculative and suggestive, at the same time its focus is much more generalized.

So I begin by asking the reader to imagine the consequences if we users of a language (and in most cases I will be taking English for purposes of illustration) did not have at our disposal any everyday reflexive *vocabulary*, such as the ordinary English words

mean	talk	speak
understand	tell	nonsense
word	promise	agree
say	answer	suggest
refer	describe	reply
true	explain	question
false	ask	request
name	language	justify

How would our experience and use of language be affected if, mysteriously, these words and our capacity to invent them somehow disappeared? I will speculate about some possible consequences below. And yet this thought-experiment should not be limited to imagining a world without metalinguistic vocabulary. For such vocabulary is only the small tip of the reflexive-linguistic iceberg. As with all language, what matters much more than the vocabulary

itself, of course, is how we use it. The importance and function of reflexive language will never become clear if we think only in terms of the vocabulary items which are employed in reflexive discourse. Analogously, we could learn little about the role and influence of ethical discourse in a given society simply by studying the vocabulary used in that discourse – no more than we would learn about the game of soccer if we merely examined the ball, the goal posts, the shoes, and clothes used in the game.

Therefore, to take another step closer to where the real ‘action’ is in linguistic reflexivity, the reader might reflect on the consequences if there were no metadiscursive *forms of expression* – by which I mean the somewhat routinized phrases for talking about language and our uses of it. The following commonplace expressions are some typical English examples.

- ‘That’s what she said.’
- ‘What did he mean by that, anyway?’
- ‘I’m talking about the one on the left.’
- ‘Sorry, could you say that again?’
- ‘Did she understand what you said?’
- ‘What does *comely* mean?’
- ‘That’s not true!’
- ‘Yes, that’s right.’
- ‘Why did he say that?’
- ‘Will you explain that?’
- ‘What’s that called?’
- ‘Is this what you’re referring to?’
- ‘I’ll try to describe it to you.’
- ‘I don’t agree with her.’
- ‘What’s his name?’
- ‘Please don’t lie to me.’
- ‘Promise me you won’t go.’
- ‘He said he was sorry.’
- ‘She ordered me to leave.’
- ‘I won’t say a word about it.’
- ‘I insist on doing it this way.’
- ‘I believe you.’
- ‘Really?’

- ‘I’m glad to hear it.’
- ‘Would you ask him to shut up?’

To take yet one more step closer to the real ‘action’, we might ask what the consequences would be if we language-users were not just lacking in reflexive vocabulary and forms of expression, but had no metadiscursive means *at all* for talking reflexively? In other words, if we had no conversational techniques or language games *whatsoever* for talking about, referring to, commenting on, expressing our disagreement with, criticizing, proposing an interpretation of, questioning, explaining, asking for clarification of, (etc.) ... something that we or someone else had said or written or signed? What if we couldn’t ask ‘Who said so?’ or ‘Why do you say that?’ or ‘Don’t you agree?’ or ‘How do you know that?’

Another way of raising these general kinds of questions is to ask what the consequences might be if we did not have:

- The concept of ‘what (someone) said’
 - The concept of ‘what (someone) meant’
 - The concept of ‘what (someone) is/was talking about’
 - The concept of ‘saying (something) again’
 - The concept of ‘understanding (or not-understanding) what (someone) said’
 - The concept of ‘what a particular word/utterance means’
 - The concept of an utterance ‘being true’ (or ‘being false’)
 - The concept of ‘why (someone) says (something)’
 - The concept of ‘explaining (something)’
 - The concept of ‘what (something) is called’
 - The concept of ‘reference’
 - The concept of ‘description’
 - The concept of ‘agreeing’
 - The concept of a ‘name’
 - The concept of ‘lying’
- Etc., etc.

Many if not all of these concepts would appear to be essential not only to the practical use of language and to making sense of its use by others, but also to the day-to-day management of our cultural lives.

However, before beginning to explore in more detail what might be entailed by the loss of the reflexive remarks by which these concepts are said to be expressed, it is important to note that this topic and these questions are hardly ever raised by theorists of language. The typical assumption appears to be that reflexive discourse is a superficial *supplement* to language itself, one which could be removed without seriously affecting language. (A noteworthy exception to this is Davis 2001.) A few theorists might concede that this imagined absence of all reflexive forms of language would make a significant difference to particular cultural *uses* of language, all the while insisting that language itself does not require reflexive features. In other words, the standard view is that, even if we were bizarrely *lacking* in all of these metalinguistic words, forms of expression, and language games, we would still have the *concepts* which they express – or at least the concepts which are most crucial to the existence and functioning of language. Naturally, there would be some disagreement about what those crucial concepts are, but it is a good bet that some version of each of the following would be included in most accounts: the concepts of ‘meaning’, of a ‘word’, of ‘being true’, of ‘understanding’, of ‘talking *about*’ (or ‘referring’ to) something, of ‘what (something) is called’, of ‘saying (something) again’, and so on. And therefore these language theorists would argue that even if, for instance, we had no such expression as ‘The word W means X’ or no discursive means at all of asking what something means, we could still grasp the relationship/fact expressed by that metalinguistic phrase: that is, we would still have the concept of ‘meaning’. After all, these theorists might say, if we did not have the concept of ‘meaning’, how could we possibly understand that a given word means just what it does: e.g., that *comely* means ‘pleasant to look at’? The concept of ‘meaning’ is too crucial to language, its use, and its understanding – even at the very earliest developmental stages – to depend on its expression in reflexive exchanges.

If pressed to support this assumption, many language theorists will point out that, the world over, young children learn their and other people’s names, the meanings of many words, what countless things are called, that some utterances are true and others false, etc., long before they show any mastery of metalinguistic vocabulary.

How could children do this unless they had already come to grasp some version of these crucial metalinguistic concepts – unless the linguistic identity of language were somehow already immanent in the phenomena themselves? For instance, John Searle adopts a version of this position in his book *The Construction of Social Reality* (1995; cf. discussion in Love 1999). Language, he says, is ‘self-identifying’. He argues that institutional facts such as money, property, marriage, etc., require language – indeed ‘are constituted by’ language. However, he claims that language is the exception to this. For, although also institutional, linguistic facts – such as the fact that *comely* is a word of English or that it means ‘pleasant to look at’ – do not require language. Language does not require (meta)language in the same way that other institutional facts do. Instead, language is

precisely designed to be a self-identifying category of institutional facts. The child is brought up in a culture where she learns to treat the sounds that come out of her own and others’ mouths as standing for, or meaning something, or representing something. And this is what I was driving at when I said that language doesn’t require language in order to be language because it already is language.

(Searle, 1995, p. 73)

A different but related version of what I will call the ‘Immanency Thesis’ is advocated, by those linguistic nativists who take metalinguistic concepts such as ‘meaning’, ‘understanding’, ‘word’, etc. to be innate – part of the human genetic endowment (e.g., Pinker, 1994; Wierzbicka, 1991). Thus the generative linguist represents the child as coming into the world equipped with and predisposed to apply such metalinguistic concepts to the vocal and gestural behavior produced around them: e.g., to recognize that the sounds mother produces are ‘language’ and that they are therefore endowed with ‘meaning’ and ‘structure’.

For language theorists such as Searle and Pinker, who adopt the Immanency Thesis, the mysterious disappearance of reflexive vocabulary and forms of expression would not make much of a

difference to the language which the child eventually learns. Linguistic reflexivity – metalanguage and metadiscourse – has, from their point of view, no role in the process of acquisition; nor indeed is it a crucial component of language. Reflexive language is a peripheral, supplementary feature of language, the disappearance of which might affect many of the particular cultural *uses* that we make of language but not *language itself*, the properties of which are self-identifying.

However, it is not my intention here to develop an argument against the Immanency Thesis. Parts of this argument may be found in my publications cited above. Instead, I will put the Immanency Thesis to one side in order to continue with a thought-experiment that does *not* take for granted the self-identifying or innate character of linguistic properties. In support of the thought-experiment performed here, there is the following lesson from history: many forms of scientific pursuit have achieved their greatest advances when their practitioners decided to suspend belief in what had previously been assumed to be the most crucial properties of their object of investigation. In the conclusion, I will briefly return to compare the implications of each version of the Immanency Thesis with those of the claim that the properties of language are reflexively constructed.

* * *

Perhaps what first comes to mind as a necessary consequence of a world without linguistic reflexivity is that no language could have a written form. For how could we have the cultural practice of writing if we could not *refer* to a word or utterance, nor make such (e.g., English) reflexive remarks as ‘You write it like this’, or ‘Write down what she says’, or ‘What does this say (or mean)?’, or ‘Read this’? The existence of any form of writing is fundamentally dependent on the use of reflexive remarks of these general types. The practice of written communication requires that we be able to communicate to each other about meanings, about the written forms themselves, and about the practices of reading and writing. At the same time, the activities involved in *learning* to read and write rely on the use of these and many other reflexive linguistic expressions and techniques. The invention, maintenance, and spread of a writing

system in a given community, as well as its day-to-day management as an effective communicational tool, all require the use of reflexive language. Indeed, this point can be extended to *all* processes of transferring language from one medium to another: speech to writing, speech to gestural signs, writing to braille, writing to Morse Code, etc.¹

It would seem, then, that we make writing what it is for ‘us’ in a given culture – and we keep it that way (or change it) – by talking (and writing) about it in particular ways. One implication of this point is that because the particular ways that we talk about writing are culturally variable, therefore what writing *is* for the members of one culture should not be assumed to be the same for the members of a different culture. Nor, furthermore, should we assume that there are universal, culture-independent principles of ‘what writing is’, or culture-independent criteria which determine whether a particular graphic practice is or is not *writing*. (Whether it is what we in Anglophone culture would typically *call* ‘writing’ is another matter.) Because the modern ‘linguistic’ study of writing is a Western cultural invention, it has been profoundly influenced by the reflexive practices within which alphabetic writing is talked about, maintained, and taught in Western culture. One consequence is that it has often been assumed that all forms of writing *must* be conceived by its users as a way of visually representing spoken language and as composed of parts which individually represent particular words or parts of words (see Harris 1986, 1995). Both of these assumptions are legacies of the lay and pedagogical practices which make writing what it is for those brought up within the Western cultural tradition. However, other cultures talk about and so ‘make sense’ of writing in other ways (see, for example, Gundaker, 1998, Basso, 1974; Dalby, 1970; and the articles in Boyarin, 1993). Furthermore, a culture can change its familiar reflexive ways of talking about writing, for instance in response to technological innovations (see Eisenstein 1983, Olson 1994, Smalley *et al.* 1990). Writing requires – is constructed by – reflexive linguistic practices; but this does not mean that all forms of writing are constructed by the familiar reflexive practices on which Western writing depends.

A related consequence to the impossibility of writing without reflexive language is that, without reflexive language, there could

not be any standardized languages, such as those commonly recognized today. For while it is debateable whether the process of standardization necessarily requires that the language have a written form (see Joseph, 1987 and Milroy and Milroy, 1985), it certainly does require that the language users be able to talk about, characterize, evaluate, recommend, prescribe, ask questions about, and refer to language. How could there be correct, or incorrect ways of speaking if we had no means of characterizing a form of utterance as 'correct' or 'incorrect'? If we could not say 'How is this said?', 'What's the right way of saying this?', or 'It's *isn't*, not *ain't*'? How could there be the crucial concept of a particular word or phrase being or not being (e.g.) English? (As in 'I see what you mean here, but it's just not English!') In other words, the formation and application of evaluative notions, those that are sometimes called matters of 'language quality', and of the normative practices that enforce them (Taylor, 1990; Cameron, 1995), clearly depend on the use of reflexive language. Without linguistic reflexivity, there could be no language policies or language planning, no linguistic prescription, no language mavenry, no language politics, and no national ideologies of language (see Schieffelin *et al*, 1998, Schiffman, 1996, and Crowley, 1996). More generally, linguistic behaviour could not be the normative form of behaviour that speakers around the world treat it as being *if* those speakers were deprived of the necessary reflexive tools for discussing, characterizing, evaluating, or explaining properties of language. And in what sense would language shorn of all its normative characteristics be anything like language as we know it to be?

* * *

Now for a different, albeit related question: How far could human language have evolved without the use of reflexive discourse? Could there have evolved a *homo loquens* who was not at the same time a *homo meta-loquens*?

Naturally, this question sounds somewhat paradoxical – as if the suggestion were that early humans could not have developed 'primary' language (language itself) until they had first developed a higher order language, a metalanguage, for talking about primary language. Of course, one might then want to ask: 'Well, if that's true,

then how could they have developed metalanguage until they had first developed meta-metalanguage for talking about metalanguage?' The absurdity of the regress is obvious.

A first step in reducing the paradoxical appearance of this question is to see how much it depends on what we are willing to call 'language'. In other words, we could begin by pointing out that humans could never have developed a form of vocal or gestural behavior that would be *recognizable to us as language* if they had not developed the means of making reflexive remarks about that behavior: remarks such as the English 'THIS is a hand', 'Do you understand?', 'What does she *mean*?', 'Say that again', 'Really?', and 'That's what I am talking about'. A form of communicational behavior in which it was not possible to say these (or any of the countless other reflexive remarks that the languages of the world make possible) would not seem to us to be the same sort of thing that we today call 'language'.

Yet this is not because a form of communicational behavior that was similar to modern languages – except that it lacked any reflexive properties – would strike us as so very bizarre. And in any case, this is not the point. Much more important is the possibility that such a form of communicational behavior could not itself possess any of the most salient properties possessed by all those that we call 'language'. In other words, the only form of communicational behavior that humans could have developed without such reflexive practices not only would not be recognizable to us as language, it also would not have the properties that characterize all known human languages. With any human language, speakers can do all or most of the following sorts of things: refer to objects and events, mean particular things by their words, say what someone else said (or what they themselves said before), perform particular illocutionary acts such as questioning and answering, understand or misunderstand what someone says, agree or disagree with a speaker, truly or falsely describe 'the way things are'. But any form of communication in which reflexive discourse was not possible could not itself possess the crucial properties that make it possible for a language to be a vehicle for such activities.

For example, how could early humans have begun to have gestures or vocalizations that had *particular* meanings before there

were any reflexive ways of saying or asking what a given gesture or vocalization meant, of speaking of it as meaning (or indeed, as *not* meaning) such-and-such, etc.? That is, how could such a gesture or vocalization have meant *just THIS* or *just THAT*, in the way that, say, we characterize the English word *cautious* as meaning not ‘nervous’ or ‘frightened’, but ‘careful to avoid danger’? If there were no reflexive forms of discourse, it is difficult to conceive how any gesture or vocalization could have been endowed with any particular semantic content. And yet every human language surely has words and utterances which mean *something in particular*, do they not?

At the same time, early humans could not have begun to have gestures or vocalizations that referred to particular objects or events *before* they made use of any reflexive ways of saying or asking what those gestures referred to, or what the speaker was talking about, or whether she was talking about *THIS* or about *THAT* – in the way that in English we say what a word or utterance is *about* or what it refers to. Even more disorienting, it would also seem impossible for hearers to have *understood* anything that speakers said before these communicators possessed the discursive means of constructing the reflexive distinction between understanding and not- (or mis-) understanding. Or to put this counter-intuitive suggestion another way: it is not clear how a hearer’s response to something said could have *counted* as a case of understanding or of not-understanding – at least not in the sense that English speakers today speak of understanding and not-understanding – before there were reflexively applicable criteria by which speakers and hearers could determine (explain, and justify) which category a response should count as an instance of. And if there was no possible means of drawing a distinction between *H understanding what S said* and *H not understanding what S said*, then in what sense could that distinction have existed at all? It is surely a form of the ethnocentric fallacy to assume that the reflexive linguistic distinctions which our culture applies in evaluating and characterizing communicational behavior *must* also be applied – and if not explicitly, then implicitly – by the members of every culture. Given this, then in the imagined circumstances of early humans without *any* reflexive forms of discourse at their disposal, is it not the same fallacy to assume that they must have been applying our reflexive distinctions – such as

that between understanding and not-understanding – albeit only implicitly or unconsciously?

We may well imagine that there have been – and are today – reflexive distinctions which were (or are) *different* from the present English-language distinction between ‘understanding’ and ‘not-understanding’ and yet which still had (or have) a homologous interactional function. However, it would seem that no such distinction could exist (or could have existed) as an interactional or phenomenological reality for a community of speaker-hearers if it were not reflexively constructed and applied in everyday metadiscourse.

Nevertheless, I want to emphasize that this is *not* to say that gestures and vocalizations could not have served various important interactional and communicational functions before there were reflexive means of talking about them as, e.g., ‘meaning M’, ‘referring to R’, ‘being a repetition of U’, or ‘being true’. *Nor* is it to say that such gestures or vocalizations, or their communicational functions, should not be thought of as precursors to the signs and meanings of languages today, or that there must have been some fundamental or unbridgeable evolutionary discontinuity between them and the forms of modern languages. On the contrary, I would argue that such gestures and vocalizations did have important communicational functions and that they were the precursors of the language forms we know today. This is not, however, the position that I have chosen to defend here (although see Taylor 1997, ch.13). My only goal in this discussion is to raise the question whether it makes sense to conceive of a gesture or vocalization having a particular property – such as being a word of some particular language and having some particular meaning – before there were the reflexive means to talk about the gesture or vocalization, its meanings, and its uses.

I also want to emphasize that my point is *not* (or not merely) that we early 21st-century humans would expect the legendary ‘first inventors of language’ to have been able to utter such reflexive remarks and that, if we discovered that they were not able to so, *we* would refuse to apply *our* reflexive term ‘language’ to what they were doing. What I *am* suggesting is that if they were not able to engage in any reflexive linguistic practices, then their communicational behaviour

could not have had the semiotic, cognitive, normative, and interactional characteristics that not only characterize all known instances of language but also appear to be essential to the functioning of any form of behavior *as language*. It might therefore be more to the point to say that we modern humans would not recognize such a form of behavior as language *because it would not have been language*.

There is nothing that prevents anyone from using the word *language* for a form of communicational behavior in which reflexive discourse is impossible and which therefore has none of the characteristics that reflexivity makes possible. Nor, as Humpty-Dumpty pointed out in *Through the Looking-Glass*, does anything prevent someone from using the word *glory* to mean ‘a nice knock-down argument’. But what is at issue in this thought-experiment is *not* what is or is not *correct usage* for the metalinguistic term *language*, but rather the question of what sense it makes to use that term for a form of behavior which its own users do not speak of – and so, do not conceptualize – as possessing any of the kinds of properties in terms of which all known human languages are characterized.

Take, for purposes of illustration, the example of personal names. Every language is said to have personal names (Lehrer, 1994; Brown, 1991). Yet how could a particular vocalization or gesture have become the first name? ‘When was “ug” merely a vocalization that typically drew a fellow hominid’s attention and when did it become his name?’ This comic strip brain-teaser trades on the paradoxical character of trying to imagine how people could have had names before it was possible to ask someone’s name (‘What’s your name?’; ‘What’s he called?’), or say that your name is such-and-such (‘Hi, I’m Elaine’), or give a name to a new baby, or speak of such-and-such as someone’s name, etc. In other words, imagine that there were *no* such reflexive practices in our culture. It then becomes difficult to conceive how there could still *be* names in our culture. It is no less difficult to make sense of the Immanence theorist’s claim that, in spite of this absence of reflexive practices, some of vocalizations we used in this culture were in fact – appearances aside – *still names*. For how could a given sequence of sounds, such as [əle:n], still be the name of a particular person if we

had none of the reflexive practices of naming? For there would not be any way that those sounds could be *treated* as someone's name – that it could function in our interactions *as a name* – if our culture had no such word as *name*, or anything similar, and no reflexive practices in which the word was used. A rose may be a rose by any other name; but a name is not a name unless that's what we say it is. (A similar point is made regarding THE pronunciation of personal names in Wolf *et al*, 1996.)

This question about names may be extended to other properties which we typically attribute to language. For instance, theorists of language typically ascribe the property of being true (or false) to certain types of utterances. Yet could the utterances of early humans have possessed these properties if they had no means of speaking reflexively? What sense can it make to say that some of early man's utterances must nonetheless have been true and other false, even though the speakers and hearers of those utterances had, according to our thought-experiment, no means of talking about the utterances in such terms? In other words, they could not say things like:

- No, that's not true.
- Yes, that's right.
- That wasn't really what happened.
- Let's see if that's correct.
- Do you really think so?
- I agree.
- I don't believe you.
- Are you sure?
- That's a lie!

An utterance that is true or false is typically characterized as standing in a more general kind of relationship to the world, that relationship which in English we characterize as 'standing for' or 'representing' or 'being about'. Without this, it makes little sense for the properties of being true or being false to be attributed to an utterance. Yet, again, how could 'the first inventors of language' have conceived of a representational relationship between what they vocalized and particular states of affairs in the world unless they spoke reflexively of those vocalizations as we in English speak about

our utterances ‘being about’, as ‘meaning’, as ‘describing’, (etc.) particular objects, events, or circumstances?

Instead, our thought-experiment requires us to imagine a world in which none of these reflexive remarks – and so therefore none of the reflexive practices which depend on their use – were available. What could it possibly mean to say that, in spite of this absence, many of the utterances said by early man still were true and many false? How can ‘the first inventors of language’ have *conceived* of their utterances’ truth or falsity, if they had no means of talking about truth and falsity? And if they did not themselves conceive of utterances as being true or being false, what sense can it make to say that, all the same, the utterances nonetheless *were* true and false?

Analogously, speaking from within the reflexively constructed perspective of English, it may seem plausible to us to characterize some of a vervet monkey’s alarm calls as ‘true’ and some ‘false’ – depending on whether the predator for whom the alarm is appropriate is or is not approaching. But on the assumption that the vervets themselves have no reflexive practices for talking about an alarm, of characterizing it as true or false (correct, right, wrong, etc.), or of saying what predator it refers to or is about, then we should ask ourselves what sense it makes to assert that the vervets nonetheless conceive of their alarms as referring to particular predators or as being true or false. And we should also ask what sense it might make to say that, even if the vervets themselves do not conceive of their alarms in these terms, all the same the alarms themselves possess the properties of reference, representation, truth and falsity.

This discussion suggests that the development in human communicational behavior of such paradigmatically ‘linguistic’ properties as reference, truth, and meaning must in some way have occurred *concurrently* with the development of the metalinguistic tools for and practice of reflexive discourse. Being a vehicle of mutual understanding, meaning, referring, being true – vocalizations and gestures would have come to possess such (or similar) properties at the same time as their speakers and hearers came to characterize them in these kinds of ways. If this picture of the evolution of language can be made sense of, the implications for the study of human evolution, animal communication, cognitive development, and language should be clear. And in this connection

it is important to remind ourselves of what Saussure said about the object of linguistic inquiry. ‘The object is not given in advance of the viewpoint: far from it. Rather, one might say that it is the viewpoint adopted which creates the object’ (Saussure, 1916, p. 8). Reflection on the role of reflexive language suggests that the ‘viewpoint’ that creates the object *language* for us – that determines *what* language is for us, in a given culture – is that which, at least in part, we adopt by speaking of language in certain ways. We fashion the viewpoint from which certain vocalizations become ‘names’ for us (or ‘true’, or ‘about X’, or ‘not good English’); and we fashion this viewpoint by means of characterizing those vocalizations *as names* and by integrating those characterizations and those vocalizations into certain kinds of reflexive practices. We make words have the meanings they do by speaking of them as having those meanings and by embedding them within certain kinds of reflexive practices. Human vocalizations, gestures, and visible marks cannot acquire and so do not have the properties of being names or having meanings ‘in advance of the viewpoint’, as Saussure would say. The viewpoint creates the semiotic object and creates it *as* having particular properties: those that we intuitively ‘know’ (assume, take, intuit) our vocalizations, gestures, and writing to have. Once we recognize the implications of Saussure’s point (and I am not saying that Saussure himself recognized those implications), then at the very least we have to conclude that the evolution of language must have depended just as much on the evolution of *that reflexive ‘viewpoint’* – what Wittgenstein (1953) called ‘grammar’ – and of the means of communicating and imposing that ‘viewpoint’, as it did on the evolution of the properties of the vocalizations, gestures, and marks used and of the properties of the users’ neurological structures.

* * *

How do children acquire new words? How do they store them? What kinds of information must children represent about each word, so they can identify and understand it when they hear it from someone else, and do they can retrieve and produce it when they speak?

(Clark, 1995, p. 393)

The acquisition of vocabulary is the component of the child's development of language which has been most thoroughly studied. From the age of a year or so, children are said to begin to acquire new words, at first fairly slowly but then quite rapidly during a period of acceleration (termed the 'vocabulary spurt') which typically begins in the last quarter of their second year (Bloom, 1993). By the age of six, children have been said to possess vocabularies of something like 14,000 words, implying that from the beginning of their vocabulary spurt they had added to their word stock at a rate of as much as ten words a day, a pace which continues into adolescence (Clark, 1995, p. 393).

There are, of course, many different theories which purport to explain how children are so rapidly able to 'map meanings onto forms', as the process of lexical acquisition is usually characterized (e.g., Clark, 1995, p. 393). However, some theorists have questioned the methods used in measuring rates of word-learning. Bloom (1973, p. 66) points out that children often forget words that they earlier seemed to have acquired. Others argue that the youngest children typically speak not in true *words* but in 'holophrases' (Griffiths, 1986, p. 280) or that the criteria for counting a word as learned are sometimes vague (e.g., Nelson 1973). Yet, one question which is never raised is the role of linguistic reflexivity in what the child learns.

While this question is discussed more generally elsewhere in this volume (chapter 6), it is worth considering its application to the issue of lexical acquisition. Most parents would say that one of the first words a child learns is its name. Yet one might argue that for a child truly to have learned that, say, *Tommy* is his name, he must be able to do more than look up every time when someone says 'Tommy'. Many dogs respond in this way, but simply responding appropriately to a given vocal stimulus is hardly sufficient justification for the claim that a dog knows that *Rover* is *its name*. After all, does a dog know *what a name is*? If not, how can he know that *Rover* is *its name*? What if the child regularly responds 'Tommy' when he is asked 'Now what's your name, little boy?' Few parents – or even sceptical language theorists – would want to deny that the child should be counted as knowing that *Tommy* is his name. And yet, again, there is more to knowing that *N* is your name

than simply being able to produce particular sounds in response to a particular stimulus. For many parrots can do as much. But does such a response, albeit correct, mean that the parrot knows that *Tweetie* is its name? For it may be safely assumed that no parrot has acquired the metalinguistic knowledge of *what a name is*. Yet we should ask ourselves what sense it makes to say that a parrot – or a dog, or a child for that matter – knows that *N* is its name and yet does not know *what a name is*?

In other words, it would appear that acquiring the knowledge that all language-users eventually acquire – that *N* is your name – is misconceived if it is theorized merely as a matter of coming to recognize an association between a certain set of sounds, e.g., [ta:mi], and you. For what the adult knows in knowing that *N* is his name is much more than this. In knowing that *N* is his name he knows what it is, in his languaculture, for *N* to be his name. That is, he is able to participate in reflexive exchanges about names and can use, or respond appropriately to, remarks like:

- ‘Hi, I’m Tommy.’
- ‘My name is Tommy.’
- ‘I’m called Tommy.’
- ‘Who are you?’ (‘Tommy’)
- ‘Say your name.’ (‘Tommy’)
- ‘Are you Tommy?’ (‘Yes.’)
- ‘Is your name Tommy?’ (‘Yes.’)
- ‘Who is the one called Tommy?’ (‘It’s me.’)
- ‘Is there a Tommy here?’ (‘Yes, me.’)
- ‘The boy called Tommy will have to leave now.’ (‘OK.’)
- ‘You must be Tommy, right?’ (‘Right.’)

It is clear that no dog or parrot could spontaneously produce or respond competently to such reflexive remarks, but that any experienced speaker of English could do so with ease. At the same time, the infant who turns his head when his name is called will typically take a few months more before he finally develops the abilities to participate competently in all such exchanges. Learning one’s name involves learning, as Wittgenstein put it, the ‘post’ which names occupy in our culture’s reflexive language games

(Wittgenstein, 1953, §257). Truly to learn what his name is a child must also acquire the ability to participate in such reflexive language games.

These reflections on the reflexive character of language-learning apply not only to personal names, but also to common nouns and other kinds of words; and they suggest problems with the very notion of measuring rates of lexical acquisition. Is the acquisition of a word best conceived as something that happens in a short space of time, so that it makes sense to say that yesterday a child had not yet learned the word X but that now today she has? For example, imagine a child who has started to say the word *shoe* in appropriate circumstances: e.g., when observing her mother putting on her shoe, she says ‘shoe’. (And she does not say *shoe* at inappropriate times.) But what if she could not respond appropriately in situations like the following?

- The child is sitting in a room with various objects in view, including her shoe. She is asked ‘Can you show me the shoe?’, or ‘Do you know what *shoe* means?’, or even ‘Is THAT (pointing) a shoe?’ Or whenever the child says ‘shoe’, she is asked ‘Is this what you said?’ by a caretaker holding up a shoe. However, in every case she fails to give an appropriate response and gives no sign that she understands the question.

What sense does it make to say of this child that she knows what *shoe* means? *Some* sense, no doubt. It is not my intention to lay down requirements for using the reflexive expression ‘has learned X’, where X is some feature of language such as the word *shoe*, the speech act of apology, her name, etc. And yet it is clear that it makes little sense to say that the child has become truly competent in some feature of the language she is learning until she is able to do a great deal more. And a significant component of what remains to be learned is reflexive. She still has to acquire the ‘viewpoint’ from which the linguistic object – e.g., the English word *shoe* – is created.

Imagine the child who never learns to participate in *any* reflexive activities, who never masters any of the terms, expressions, exchanges, or techniques of metadiscourse, who never learns to adopt the reflexive ‘viewpoint’ from which a language is shaped,

fashioned, and made into what we intuitively know it to be. What will language be for her? What will she know about language and its properties and features? What will she be able to do with that knowledge? How will it compare to what a normal human child comes to know about her language? The competence (the knowledge) that a child acquires in learning the words, meanings, speech acts, grammar – indeed any of the ‘primary’ features – of her language cannot be separated from the web of reflexive linguistic abilities into which that competence is securely woven. ‘The object is not given in advance of the viewpoint’.

* * *

Finally, consider what the consequences would be for someone who, for some bizarre reason, had learned the vocabulary and grammar of English but could not acquire the ability to participate in any reflexive routines concerning his or your or another person’s understanding. So if you asked him, say, if he understood what you just told him, he would not know what to respond. In other words, this metadiscursively handicapped person could neither confirm that he did understand or that he didn’t understand, because he couldn’t make sense of what he was being asked. Similarly, if someone else accused this person of not understanding something she’d said, this metadiscursive cripple would be incapable of making any sense of this remark; consequently, he would have no means of determining whether the objection was correct or of determining what he reply to her to show that her accusation was incorrect. Or: if you said to this person ‘Let me make sure I understand these directions you’ve just given me...’, again, he would not know how to respond. Nor would he know how to respond if you spoke to him about someone else’s misunderstanding of a particular instruction or sign.

My point is that this person – who is incapable of participating in English metadiscourse concerning understanding – would clearly have a very difficult time navigating through anglophone communicational life. (And, *mutatis mutandis*, the same would also clearly be the case for the member of another languaculture who, in spite of knowing the language’s vocabulary and grammar, was incapable of participating in that language’s metadiscourse about understanding.)

Indeed, he would lead the odd kind of life that one expects to read about in the books of Oliver Sacks. Imagine the difficulties he would get into – say, in school, or in dealings with the police or with the legal system – if he could not participate in any way in discourse about understanding, whether his own or someone else's. No less challenging would be his everyday dealings with friends, neighbors, workmates, and others.

Reflecting in this way on how challenging it would be for anyone lacking in reflexive competence to participate in the languacultural world should, I feel, lead us to the conclusion that the ability to participate in reflexive discourse is a *prerequisite* for engaging with and contributing to the communicational worlds in which we all live. It also should lead us to the answer to the questions asked in this chapter's opening paragraph: Shorn of its everyday reflexive practices, language – at least in all the forms that we enculturated humans have made it and have known it to be over the past millennia – could not exist.

* * *

Those who conduct thought-experiments must eventually come back to reality. And in this case 'reality' consists of the Immanency Thesis, which is the standard assumption in language theory but which, for the most part of this discussion, I have purposefully ignored. But if the counter-intuitive picture that I have drawn is found to be too disorienting, then the reader can always choose to return to the theoretical safety of one or other version of that thesis. For, it has to be admitted, no argument or evidence has been provided here that would *oblige* anyone to accept that language is reflexively constructed, or that early humans could not have had words, or meanings, or reference, or languages without at the same time having had metadiscourse, or that children could not acquire language if they did not also learn how to participate in reflexive linguistic practices. And there are clear, institutionally recognized alternatives to these bizarre ideas about language constructing language.

One such alternative is simply to assume with Searle that language is self-identifying, that the 'viewpoint' from which its properties take shape is somehow emblazoned on the very face of the linguistic phenomena that early humans first developed and that

generations of children so easily acquire. Instances of words, of names, of meanings, of symbols, of reference, of truth, of languages just are recognizable *as such*, whether or not this recognition is ever explicitly articulated or communicated in metadiscourse.

Or, on the other hand, one may opt for the nativist alternative of the Immanency Thesis: Pinker's assumption that the 'viewpoint' from which the properties of language emerge – ontogenetically and phylogenetically – is a matter of human instinct, a legacy of a genetic endowment which is unique to *homo sapiens*. Thanks to their innate endowment with a language organ, children simply 'see' words, names, meanings, reference, and grammar in the vocal or gestural stimuli with which they are bombarded in their infancy. And 'the first inventors of language' – those who, unlike the previous generation, were the first to be endowed with the language organ – they must have had this gift as well.

Of course, if it is Searle's version of 'reality' to which we opt to return after this thought-experiment, we will have to concede that there is as yet no way of explaining how early humans could have transformed non-linguistic vocalizations and gestures into the specific features of self-identifying language: that is, into those words, names, nouns, verbs, meanings, questions, sentences, etc., which are said to inhabit every human language. And we will have to admit that it still remains unclear how children so easily learn how to recognize these linguistic properties in the 'blooming, buzzing confusion' that surrounds them in their infant environments. Or, if it is Pinker's version of 'reality' which we opt for, we will still have to acknowledge that there is as yet no remotely plausible account of how a neurological language organ could possibly have evolved by natural selection – by the Darwinian method of slight, adaptive modifications to some pre-existing feature. (But no matter, since we can fall back on Chomsky's 'monster-mutation hypothesis': the claim that the language organ must have emerged through some 'catastrophic' event. cf. Chomsky, 1988, 1991; Piattelli Palmarini, 1989; but cf. also Taylor, 1997.) And if we opt for Pinker's 'reality', we will have to concede that no language organ has yet been found in the human brain and that no part of the human genetic code has yet been isolated as the source of the child's innate linguistic knowledge (cf. Shanker, forthcoming).

Another option – attractive, one might suppose, to those who are interested in rethinking linguistics – is to keep an open mind about how language became, how it is, and how, with each new generation, it again becomes what it is to human cultures. This option would entail refusing to take on faith any claims about evolutionary or ontogenetic miracles. And it would shun any version of linguistic ‘reality’ whose comfortable familiarity is bought at the price of closing off those avenues of research which seek alternatives to the presupposition of miracles. My hope is that the picture drawn here of the reflexive character of language might offer some speculative suggestions for such research.

Note

1 I am grateful to Nigel Love for bringing this point to my attention.

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RETHINKING LINGUISTIC CREATIVITY

John E. Joseph

Introduction

That human beings display infinite creativity in the use of language has been a powerful axiom within theoretical linguistics for 40 years, ever since Noam Chomsky first set out his views on the matter in his paper ‘The Logical Basis of Linguistic Theory’ at the 9th International Congress of Linguists in 1962. Yet applied linguists keep bumping up against the limits of such creativity. Research into collocation, notably that carried out in the COBUILD project led by John Sinclair, has progressively confirmed the important insight of J.R. Firth (1890–1960) that language is ‘chunkier’ than non-Firthian linguistics would have us believe. If a given English word has just been uttered, we can predict with a surprising degree of accuracy which words, or which of a limited number of options, will follow it. This is a significant tempering of the notion that we are constantly saying things that have never been heard before in human history.

Other applied linguists such as Scollon (1994, 1995) and Pennycook (1996), who have tried to come to grips with the concept of plagiarism from an applied linguistic point of view, have concluded that there is not, after all, a limitless number of ways of expressing the same information within a particular language. This renders the enforcement of anti-plagiarism rules difficult, if not absurd.

What is more, Chomsky’s own theory of language has evolved in such a way that it is no longer clear whether ‘linguistic creativity’

means anything like what it did four decades ago. After Sampson's (1979) thorough criticism of the politics of Chomskyan creativity, the notion has taken a lower profile, and no change in its meaning has ever been acknowledged. But it remains a linchpin of the argumentative basis for the theory of innate universal grammar. Creativity for Chomsky is centred on production and excluded from interpretation – and it is precisely there that I claim a redefinition needs to take place.¹

Chomskyan creativity and its limits

Chomsky's 1962 paper, the one that secured his international reputation in linguistics, exists in four published versions, the differences among which are looked at in some detail in Joseph (1990).² However, the opening pages of all four versions agree on the following:

The central fact to which any significant linguistic theory must address itself is this: a mature speaker can produce a new sentence of his language on the appropriate occasion, and other speakers can understand it immediately, though it is equally new to them. Most of our linguistic experience, both as speakers and hearers, is with new sentences; once we have mastered a language, the class of sentences with which we can operate fluently and without difficulty or hesitation is so vast that for all practical purposes (...) we may regard it as infinite.

(Chomsky 1964c: 7)

The stunning originality of this statement lay less in the assertion that the class of sentences is infinite than in the proclamation that this is the 'central fact to which any significant linguistic theory must address itself'. Since none of the linguistic theories on offer in 1962 did address it, this amounted to a declaration of the insignificance of all of contemporary linguistics except Chomsky's own embryonic theory. Certainly the Bloomfieldian and Sapirian approaches of Chomsky's teachers took for granted that languages are limited systems with extraordinarily large productive capacities.

But they saw the task of the linguist as being to capture the systems analytically in such a way that the whole of their productive capacity could be accounted for, by writing a linguistic analysis of Arabic, another of French, and so on. Chomsky argued that not only can the whole of the productive capacity never be grasped, but that grasping it is not the point. Rather, the point is to figure out what makes it infinite and therefore ungraspable – the language faculty itself, in the form of an innate ‘universal grammar’ that supplies the basic structural principles and parameters for every existing and every possible human language.

Chomsky’s views on creativity played brilliantly, not just to the 1962 ICL but to the whole *Zeitgeist* of the 1960s. It implied that every human being is infinitely creative, starting from childhood, rather than creativity being the exclusive property of ‘creative types’. Nobody liked ‘creative types’ – the left associated them with bourgeois decadence, the right with socialism – but everybody liked the idea that all of us, especially children, are infinitely, and therefore equally, creative.³

And yet, a curious asymmetry lurks beneath the surface of Chomskyan creativity. The passage quoted above appears to treat production by speakers and understanding by hearers on an equal basis: both have infinite creativity in the sense that speakers can produce an infinite number of sentences, and every one of these sentences can be understood by the hearers, provided that they share the same language. But further consideration reveals a subtle and interesting trick at work in Chomsky’s use of the word ‘creativity’, such that it does not mean quite the same thing when applied to speakers as to hearers. While speakers may have the freedom to ‘create’ new sentences at will, in something recognisable as the general meaning of the word ‘create’, hearers do nothing more than passively register what the speakers have created.

This becomes apparent when Chomsky (1964c: 7) points out that mastery of a language also involves ‘the ability to identify deviant sentences’, such as *Colorless green ideas sleep furiously*, and ‘on occasion, to impose an interpretation on them’, ‘if a context can be constructed in which an interpretation can be imposed’. The poet John Hollander famously constructed such a context in ‘Coiled Alizarine (for Noam Chomsky)’ (from Hollander 1971):

Curiously deep, the slumber of crimson thoughts:
 While breathless, in stodgy viridian,
 Colorless green ideas sleep furiously.

Although I have known this poem for many years, I did not really understand it until I read Garfield (2000), which recounts the story of Sir William Perkin's discovery of alizarine, the coal-tar derivative from which most modern dyes originated. I can now find what to me is very real meaning in the poem's last line, where a previously unseen range of colours, including greens, in the brown coal tar, sleep in Perkin's imagining, but with that fury to be realised that drove him to make the discovery. This is a clear case of 'imposing' an interpretation as Chomsky defines it. But no interpretation needs to be imposed on a sentence like *Revolutionary new ideas appear infrequently* (Chomsky 1964c: 7–8, n. 2). The speaker's mental grammar assigns it a structural description which indicates that it is perfectly 'well-formed'. Interpretation then proceeds automatically out of the mental grammar.

We thus have two completely different mechanisms of interpretation, one for well-formed and the other for deviant sentences. The first is automatic and straightforward. The second is much more complex: the grammar assigns a structural description that indicates the manner of its deviation from perfect well-formedness, after which, '[...] an interpretation can often be imposed by virtue of formal relations to sentences of the generated language' (ibid., p. 9). But the interpretation does not follow directly or automatically out of those 'formal relations' – if they did, the word *imposed* would not be applicable to them. The interpretation of the well-formed sentence is *generated* by the grammar, but that of the deviant sentence has to be imposed *by someone*, John Hollander for instance.

Now, of these two processes, which might one characterise as 'creative' in the ordinary sense of that word? Obviously the interpretation of the deviant sentence, the 'imposed' interpretation, is the creative one. And it is precisely on account of its creativity – the active role of a linguistic agent, namely the hearer – that it is marginalised as something in direct opposition to the 'central fact to which any significant linguistic theory must address itself', namely

that ‘linguistic creativity’ as defined by Chomsky, in which the hearer’s ‘creative’ role is to sit back and let his or her mental grammar assign an interpretation.

The fate of ‘rule-governed creativity’

Chomsky next states that ‘rote recall is a factor of minute importance in ordinary use of language’. He therefore opposes what he takes, not altogether justly, to be the position of Ferdinand de Saussure (1857–1913) and his 19th-century American predecessor William Dwight Whitney (1827–1894):

Saussure, like Whitney (...), regards *langue* as basically a store of signs with their grammatical properties, that is, a store of word-like elements, fixed phrases and, perhaps, certain limited phrase types [...]. He was thus quite unable to come to grips with the recursive processes underlying sentence formation [...]. There is no place in his scheme for ‘rule-governed creativity’ of the kind involved in the ordinary everyday use of language.

(Chomsky 1964c: 22–23)

Instead, Chomsky endorses the view of the earlier 19th-century thinker Wilhelm von Humboldt (1767–1835), the essence of which he explains as follows:

The role and significance of each individual element can be determined only by considering it in relation to underlying form, that is, in relation to the fixed generative rules that determine the manner of its formation.

(*ibid.*, p. 17)

Chomsky thus accounts for infinite linguistic creativity by this grammar of ‘fixed generative rules’, fundamentally innate, in the mind of every speaker. The thousands of individual lexical and functional elements are fed into the grammar, yielding countless combinations. Some of the rules of the grammar are ‘recursive’, such as the one that allows me to add limitless tokens of *very* to the

phrase *I am very very very hungry*, and this means that my potential for creating sentences never heard before is literally infinite, apart from the fact that I expect to die eventually. Whereas, if language were something we learned by memorising words and phrases, what we could produce and understand would be limited to what we have already heard. Creativity demands that the ‘computational system’, the essential core of language structure, including its property of recursivity, must be physically part of our brain at birth, in Chomsky’s view. Infinite rule-governed linguistic creativity is thus one of the underpinnings of Chomskyan nativism.

The major developments of Chomsky’s theory since the early 1960s are traced in Harlow (1996), Freidin (1996) and Atkinson (1996). Since the early 1970s it has gone steadily in a ‘lexicalist’ direction, to the point that what was ‘minimal’ in his 1990s Minimalist Program was grammar, syntax, the ‘fixed generative rules’ of the last quotation above. Nearly all the work that innate Universal Grammar used to do is now accomplished by morphological features that are already part of words as they are stored in the lexicon. Of the two leading lights of Minimalism at MIT, David Pesetsky has published a book called *Zero Syntax*, and Alec Marantz has proclaimed ‘the end of syntax’:

The syntactic engine itself – the autonomous principles of composition and manipulation Chomsky now labels ‘the computational system’ – has begun to fade into the background. Syntax reduces to a simple description of how constituents drawn from the lexicon can be combined and how movement is possible [. . .]. A vision of the end of syntax – the end of the sub-field of linguistics that takes the computational system, between the interfaces, as its primary object of study – this vision encompasses the completion rather than the disappearance of syntax.

(Marantz 1995: 380–381)

That last rhetorical flourish of ‘completion, not disappearance’ is an attempt to quell dissent among Luddite adherents to Government-and-Binding, the version of Chomsky’s theory that immediately preceded the Minimalist Program.

Chomsky, meanwhile, has held on to a ‘minimal’ syntactic engine, to the frustration of disciples not yet born when he constructed the theory of rule-governed creativity, which he has never bothered to reformulate, only to reassert while denying that anything has changed. Perhaps he clings to that last bit of innate grammar just because, although he has lost faith in it as an explanation of language, he remembers how much was built on its foundation, even if few others do. For if Marantz is right and ‘Syntax reduces to a simple description of how constituents drawn from the lexicon can be combined and how movement is possible’, then we have come back to something uncomfortably close to what Chomsky in 1962 described as the position of Saussure, who ‘regards *langue* as basically a store of signs with their grammatical properties’. What Chomsky saw as crucial then, the ‘recursive processes underlying sentence formation’, is no longer an essential part of the system forty years on.

Chomsky has dropped the expressions ‘rule-governed creativity’ and ‘infinite creativity’, along with his other 1960s keywords ‘competence and performance’ and ‘deep and surface structure’, because of gross misunderstandings to which all of them gave rise – a point I shall take up further on. Nevertheless, creativity as he defined it continues to be cited regularly as a knock-down argument for linguistic nativism, even by linguists who do not consider themselves followers of Chomsky. There may or may not be a serious problem here, but there is certainly an opportunity. A redefined creativity, taking account of the changes in Chomsky’s theory and of the original imbalance in the treatment of production and interpretation, might pave the way to a resolution with other approaches to language based on a very different conception of linguistic utterances.

Firth and Orwell on collocation

As was mentioned at the outset, those contemporary British (applied) linguistics traditions that stem from the teaching of Firth take seriously the evidence that our processes of speaking, writing and understanding do not proceed word by word, but in larger ‘pre-packaged’ chunks. This observation has important implications for

how we imagine language being ‘stored’ in the brain. The standard imagery has long been of a grammar and a lexicon in our heads.⁴ The idea is that in one part of our brains is an inventory of ‘atomic’ words, understood as sound-meaning correspondences, and in another part are rules for putting the words together (hence the title of Pinker 1999).

This atomic model makes a certain amount of explanatory sense so long as we accept that words (or morphemes) are the basic unit from which utterances are built. But if we take seriously the data suggesting that our linguistic output is constructed of larger units, the picture changes. Somehow or other we have to complexify our account to explain the high predictability of collocations. The mental lexicon-plus-grammar metaphor leads us on the contrary to expect a much freer occurrence of words in the same grammatical and semantic category. Chunks of language would have nothing to do with the language faculty as such, but with general memory.

A phrase like *Many a mickle makes a muckle* is something I have memorised more or less as I have memorised the order of the days of the week. According to the atomic model, what I have memorised is a pattern for putting together elements drawn from my mental lexicon, where *mickle* and *muckle* are listed separately and, so far as the lexicon is concerned, are no more likely to occur with each other than either is to occur with *tackle*. In fact, however, I have read but never heard or used *mickle* outside this set phrase. Its existence as a word is something I project out of my memory of the phrase, rather than the other way around, the phrase being projected out of this and the other constituent elements. Had I grown up in Scotland, things would be different; and my passive attitude toward *muckle*, even more than *mickle*, is a sign that I am not a muckle good Scot.

Mackin (1978: 149) pointed out how our linguistic knowledge of collocation is used as a stylistic device, for example by P.G. Wodehouse:

It being my constant policy to strew a little happiness as I go by, I hastened to point out the silver lining in the c.’s.

I could see at a g. that Jeeves had been right in describing her demeanour as despondent.

To this I would add that it is used as a money-saving device, e.g. in lonely hearts adverts, where a DWM, N/S, ISO a BF with a GSOH, pays for eight words instead of sixteen. Again, what the corpus-based data have shown is that set phrases are the rule rather than the exception in language, and if we follow the implications of this through, they blur any distinction between our general memory and the part of the mind that processes language.

The legacy of Firth has been developed by Sinclair (1966, 1991), Halliday (1966), Halliday & Hasan (1976) and others, in increasing isolation from the American linguistics dominated for the last 40 years by Chomsky. Thus the recent renaissance of applied linguistic interest in collocations has not met with the objection put to such research in the 1960s: that it could not account for creativity. The two lines of enquiry shall have to be put back into confrontation with one another if the resolution suggested at the end of the preceding section is to have any chance of succeeding.

I shall begin by going back to Firth (1951), where, in the course of a discussion of Edward Lear's limericks, he proposed 'to bring forward as a technical term, meaning by "collocation", and to apply the tests of "collocability"' (Firth 1957 [1951]: 194). 'One of the meanings of *ass*', he famously writes there (ibid., p. 195), 'is its habitual collocation with an immediately preceding *you silly* [...]'. Firth insisted that 'meaning' must be broadly construed to embrace not just words, but actions, and the people who speak the words and do the actions.

The commonest sentences in which the words *horse*, *cow*, *pig*, *swine*, *dog* are used with adjectives in nominal phrases, and also with verbs in the simple present, indicate characteristic distributions in collocability which may be regarded as a level of meaning in describing the English of any particular social group or indeed of one person.

(ibid.)

On the phonological level too, he says, 'Surely it is part of the meaning of an American to sound like one' (ibid., p. 192).

He distinguishes between meaning by collocation and 'the conceptual or idea approach to meaning', saying that the one is

not directly concerned with the other, which I take to imply that it is indirectly concerned:

The statement of meaning by collocation and various collocabilities does not involve the definition of word-meaning by means of further sentences in shifted terms. Meaning by collocation is an abstraction at the syntagmatic level and is not directly concerned with the conceptual or idea approach to the meaning of words. One of the meanings of *night* is its collocability with *dark*, and of *dark*, of course, collocability with *night*. [...]

Examples may be taken almost at random from any English work at any period. *Gorboduc*, for instance; *The silent night, weary day, tender love, deadly strife*, [...] *hold life in contempt, Is all the world drowned in blood and sunk in cruelty, learn to live in peace*. [...] Of course a large number of collocations [...] have been common property for long periods and are still current even in everyday colloquial.

(ibid., p. 196)

Some of the most important later work on collocation has been that taking it in the ‘paradigmatic’ direction of lexical set theory, but here I shall stick to the ‘syntagmatic’ definition envisaged by Firth (see also Herbst 1996). His penultimate example is especially striking, as the only full sentence. Firth makes reference to ‘the guessing game of filling in blanks’, and it does seem plausible that most English speakers would fill in the blank after *Is all the world drowned in _____* with either tears, sorrow or blood. In the wake of that, if it is to be sunk in something, cruelty seems a reasonably likely choice. Projecting these reactions back as part of the meaning of *cruelty* in the English of the 17th-century tragedy *Gorboduc*, as Firth wants to do, raises other difficulties, but they fall well beyond the present scope.

Firth was not the only person in mid-20th-century Britain thinking about collocation. There was also George Orwell (the pen name of Eric Arthur Blair, 1903–1950), whose attitude toward them was radically different. In his book *The English People*, Orwell wrote:

[P]robably the deadliest enemy of good English is what is called ‘standard English’. This dreary dialect, the language of leading articles, White Papers, political speeches, and BBC news bulletins, is undoubtedly spreading: it is spreading downwards in the social scale, and outwards into the spoken language. Its characteristic is its reliance on ready-made phrases – *in due course, take the earliest opportunity, warm appreciation* [...] – which may once have been fresh and vivid, but have now become mere thought-saving devices, having the same relation to living English as a crutch has to a leg. Anyone preparing a broadcast or writing to *The Times* adopts this kind of language almost instinctively, and it infects the spoken tongue as well.

(Orwell 1968 [1947]: 26–27)

Orwell’s novel *Nineteen Eighty-Four* was published in 1949, and Firth refers to it already in a 1950 paper, where, quite surprisingly in view of what he will say a year later, he agrees with Orwell about the danger of ‘prefabricated phrases’:

An English writer, George Orwell, has tried to frighten us by suggesting it would not be beyond human ingenuity to write books by machinery. The sort of mechanizing process we see at work in the film, in radio, in publicity and propaganda, and in the lower reaches of journalism is greatly to be deplored. A great deal of writing, he says, consists of prefabricated phrases bolted together like the pieces of a child’s Meccano set. Too much mechanism, too much totalitarianism, prevents a creative personality from making the most of his language, and there is a prevention of originality. In his recent satire, *1984*, he provides a new language for the new society, Ingsoc. He calls it Newspeak, distinguishing this form of language from Oldspeak or Standard English. There is quite a lot of Newspeak about nowadays, and language education should train people to be aware of it.

(Firth 1957 [1950]: 188)

If Firth had been among the small number of readers of *The English People*, he would have realised that for Orwell, Standard English is not to be equated with Oldspeak. Quite the contrary, Newspeak is Standard English taken to its logical endpoint as Orwell saw it, with a bit of prompting from Ogden's Basic English, a project that already pushed the standardisation process into the danger zone (see Ogden 1930; Joseph 1999a; Joseph, Love & Taylor 2001: 35–38). Similarly, where Firth imagines the problem to lie in the 'lower reaches of journalism', it is *The Times*, pre-Murdoch even, that Orwell points to.

Orwell believed that the power of language to promote clear thinking and combat tyranny is inherent to the language of the working classes. The tendencies of language and thought he believes must be resisted are those he associates with the educated middle and upper classes.

The temporary decadence of the English language is due, like so much else, to our anachronistic class system. 'Educated' English has grown anaemic because for long past it has not been reinvigorated from below. The people likeliest to use simple concrete language, and to think of metaphors that really call up a visual image, are those who are in contact with physical reality [...].

(Orwell 1968 [1947]: 27)

Orwell's famous 1946 article 'Politics and the English Language', besides restating most of the views quoted above, says that:

This invasion of one's mind by ready-made phrases (...) can only be prevented if one is constantly on guard against them, and every such phrase anaesthetizes a portion of one's brain.

(Orwell 1946: 263)

This seems to be the article Firth is referring to the 1950 article, though he directly cites only *Nineteen Eighty-Four*. Firth could be selective about what he knew; his 1951 article that established the study of collocation also mentions Orwell, but takes an entirely different tack:

As George Orwell's satire [*Nineteen Eighty-Four*] suggests, Oldspeak is perhaps being replaced by Newspeak. Should the snob value of Newspeak establish itself, the spectrum of meaning analysis might then describe the new language at all levels from pronunciation through word distribution in collocation to the study of the processes of the newer contexts of situation.

(Firth 1957 [1951]: 206)

No more 'child's Meccano set', then. It appears as though Firth had not fully thought through the significance of collocation until after writing the 1950 paper; and once he realised that it could create a substantial new area of work for linguists to do, there is no more worry about 'Too much mechanism, too much totalitarianism' preventing 'a *creative* personality from making the most of his language', or the 'prevention of originality' (Firth 1957 [1950]: 188, emphasis added). Perhaps Firth had a genuine epiphany that collocation characterises the language of everyone, even such 'creative personalities' as Swinburne and Wilberforce from whose writings he takes study examples in the 1951 article, but it is surprising that he would mention Orwell there without noting Orwell's view on collocations which Firth himself appears to have turned away from. An alternative explanation is that Firth's 'collocations' of 1951 have nothing to do with his 'prefabricated phrases' of 1950, but the examples he and Orwell cites make this difficult if not impossible to accept.

The politics of interpretation

My somewhat cynical suggestion that Firth's shift in position toward Orwell may have been linked with his realisation that collocation could give linguists lots of new work to do may have been true at one level. But the very fact that the critical discourse analysis (CDA) tradition comes primarily out of Firth, via Halliday and others, suggests that his concerns of 1950 were only temporarily sublimated. CDA is, clearly I think, an Orwellian-spirited tradition of textual analysis, of which the quote from Firth (1957 [1950]: 188) above (beginning 'An English writer') could well be identified as the

point of departure. By the same token, the quote from Firth (1957 [1951]: 206, beginning ‘As George Orwell’s satire’) is the point of departure for the more descriptivist corpus approach to collocation represented by the work of Sinclair. This latter tradition departs from Orwell in the fact that it recognises the ubiquity of collocation across social divides, and denies any association between collocation and lack of creativity. In Chomsky’s model, on the other hand, that association is implicit, inasmuch as infinite linguistic creativity is the reason why a collocational model of language is rejected. To be sure, Chomsky does not share Orwell’s views on language and class – and yet his politics are straight out of Orwell, literally (see Barsky 1997: 31). Chomsky’s closest point of contact to British applied linguistics comes where CDA meets the great mass of Chomsky’s writings on ‘manufacturing consent’ (see, e.g., Chomsky 1985 & 1992, Herman & Chomsky 1986, which are only the tip of the iceberg).

It puzzled me for a long time how Orwellian manufacture of consent could be reconcilable with ‘infinite linguistic creativity’ for Chomsky. Infinitely supple linguistic minds operating on innate principles should not be so immediately susceptible to verbal control, like rats in a Skinner box. The solution to the puzzle lies, I think, in another curious feature of Chomsky’s history, his serial repudiation of his own collocations. In the early stages of his work, it really is collocations he deals in – he does not invent terminology, but puts existing words together in a way that gives them new, specialised meaning in his particular context of use. A few years later, however, he gives them up, explaining that they have given rise to too many misunderstandings – ‘deep structure’ taken to mean a universal level of sentence structure that is the same across all human languages for a given utterance like *John hit Bill*, which Chomsky insists is never what he meant.⁵ He had to replace ‘deep structure’ with D-structure, then with DS, then to stop talking about it altogether, lest his theories be distorted beyond all recognition.

This is the same man who believes in ‘absolute freedom of speech’ so strongly that he went to great lengths to become the world’s most prominent advocate for Holocaust deniers, even though he himself is not one (see Joseph 1999b). When it comes to his own collocations, it is quite another story – their meaning is not open for

interpretation, the way something ‘hypothetical’ like Auschwitz is. But there is a consistency here: Chomsky has made clear that for him all interpretation is *political*, except when it is generated directly by grammar. This is precisely his position on the difference between *Revolutionary new ideas appear infrequently* and *Colorless green ideas sleep furiously*. For the latter, an interpretation must be ‘imposed’, and imposing is always potentially a political move. If people interpret that sentence differently, the one who argues most powerfully for his or her interpretation will impose it. Whereas, the ‘perfectly well-formed’ sentence is closed to political interpretation by the real interpretation generated physically by the grammar in the speaker’s brain. Thus, the linguistic creativity Chomsky calls infinite is on the production side only. Interpretation is normally finite, and in the abnormal cases, where he might have called it creative, he instead castigates it as ‘imposition’.

All those who were attracted to Chomsky’s views on creativity because they understood them as meaning that everyone’s linguistic utterances are ‘creative’ in any of the ordinary senses of that word – rather than the specialised sense it has in Chomsky’s collocation – might have been disabused of this misinterpretation if they had paid closer attention to the actual example sentences he used. If you took him to mean that everyone is creative intellectually, his own linguistic example informs you that *Revolutionary new ideas appear infrequently*. And if you took him to mean that everyone is linguistically creative in a poetic way, along the lines of Carter’s (1999: 207) assertion that ‘All language is literary language’, you ought to have taken note that *Colorless green ideas sleep furiously* does not in fact have a ‘real’ meaning.

As the title of Mackin 1978 suggests, the study of collocation is in part the interpretation of words in their ‘social’ relations to one another. It is also the point at which ‘creative’ use of language is subject to surreptitious convention – individuals think they are combining words freely, but are not. Chomsky benefited from the ambiguity of *governed* in ‘rule-governed creativity’: readers automatically interpret it to accord with their particular notion of how a government ought to operate. The great majority of Chomsky’s readers being, no doubt, believers in some form of liberal democracy, understood the phrase as suggesting that free will does operate in our

use of language ('creativity'), it simply does so within bounds ('rule-governed'), much like Mills's ideal of individual liberty being constrained only by the simple rule that no other individual's liberty must be infringed. Naturally this would appeal to them more than what the behaviourists seemed to be saying, that there is no creativity in language, only operant conditioning and its effects, the political equivalent of which would be Orwell's Oceania; and of course more than creativity ungoverned by rules, which would be mere verbal anarchy, Dadaism or Gertrude Stein at their worst.

Such readings do not accord with authorial intent, since the kind of government Chomsky believes in is the anarcho-syndicalism that flourished for a few months in Catalonia early in the Spanish Civil War, a sort of rule-governed creativity in which groups of workers make the rules;⁶ and Skinner (1957) does not repudiate human linguistic creativity but ambitiously attempts to explain how it is possible without a theory of free will. More importantly, however, when we look closely into how rule-governed creativity is supposed to operate in language, it seems that the 'creativity' is not merely constrained by the rules, but *generated* by them. The governing which the rules exert over linguistic creativity is very thorough indeed; they start by governing its very existence. Huxley's *Brave New World* might be the closest literary counterpart.

Within the realm of rule-governed creativity, the authority to identify the rules which Chomsky claims in his capacity as a native speaker is absolute. The perceptive Archibald A. Hill (1902–1992) was among the first to pick up on this, in a symposium discussion he moderated in 1958 between Chomsky and the Romance philologist Anna Granville Hatcher (1905–1978):

HATCHER: [...] I think the only way to study sentences is to study normal sentences, produced under no prejudicial theories, in ordinary language use. When a grammarian constructs sentences there are enormous distortions, and when we try to decide what we would or would not say, we are very likely to fool ourselves.

CHOMSKY: The trouble with using a corpus is that some authors do not write the English language. Veblen, for example, speaks of 'performing leisure', and the verb *perform* cannot take such an object.

HATCHER: I admit it sounds unusual. But I bet that if you studied the verb *perform* you would find other expressions not too far from this, pointing the way to this. He has gone farther perhaps along a certain road but I do not believe he has created something new.

CHOMSKY: No. He has broken a law. The verb *perform* cannot be used with mass-word objects: one can perform a *task*, but one cannot perform *labor*.

HATCHER: How do you know, if you don't use a corpus and have not studied the verb *perform*?

CHOMSKY: How do I know? Because I am a native speaker of the English Language.

HILL: I think at this point I would like to strike a blow for liberty. [...]

(Hill ed. 1962: 28–29)

Hill goes on to argue for the liberty of linguists to use corpus, observational or intuitional data as they see fit. I think, though, that his comment can be interpreted as partly directed against Chomsky's blatant and militant prescriptivism – the clue Hill has left us, taking advantage of his editorial role, is the capital L in 'English Language' in Chomsky's last statement. This was not Hill's normal usage (cf. for instance Chomsky's first statement above), and there is of course a well-established tradition in Modern English of capitalising a common noun, à *l'Allemande*, in order to deflate an overblown conception of it of as an Institution.

Earlier on in the same Symposium, Hill and Chomsky had engaged in an interesting exchange, where Hill refers to views of Edward Sapir's (1884–1939) which I believe point the way forward to a more meaningful conception of linguistic creativity.

HILL: I think on this matter of ungrammatical sequences that I should be more nearly in agreement with Sapir than with you. Sapir said, many years ago, that you could probably not present any written sequence of words to a native speaker without his trying to wring some kind of sense out of it. [...]

CHOMSKY: [...] You are quite right in saying that people will read something into a sequence of nonsense syllables. But the point

is that there is a very significant difference in how hard people have to work to read sense into different sequences, and it is precisely this difference which I think the linguist should investigate.

(Hill ed. 1962: 19)

What Chomsky is calling ‘hard work’ in 1958 corresponds to the imposition of an interpretation in 1962. He certainly had a point in calling for the difference to be investigated; but by the 1962 ICL paper the difference is no longer to be investigated – instead, the imposed type of interpretation is ruled out of consideration altogether. One can see why this had to happen by considering the exchange over Veblen’s phrase ‘performing leisure’. Neither Chomsky nor any English speaker has to ‘work hard’ to read sense into it. It is ‘self-interpreting’ *even though* Chomsky’s mental grammar identifies it as deviant, to the point that he denies it is English.

In the next section, I shall propose that, Chomsky having had his turn, it is now time for something like Sapir’s view, as reported by Hill, to become the principal thing that linguists should investigate.

Making ‘linguistic creativity’ meaningful

At the present stage of our understanding of the human mind, no account of the creative aspects of human linguistic production can be definitive. Perhaps the notion that there could ever be a definitive account is only a dream. We will keep producing such accounts all the same, simply because the questions about linguistic creativity get at the most fundamental aspects of what it is to be human. If progress is possible in this quest, then, I would propose that *a satisfactory account of creativity in linguistic production must be preceded by and built upon a satisfactory account of creativity in linguistic interpretation.*

In other words, we need to relocate ‘infinite linguistic creativity’ from production to interpretation. The relocation requires us to accept as axiomatic that *Interpretation is universally and infinitely creative.* It is not obvious that the axiom is acceptable. Western culture, and probably every other culture, operates with a strictly defined economy of creativity within what will be allowed to count

as an ‘interpretation’ of a prior text, as opposed to a new text that has been inspired by the earlier one. In religion and law, as well as philosophy, creativity of interpretation is recognised as existing but is treated as a problem needing to be brought under control. Applied to biblical interpretation, for example, different reactions to interpretative creativity partly define some of the major differences within Western Christianity. Broadly speaking, the Catholic position is that each individual is capable of interpreting scripture in such idiosyncratic and erroneous fashion that great care should be taken about who is and is not authorised to read, and authoritative, canonical interpretations are granted the same sacred status as scripture itself. The Protestant position, on the other hand, is that the individual’s reading will be so guided by the grace of the Holy Spirit that only the meaning intended by the Author, God, will be interpreted. Something like the Protestant position is found in those approaches to interpretation which assume that the text itself clearly limits the interpretative options available to the listener/reader. Peirce’s notion of the ‘interpretant’ is in this spirit. Chomsky’s dichotomy of real and ‘imposed’ interpretation can be thought of as a sort of compromise between the Catholic and Protestant positions: real interpretations proceed from the text just as a Protestant would like, but ‘imposed’ interpretations are the worst sort of wilful (mis)reading the Catholic fears.

Contract law and constitutional law have constraint and creativity of interpretation right at their core, and they figure prominently in each of the other divisions of law. The public saw a good example of this in August 1998, when President Clinton gave testimony concerning his false statements about his relationship with Monica Lewinsky in the Paula Jones sexual harassment case. He famously defended his public statement that he had not had ‘sexual relations’ with that woman on the grounds that he interpreted sexual relations as meaning coitus; and that the truth of his statement that ‘there is no relationship’ depended on what the meaning of ‘is’ is. This is creative interpretation – and most people (except for lawyers and those for whom Clinton could do no wrong) find it repugnant, seeing it as an attempt to evade the truth. Yet it is carried out in an awareness that truth, at least where the law is concerned, is itself a construct, formed by the creative, rule-governed play of interpretation

and argument (see further Joseph 1995). This does not mean that ‘anything goes’ in interpretation: alternative readings will be judged to be more or less convincing, and accepted or rejected accordingly. Yet the production of such interpretative alternatives is a process unlimited in its creative possibilities.

In Joseph (2000, 2001a & b) I have argued that linguistic identity functions essentially through a form of creative interpretation: we interpret the identity of people with whom we come into contact based on very subtle features of behaviour, among which those of language are particularly central. Ever since the breakthrough of Malinowski’s (1923) conception of phatic communication, we have taken what is ‘meaningful’ in linguistic utterances to extend far beyond their propositional content, and to include all those features of utterances beyond the propositional meaning and its expression which hearers use to interpret things about the speaker – geographical and social origin, level of education, gender and sexuality, intelligence, likeability, reliability and trustworthiness, and so on. Indeed, it has been solidly and repeatedly demonstrated that interpretation of the speaker’s trustworthiness from the non-propositional content of utterances bears directly upon the hearer’s assessment of the ‘truth value’ of the proposition itself. As Edward Sapir wrote:

In spite of the fact that language acts as a socializing and uniformizing force, it is at the same time the most potent single known factor for the growth of individuality. The fundamental quality of one’s voice, the phonetic patterns of speech, the speed and relative smoothness of articulation, the length and build of the sentences, the character and range of the vocabulary, the scholastic consistency of the words used, the readiness with which words respond to the requirements of the social environment, in particular the suitability of one’s language to the language habits of the persons addressed – all these are so many complex indicators of the personality. [...] All in all, it is not too much to say that one of the really important functions of language is to be constantly declaring to society the psychological place held by all of its members.

(Sapir 1949 [1933]: 15–18)

A modern ‘constructionist’ approach to identity would balk at Sapir’s treatment of these features as mere ‘symbols’ and ‘indicators’, as though they and their values were fixed and given in advance. But for present purposes the important point is that, in the kind of overinterpretation described here, in which linguistic identity is grounded, hearers make far more determinations about speakers than the actual linguistic evidence can sustain. This is not to suggest that such ‘creative’ overinterpretation is inherently misguided or problematic, except when it engenders prejudice. The process is so ubiquitous and powerful, taking place in virtually every encounter between people, that without them the entire range of processes which we call meaning and communication would be, if not impossible, at least of a vastly different form. Indeed, it is possible to argue that this process of overinterpretation is shared with other species, and that it therefore predates language in human evolutionary development.⁷ Certainly a tremendous amount of survival value inheres in the ability to size up the truth or falsity of what people tell us. Identity and interpretation form, in other words, the fundamental basis of human communication and interaction upon which ‘language’ in the usual sense is grafted.

The creativity of interpretation of linguistic identity is readily apparent in research I have been carrying out in Singapore, in which I have asked people of various ethnic and cultural backgrounds to listen to short snippets of taped conversation and tell me everything they are able to surmise about one of the speakers. Here is a transcription of one 36-second snippet (unfortunately, the accent of T, whom the subjects are asked to describe, cannot be transcribed):

J: Did you do a lot of shopping when you were in Edinburgh? Clothes shopping?

T: Uh, no. I bought a lot of jeans that are not available in Singapore –

J: Uh-huh.

T: – and a few t-shirts, but not ... jackets, or things like that. Because when the waist fitted, the sleeve lengths were too long –

J: Oh.

T: – and the ... lengths ... didn’t fit.

J: 'sit the same when you shop in Australia? Or is it better –

T: Australian clothes are better.

J: Uh-huh.

T: The price is also ... closer to Singapore prices.

J: Yeah, Edinburgh's expensive –

T: Yes ... it is.

J: – UK's expensive.

T: The British pound ... is, I think a little bit more expensive than the Australian dollar.

I prompted the subjects (i.e., those listening to the tape and describing T for me) to include information on age, nationality and other key bits of information but also invited open-ended responses, which roughly half of the subjects did at considerable length, without any visible signs of straining for things to write, and utterly confident in their judgements. Here are six of the responses (with the English verbatim), followed by information the subjects provided about themselves:

- 1 She is an Australian, around 33. Not really sociable. She only answered what has been asked straight away, but she seemed friendly or willing to do what is required. In conclusion I think she is also good in term of hardworking, friendship, reliable, etc. [Cambodian M, 30]
- 2 She is Chinese, around 20, upper intermediate education, a nurse or a student or a primary teacher, thin, short, attractive. [Myanmar F, 35]
- 3 She is Asian, a non-native speaker, age 30–40, a teacher. She talked causally and shared the conversation. She indeed seemed to be friendly person. Her speaking's not so sweet to the ears so I don't think she cute, or attractive. She's just all right. [Thai M, 29]
- 4 She is Indian, late 20s/early 30s, has done O-levels (secondary education), is fat and short, a clerk or assistant secretary. Sounds *anxious* (tone). [Singaporean F, 40]
- 5 She is a Singaporean Indian, 40+, at least tertiary education, a teacher. Sounds like a 'normal' person. Somewhat tense. She is

‘slightly’ overweight, average height for a Singaporean woman (about 5’4”). She is knowledgeable, well-travelled, talks about currency differences like a seasoned traveller, has enough financial resources (travels, focus on clothes). Probably speaks an Indian language as co-L1 (with English) or L2. Thinks in English. [Singaporean Punjabi M, 43]

- 6 She is a Singaporean Indian, about 45, a graduate, a teacher or lecturer. She is nice, pleasant, though not exactly relaxed. She has a big build, fat, tall, average looking, wears spectacles, dark. Down-to-earth, good income earner which explains her travelling and shopping often. Confident of herself. [Singaporean Chinese F, 28]

Nearly all were able to sketch a physical portrait of T, including every possible combination of tall, short, thin and fat, but also her skin tone (6) and her attractiveness (2, 3) – and note what 3 says about the ‘sweetness’ of her speaking, an important concept of language in many cultures to which linguistics rarely accords any attention. One subject (5) actually specifies T’s height and says that she thinks in English. I have arranged them here in order of accuracy – (6) has T nearly to a tee, though in fact she is not a Singaporean but a Malaysian who commutes to work in Singapore. This may seem a slight distinction to outsiders, but is a mistake the subjects would be surprised to know they made. The evident correlations between cultural proximity and ability to interpret linguistic identity are what I am interested in, but they do not impinge on the fact that all these interpretations are manifestations of linguistic creativity through and through.

Whether we think that creativity of interpretation is something to celebrate or to control, it is not something we can simultaneously *deny* and control, à la Chomsky, without giving up intellectual consistency and setting up a roadblock to an adequate account of the overall phenomenon. To anyone worried that the rights and status of individuals cannot be maintained unless we stick to the traditional conception of productive creativity, I would say that I hope such individuality will stand all the more firmly on a foundation that is consistent with the current state of both our theories and our practice, that admits the minimality (at most) of universal grammar,

the collocability of words and the ‘chunkiness’ of language, and the difficulties they pose for intellectual property.

As academics we know that what our students produce, and we ourselves produce, is to a large extent the circulation of collocations. This circulation is subject to social regulation, which is itself a text that we interpret and change. Ideas about collocation and creativity have shaped the current debate over plagiarism and the more general concept of intellectual property. As pointed out by Pennycook (1996), regulations about plagiarism by university students were set in a time when the sector was very small. Its massive expansion since the 1960s took place in the atmosphere of the Chomskyan view of infinite universal linguistic creativity, and given such creativity, the circulation of signs should be unlimited and plagiarism a straightforward question of honest or dishonest intention. But the ‘chunky’ view of language changes that, as does Chomsky’s unacknowledged retreat from the principles on which his version of creativity was based.

There is also a legal dimension: collocations are ownable, in a way that individual decontextualised words of common usage are not. When C.K. Ogden was granted a copyright on Basic English, he was given ownership of the collocation of those 850 words in the particular context of use he defined for them, though not, fortunately, of texts containing them. Registered trademarks are often common words in collocation, such as ‘Happy Meal’, or ‘Oscar’ in the context of film awards. The point at which they become registerable is a vague and problematical one, with the relevant laws steadily in flux.⁸

Conclusion

Part of Chomsky’s genius lay in recognising that we need to believe in our own linguistic creativity. Without it, we are machines. If we hold that some of us have it and others do not, this is as much as to say that some of us are more human than others. Yet within Chomsky’s own programme of linguistic research, the space allotted to linguistic creativity has steadily dwindled in the forty years since he proclaimed it. Meanwhile, other theoretical and applied linguistic research programmes, in principle largely antithetical to Chomsky’s, have over the same period developed in directions which similarly

limit the meaningfulness of ‘unlimited linguistic creativity’. This potentially creates a greater opportunity for reconciling these approaches with one another – but for this to happen on the grounds of a limited view of creativity would certainly not be to the advantage of linguistics, for the reasons laid out at the start of this paragraph.

By redefining linguistic creativity in such a way as to relocate it first within interpretation (from which Chomsky excluded it), and postponing its application to language production until a suitable base in interpretation has been established, we accomplish a number of useful tasks. We give ourselves a theoretical framework for understanding the (over)interpretation that is at the heart of linguistic identity, something which linguistics has been struggling to do at least since Sapir. In so doing we establish a new basis for the evolutionary continuity from human to animal forms of communicative/interpretational behaviour, from which the unique facets of human language can be more fully appreciated. Moreover, we contribute to a basic reorienting of linguistics away from the single-minded concern with speaker intention which has brought about an indefensible lack of concern with what hearers and readers do. And we lay the ground for understanding the process of interpretation as something that is not automatically determined by the text, but a creative act by the hearer/reader. Admittedly, this will be regarded as a problematic notion by most people; but the way to come to grips with a problem is to analyse what makes it happen, not to go into denial that it happens at all.

One last consideration: every day, computers come ever closer to reproducing our linguistic creativity on the production side. Where once the definition of humanity was threatened by the prospect of evolutionary continuity, now that machines are encroaching upon the human, our evolutionary roots are the guarantor of human uniqueness rather than the threat to it. Groping toward an understanding of the infinitely creative nature of linguistic interpretation is the best way to hang onto that important portion of our humanity that depends on language. A computer can generate both *Colorless green ideas sleep furiously* and *Revolutionary new ideas appear infrequently*, assign an interpretation to the latter (though not *the* interpretation), or tell you that the former is ill-formed. What it cannot do is ‘impose’ an

interpretation. That remains the most distinctively human creative linguistic act.

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Notes

- 1 For an introduction to the work of several of the figures discussed here, see Joseph, Love & Taylor (2001), especially chapters 1 (on Sapir), 3 (on Orwell), 5 (on Firth), 8 (on Bloomfield and Skinner) and 9 (on Chomsky).
- 2 The four published versions are Chomsky (1962, 1964a, b, c). I shall here be citing the most fully developed and widely available version, 1964c.
- 3 Chomsky (1966) greatly extends the interest in the French 17th century that first surfaced in his revisions of the 1962 paper. In so doing he is led to consider how the matter of linguistic creativity weighs upon the issue of free will, with its immense political implications. Chomsky's views on creativity had already gained wide notoriety with his 1959 review of Skinner (1957). Skinner acknowledged that his radical behaviourism threatened the status of such things as human freedom, and considered this merely a matter of making verbal adjustments: "Personal freedom" and "responsibility" will make way for other bywords which, as is the nature of bywords, will probably prove satisfying enough' (1957: 460). The irony is that, apart from a few such throwaway references to his frighteningly shallow political philosophy in its closing pages, Skinner (1957) was the last great attempt at a humanistic account of human communication. The bulk of its data and insights were drawn from the literary canon, and its express aim was to bring psychology close to the level of human insight that the greatest literature achieves. Chomsky's review of it, with its numerous references to experiments with rats that in fact are never mentioned by Skinner, followed up by his 1962 paper identifying creativity as the central problem for

linguistics to address, has led to a widespread association of behaviourism with the denial of linguistic creativity. This is certainly not the case with Skinner's book. See also Chomsky (1971), a review of another book of Skinner's (1971) written after Chomsky had developed a political philosophy of his own.

- 4 This is plainly metaphorical, if we consider that grammars and lexica were originally books; but of course the meanings of words are unstable enough that questions of metaphorical vs. literal meaning are ultimately matters of interpretation.
- 5 I have analysed the 'misinterpretations' in Joseph (1999c).
- 6 For a first-hand account of its demise, see Orwell (1938); and for a comparison between the surface claims and deeper reality of Chomsky's political views, see Sampson (1979).
- 7 I say no more than 'possible to argue' in order to forestall the need to address here the serious constraints on evolutionary argumentation put forward by Talbot Taylor in Savage-Rumbaugh, Shanker & Taylor (1998).
- 8 Butters (2001) contrasts the way the problem of lexical distinctiveness is approached by linguists, lexicographers, and trademark lawyers.

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6

RETHINKING LANGUAGE ACQUISITION: WHAT THE CHILD LEARNS

Talbot J. Taylor and Stuart Shanker

One way of thinking about the study of language acquisition is to see it as concerned with questions that are divisible into two major kinds. On the one hand, there are questions concerning what the child acquires. And then, there are questions about how the child acquires it. We will refer to these two types of question about language development as the *WHAT* question and the *HOW* question. Given this way of dividing the territory, any rethinking of the topic of language acquisition should begin with the *WHAT* question, because it lays the groundwork for the *HOW* question and, so, for the ways that we might approach the latter. At the same time, we should resist the commonsense view that the *WHAT* question is really very simple and that it is the *HOW* question that poses all the difficulties.

Q: 'What does the child learn?'

A: 'Why, the language of her community, of course.'

Q: 'How does she learn this language?'

A: 'Well, different theorists have different ideas about that...'

However, on reflection, we can see that there is in fact a wide variety of ways that the *WHAT* question might be answered. What does the child learn? Well, she learns English, or Swahili, or Pitjantjatjara, or Mohawk, etc. Or, another answer might be: She learns the

phonology, morphology, syntax, and lexicon of the English language. (For the purposes of this general, meta-methodological discussion, we will make all our references to the acquisition of English, although with the presupposition that the major points in the discussion could be addressed to the acquisition of any language.) Or, she acquires the ability to speak and understand English. Or, she learns how to make statements and commands, to ask questions, to make requests, to express her desires, etc., in English. Or, she learns at what English words and grammatical constructions mean and how to use them with those meanings. Or, she learns the recursive processes of English sentence formation which enable her to produce and understand new sentences. Or, she learns to distinguish grammatical from ungrammatical combinations of English words. Or, she learns at which positions to set the parameters of Universal Grammar to conform with the computational system of English. And so on.

It is worth emphasizing an important methodological implication: each such answer to the *WHAT* question determines the kind of *HOW* question that acquisition researchers will see themselves as concerned with. In other words, it will determine the kinds of explanations of language development that will be looked for and the criteria by which those explanations will be evaluated. If we answer the *WHAT* question (1w) ‘She learns the grammar and lexicon of English’, the *HOW* question (1h) that will then concern us is ‘how does she learn the grammar and lexicon of English?’. If we answer the *WHAT* question (2w) ‘She learns how to make statements and commands, ask questions, make requests, express her thoughts, etc.’, then the *HOW* question (2h) which will occupy us will be ‘how does she learn to do these things?’. Furthermore, each such *HOW* question raises different corollaries. The *HOW* question (1h) leads us to ask how the child can get information about the grammar and lexicon of English, whether that information is sufficient, how the child processes the information, what the obstacles are to this task, etc. Whereas the *HOW* question (2h) suggests different corollaries, concerning the means available and obstacles presented to the child who is learning to perform these speech acts. Moreover, it is clear that the kind of reply that might be seen as a satisfactory answer – or partial answer – to (1h) is of a very different sort than might be seen as a satisfactory answer to (2h), and this applies to any *HOW* question

whose import might be motivated by a given conception of WHAT the child acquires. In sum, the way in which acquisition researchers answer the WHAT question determines what they will see as their investigational task, as well as the kind of research methods, hypotheses, evidence, and assumptions that they will see as relevant to that task.

* * *

The importance of this general point is easily illustrated by the approach to language development taken within generative linguistics. From the generativist perspective, WHAT a child eventually acquires (the adult competence referred to as ‘the steady state, S_S ’) is a complex, formal system of core and peripheral knowledge – knowledge that has an anatomical realization in brain structure. It is understandable, therefore, that the basic principle of the generativist view of language acquisition is that the study of HOW a child acquires language belongs, not to psychology, but rather, to biology, cognitive neuroscience, and Artificial Intelligence – with linguistics seen as a subfield of biology. For language acquisition is seen as ‘a matter of growth and maturation of relatively fixed capacities, ... largely determined by internal factors’ (Chomsky 1966: 65). That is, a child need only be exposed to the ‘right’ kind of environment in order to allow for the information stored in the ‘language gene(s)’ to be activated. Hence, from the generativist perspective, the HOW question relates to the development of those internal, neurological structures in which that information is encoded and the mechanisms whereby it is processed. ‘In certain fundamental respects we do not really learn language; rather, grammar grows in the mind’ (Chomsky, 1980, p. 64).

In this light, we should note the connection between this conception of HOW children acquire language and Chomsky’s formulation of the ‘poverty of the stimulus’ argument. This argument claims that, given the formally complex and internalized version of WHAT the child acquires (S_S), the child’s experience does not – cannot – provide the kind of information, nor enough of it, to make it possible for the child to acquire it. And yet, *ex hypothesi*, she *does* acquire it. Indeed, generativism is committed to the principle that, strictly speaking, a child cannot *learn* language. For according to the

poverty of the stimulus argument, a young child's knowledge that, e.g., anaphors are bound within a clause, could not be learned inductively. Similarly, if this internalized knowledge of the 'abstract principles of language' (as defined by generativism) is not neurologically present at birth – as is said to be the case in the child suffering from Specific Language Impairment (SLI) – then that knowledge *cannot* be acquired by training or practice (Gopnik et al. 1997).¹ The poverty of the stimulus argument therefore stands as the keystone in the generativist conception of how the child acquires internalized grammatical knowledge: since experiential learning is inadequate in helping the normal child move from her initial state, S_0 , to the steady state, S_S , the child must therefore rely on 'internal factors' – certain kinds of innate knowledge – which determine the growth and maturation from S_0 to S_S .

In earlier stages of generativist thinking, the neurologically-realized grammar which the child eventually develops was thought to consist in a lexicon and in different kinds of rules determining the possible combinations of the lexical items. Given this early conception of WHAT the child acquires, the generativist interested in language acquisition attempted to determine how children acquire it: that is, HOW they acquire an internalized grammar thus defined in terms of knowledge of rules and a lexicon. Since the poverty of the stimulus argument concludes that grammatical knowledge cannot be learned merely by inductive methods, therefore the attempt to answer the HOW question was refocused as an inquiry into the properties that an innate language faculty requires so that it can derive the rules of the language from the limited information provided by experience. However, over the next few decades, generativist thinking went through changes concerning the structural properties of the competent speaker/hearer's linguistic knowledge – i.e., concerning the content of S_S . Grammar came to be seen not so much as a matter of recursive rules whose job is to generate all-and-only the grammatical combinations of words in a language but rather of over-arching constraints whose task is to prevent any ungrammatical combinations. Along with these changes in the specification of WHAT the child acquires, so also did generativist claims concerning HOW the child acquired them: specifically, claims concerning the character of the innate, grammar-forming principles

that are assumed to be part of the child's initial state S_0 . In other words, as generativist thinking about WHAT the child acquires changed, so, necessarily, did generativist hypotheses concerning HOW this occurs.

While different conceptions of WHAT the child acquires lead ineluctably to contrasting accounts of HOW it is acquired, the reverse is not always true. For instance, generativist, social interactionist, and cognitivist explanations of language acquisition are quite different. However, the salient differences in their accounts of HOW the child acquires language are not matched by fundamental differences concerning their conceptions of WHAT is acquired.

Social interactionist models of language acquisition reject the 'poverty of the stimulus' argument and maintain that, on the contrary, the child's experience *does* provide more information to the acquisition process than that argument assumes (see Gallaway & Richards 1994). Thus social interactionists such as Jerome Bruner, Charles Ferguson, and Catherine Snow set out to show that speech directed to a child (CDS or 'motherese') is much different from adult speech and much different from the way that Chomsky had described it in the 'poverty of the stimulus' argument. In particular, they showed that CDS is syntactically and semantically simpler, grammatically more 'correct', and more fluent than Chomsky claimed. They also showed that there is a significant correlation between a caregiver's utterances and the child's preceding behaviour (e.g. vocalizations, gestures, gaze), as well as between the child's subsequent behaviour and the preceding caregiver utterance. These and other features of CDS were claimed to provide children with an environmental resource of information that 'scaffolds' their acquisition of linguistic knowledge, including the acquisition of many features of linguistic knowledge which generativist theorists had claimed to be impossible to acquire without an innate language acquisition device.

For instance, one of the most powerful predictors of a child's later linguistic ability is the proportion of maternal utterances that are semantically related to the preceding child utterances. In the past decade, researchers have started to discover more specific correlations: e.g. between a child's auxiliary verb use and the frequency of yes-no questions in which an auxiliary is preposed; the growth of a

child's auxiliary verb use and the frequency with which caregivers expand child's utterances; the amount of maternal talk and the child's vocabulary growth; and the child's mastery of particular structures (e.g. passives, relative clauses) and the frequency with which these are used by the caregiver (see Gallaway & Richards 1994; Owens 1996).

Also standing in opposition to the generativist approach to language development is that of cognitivist linguistics. Cognitive linguistics is a broad church, but the following two examples should be sufficient to illustrate the point we are trying to make. An early cognitivist theory of acquisition was articulated by Roger Brown (Brown 1973), who discovered that, for English-speakers, grammatical morphology occurs in a fairly regular order. But why, e.g., does a child acquire the progressive '*ing*' before she acquires the possessive '*s*'? Rather than making the easy assumption that order of acquisition is simply determined by an innate language faculty, Brown looked at two possible explanations: the frequency hypothesis and the cognitive complexity hypothesis. According to the former, acquisition is determined by the frequency of the morpheme in the caregiver's speech. But close study of parents' speech patterns did not bear this out. So attention shifted to the cognitive complexity hypothesis. Brown argued that the order of morphological development could be accounted for in terms of the cognitive complexity of the concept or structure involved. For instance, the concept of *in* is said to be easier for the child to grasp than that of *behind*. Accordingly, Brown hypothesized that it is because of its greater cognitive complexity that *behind* is typically acquired after *in*.

Not surprisingly, critics quickly drew attention to the fact that Brown's cognitivist argument has a distinctly circular feeling. How is one to determine a criterion for 'cognitive complexity' that is independent of acquisition order? Or does a construction's cognitive complexity simply boil down to the fact that it appears later in development? How else can one measure cognitive complexity? Worse: the cognitive complexity criterion does not mesh well with cross-linguistic findings. For certain constructions that appear relatively late for English-speakers – e.g. passives – appear quite early for other language-speakers (Crago et al. 1997). At the same time, the cognitivist premise is challenged by generativist arguments

about linguistic savants (cognitively impaired children with age-matched language abilities) and about children with selective impairments (cognitively age-matched children with significant language deficits). These populations are said to demonstrate that acquisition of language proceeds independently of cognition and that it is therefore an autonomous, maturational phenomenon (Pinker 1994).

In any case, whether one accepts the cognitivist or generativist account of how morphemes are learned, the relevant point here is that both accounts share a common view of what is learned: viz. meaning-bearing formal units (known variously as ‘morphemes’, ‘formants’, ‘signs’, or ‘lexical entries’ in the literature). These are viewed as discrete, self-contained, code-defined units, each possessing a distinct form and meaning and having an existence independent of any particular act of speech. With regard to the acquisition of morphemes, the cognitivist Brown and the generativists do not disagree over what is acquired, but only how it is acquired.

A more recent example of a cognitivist theory is that proposed by Michael Tomasello (cf. Tomasello 1999). Tomasello takes the task of acquiring grammatical constructions to be guided by general cognitive predispositions, in particular, the child’s ability to recognize – and so imitate – the relations and event schemas to which her caregivers attempt to draw her attention by their use of particular grammatical constructions.

Fundamentally, the way the child learns a concrete linguistic construction . . . is the same way she learns words: she must understand which aspects of the joint attentional scene the adult intends for her to attend to when using this linguistic construction, and then culturally (imitatively) learn that construction for that communicative function.

(Tomasello 1999: 143)

Imagine, for instance, that one of the aspects of the perceptual scene to which the adult intends the child to attend is the cognitive relationship of agent-patient, holding between two of the items in that scene: e.g., the dog is biting the postman. Furthermore, the adult signifies this communicative intention by using an SVO construction:

i.e., he says ‘Hey, Fido is biting the postman’. In this case, the child’s ability to recognize the adult’s communicative intention will facilitate her acquisition of the SVO construction. When she herself next has the communicative intention of drawing her hearer’s attention to an agent-patient relation, she will imitatively use an SVO construction to do so. Accordingly, the way a child acquires such grammatical knowledge is not a matter of computational processes which mechanically deduce the rules for generating well-formed formulae. Rather, grammatical knowledge is acquired by means of role-reversal imitation, by means of which the child learns to use particular grammatical constructions to express her communicative intentions (see further discussion of this in Joseph, Love, and Taylor 2001, ch.12). What Tomasello calls ‘functionally based distributional analysis’ is therefore founded on the general, cognitivist assumption that ‘the child must learn that the various linguistic symbols in a complex utterance partition the referential scene into isolable perceptual/conceptual elements, and that these two sets of elements – the symbolic and the referential – must be aligned appropriately’ (Tomasello 1999: 145).

Human children are not innately equipped with a universal grammar applicable to all of the languages of the world equally. They are adapted to enter into joint attentional interactions with adults and to understand adult intentions and attention – and eventually to adopt adult roles in these interactions, including their use of particular linguistic conventions.

(Tomasello 2001: 36)

However, as with Brown’s theory of acquisition order, one cannot ignore the whiff of circularity in Tomasello’s cognitivist account of the acquisition of grammatical constructions. For how, independently of the properties of the utterance expressing it, is the acquisition researcher to identify the properties of a communicative intention? How is the researcher to determine what the cognitive relations, schemas, elements, and structures are to which competent speakers intend their hearers to attend and with which the symbols in their utterance are aligned – *except* by taking the relations,

schemas, elements, and structures of their utterances as transparent reflections of that cognitive content?

All the same, from the point of view of this chapter's argument what is most important here is that, regardless of the differences between cognitivists, social interactionists, and generativists on how grammatical constructions are acquired – whether by means of cognitive, 'mindreading' predispositions, by 'motherese' and interactional 'scaffolding', or by means of an innate grammar-deducing faculty – these three theoretical schools share a common view of the nature of (the grammatical aspect of) WHAT is acquired. What the child acquires is knowledge of the grammatical constructions of her language: e.g., SVO structures, auxiliary + verb structures, passive structures, morphological structures (such as noun inflection), head + modifier structures, etc. Although we would not want to ignore the important differences between the cognitivist and the generativist models of grammar – in particular, the cognitivist does not take grammatical constructions to be well-formed formulae but symbolic devices for expressing communicative intentions – nevertheless, they share, along with the social interactionist, the general assumption that WHAT a child acquires in acquiring a language is *a grammar*, conceived as *internally-realized knowledge* of a complex system of units (morphemes, words, signs, lexical entries, etc.) and the combinatorial relations between them.

The claim being made here, on the other hand, is that for the advances to be made in the study and explanation of language acquisition, there must now be some 'rethinking' about WHAT it is that the child acquires in acquiring language. Different approaches to the HOW question will fail to make appreciable advances unless and until this is done. Because the WHAT question is methodologically prior to the HOW question, new breakthroughs in explaining how children develop language wait upon a different way of conceptualizing what it is that the child acquires in becoming linguistically and communicatively competent.

* * *

There is another, related set of ways in which the WHAT question influences attempts to explain HOW language is acquired. Often, assumptions about what the child *eventually* acquires are read

'retroactively' into claims about what the child is acquiring at an *earlier* stage in her development, whether this is at 3 months old or 6 months or 18 months or 3 years. For example, given a child who at an early age begins doing a recognizable form of behaviour – say, at 9 months she extends her arm and/or finger in the direction of a toy – this is often taken as showing that she now is already performing a gestural version of a linguistic act which every human, all things being equal, eventually learns to do: namely (in this example), she is referring to the toy. As seductive as this way of describing early child behaviour is, the study and explanation of how children learn is vitiated if it is taken for granted that WHAT they will eventually learn – to refer, to mean such-and-such by a given word, to follow particular rules, to request, to say what they are thinking, etc. – is already present in germinal forms in their early communicative behaviour, only needing conventional refinement and normalization as the child matures.

Let us consider in more depth an illustrative example of the effects of taking a retroactive perspective. All normal children growing up in an English-speaking culture learn how to make what are called 'requests': that is, they learn how to 'ask for' things that they want and for actions that they want others to perform for them. Moreover, they typically learn a variety of ways of asking for things and actions. In other words, the speech act of requesting (or asking-for) is one feature of WHAT the child is thought eventually to acquire in becoming a fully competent speaker/hearer of English.

Now, as many acquisition researchers have noted, most children in the second half of their first year develop a gestural complex which involves extending a hand (or both hands) in the direction of some (presumably desired) object while directing their gaze to the eyes of the person who might be able to obtain the object for them – often accompanying this gesture with 'fretting' noises or even what is called a 'phonetically consistent form'. In many studies of the acquisition of requests (e.g., Bruner, Roy, and Ratner 1982, Masur 1983, Zinober and Martlew 1985, Griffiths 1985, Ervin-Tripp and Gordon 1986, Wootton 1997), this gestural complex (open hand extended, fretting, mutual gaze) is treated as an early instance of a request, that is, of that speech act which is a universal feature of every adult English speaker's competence. The general view

propounded in these studies is that the behavioural components of the child's early request will, over time, be reshaped, verbalized, and socially normalized until it more nearly approximates the conventional form of an adult English speaker's request. Accordingly, in such studies, the 9 month-old is described as 'requesting' and as 'asking for' things – in other words, she is described as if she were producing instances of the same communicational act which 5 year-olds and adults produce, but in an early, 'primitive', only-partly-conventionalized form. Such studies therefore 'retroactively' interpret the child's behavior at 9 months as *already* a request, although not yet one which has all the properties that an adult speaker's request would have. A request, nonetheless, is *WHAT* the 9 month-old has produced.

Countless similar claims can be found in the language acquisition literature concerning the child's early instances of acts of reference, of meaning, of following particular rules, of offering, of grasping the meanings of particular words, etc. For instance, a child's early utterance of [dɔgi] will be described as 'referring' to a particular dog in the contextual environs. Her utterance of [bæd dɔgi] will be characterized as an early instance of the modifier + head construction (or of following the rule that modifiers come before heads). Or when she utters [dædi] upon hearing the front door close, she will be described as meaning that her father has come home. Doubtless, these acts are all within the competence of the older child. In other words, all things being equal the child in question *will* learn how to refer to things, to request, to combine modifiers and with the words they modify, and to say what she means. This in turn fuels our inclination to characterize the younger child's behaviour using the same metalinguistic expressions. However, there are potential dangers in this 'retroactive' way of studying child language development and, even more important, a possibility of serious conceptual and methodological confusions.

In the first place, we should note one unfortunate and misleading effect that describing the 9 month-old's behaviour as 'a request' can have on child language research. It can steer investigative programmes away from looking at the child's behaviour in its own, context-specific terms – that is, away from looking at the ways in which, at a given age, the child's behaviour can be observed to

function in the interactions in which it is produced. In other words, if the acquisition researcher studying the videotapes of a nine-month-old takes the child to have produced an instance of one of the speech act types in the adult's repertoire – albeit an immature token of this speech act – then he has a readymade answer to the WHAT question. 'What was that behaviour (REACH + FRET + MUTUAL GAZE) she just produced? Ah, a request, of course'. Given this seductive power of this methodological shortcut, the motivation to inquire any further into the WHAT question is sharply reduced. Would it not seem pointless and redundant for the researcher to persist in asking how a child's behaviour functions in a given interaction, WHAT it is *for her* in that interaction, if the answer – a request – is apparently already available? This is just what commits us to a particular way of looking at the matter. The WHAT question seems already to have its answer; so the researcher is compelled to move on to the next question: how does the child acquire this ability? How, so early in her life, does a prelinguistic, nine-month-old child learn to request?

*The decisive movement in the conjuring trick has been made,
and it was the very one that we thought quite innocent.*
(Wittgenstein 1953 §308)

In contrast to this *retroactive* perspective, Alan Fogel's research is an example of a research programme that approaches communicational development from a more *progressive* perspective. Fogel takes communication to be a 'co-regulative' process, a term which is intended to highlight the nonlinear nature of continuous mutual adjustment. The actions of communicating partners are fundamentally relational, as partners mutually adjust their behaviors to each other in subtle ways. Thus, communication, according to Fogel, cannot be reduced to a single modality, or to the summation of multiple modalities.

In his *Developing through Relationships*, Fogel provides a micro-analysis of the first time Andrew, a one-year old infant, voluntarily releases an object into his mother's hand.

First, his arm extends...and then he releases the object. In past weeks, Andrew has extended his arm many times

toward his mother without releasing the object. (But, on this occasion) once Andrew's arm is extended his hand remains relatively stationary and gradually opens as mother's hand moves underneath his hand. The fork gently leaves Andrew's hand as it is pulled only by the slightest contact with the mother's moving palm. The object release, therefore, is not entirely due to Andrew's initiative. Since the child does not actually drop the object into the mother's hand and the mother does not actually take hold of the object, the object transfer seems to be jointly constructed by both, a genuinely co-regulated activity.

(Fogel 1993: 21)

It would be misleading to describe the child's action in Fogel's example as an early instance of Andrew 'giving' or 'offering' an object to another, just as it is misleading to study a child's early REACH + FRET + MUTUAL GAZE as 'an early version of a request'. But Fogel's frame-by-frame analyses look at how the child's behaviour functions in his interactions at one year. They characterize WHAT the child is doing at that time in terms of the interactions in which he produces it, rather than as an early instance of a type of act that he will later be able to produce adult versions of. Fogel's description in the passage above reveals how Andrew's actions are subtly integrated, moment by moment, with those of his mother, which are, in turn, designed in response to his actions. What emerges from Fogel's patient analysis is that the passing of the fork from son to mother is as much the mother's action as it is the son's – a mutual accomplishment. The result is therefore better understood as a jointly managed, dynamically unfolding interaction, rather than as a sequence of discrete acts produced by two independent agents. From Fogel's perspective, WHAT each person does at any given moment appears as a non-discrete, co-produced component of a jointly managed endeavour. No behavioural segment is independent of the interaction as a jointly managed whole. It is the child's methods of integrating his behaviour into that jointly managed whole that develop over time, as do the integrational methods used by the mother. It is by tracing the increased sophistication of the methods by which child and caregiver integrate their behaviour that Fogel's

progressive approach addresses the HOW questions of child development. What emerges is a picture of the child progressing by means of context-dependent, practical, purposive steps, each taken one at a time, rather than by advancing automatically (miraculously) down a species-defined path to a predetermined goal.

It cannot be denied, however, that such a progressive approach to development leaves a troubling question unanswered. How can interactional development, so understood, ever lead to the point at which we know all normal children eventually to arrive – that is, to the point where they produce true requests, offers, acts of reference and meaning? At what point – if ever – does a child's means of integrating his behaviour into such jointly produced interactions somehow transform into the production of, e.g., discrete instances of true requests? All English-speaking children eventually produce requests; yet, if the REACH + FRET + MUTUAL GAZE gesture is not an early, germinal version of a request, then what must be added or changed so that the child will start producing true requests? To address this question, we need to look at a second reason why it is so misleading to take a retroactive perspective on language acquisition.

* * *

WHAT is a request? Does it consist merely in the production of a particular behavioural complex? If not, then what more is there to requesting or asking for something than merely producing the sounds [pliz me ai hæv ə krækr] – which, of course, many parrots and computers can do? In addressing this question, we should reflect on the important fact that the adult who produces a request – who asks for something – not only can produce the behavioural components of a request, she can also contribute to and respond sensibly to another's reflexive discourse about her behaviour. That is, she is able to participate in reflexive interactions ('metadiscourse') about her communicational acts. Such a reflexive interaction is predicated on the mutual acknowledgement of WHAT she is doing – namely, in the present example, that which in English-speaking cultures we call 'requesting' or 'asking for' something – and of its implications. For example, the adult speaker of English is able to make sense of or make an adequate reply to such questions as 'Is *this* what you're asking for?', 'Are you *asking* me for that or just

showing it to me?', 'Why do you want this?', 'What do you mean?', and a host of other metadiscursive remarks that treat what she has just done *as an intended instance of a request*. She knows how to explain the act that she has just produced and how to confirm or object to the understanding of her act that is manifested in her addressee's response: e.g., A: 'Don't tell me what to do!'; B: 'I wasn't telling you; I was merely asking.' Her ability to produce a request is inseparable from her ability to participate in the metadiscursive 'support-mechanism' provided by such reflexive practices and without which the act of requesting could have no cultural existence. (How could there be the act of requesting in a culture in which no such metadiscursive practices existed? That is, in a form of life in which one could never speak of a behavioural product as a request, or of its interactional implications, or of its understanding or misunderstanding, etc.? See Taylor 2000.)

So there is this important difference between a 9 month-old's REACH + FRET + MUTUAL GAZE gesture and an adult's request. If you ask 'Is this what you are asking for?', an adult – but not a child – can reply 'No, it's the other one' or 'Yes, that's the one'. Or if you ask 'Are you asking me to get that for you?', the adult – but not the child – can reply 'Yes, would you please' or 'No, I'm showing you the object I just mentioned'. And so on. A competent speaker of English is able not only to do what the young child can do – namely, produce the behavioural components of a request – the competent speaker of English is also able to participate effectively in reflexive discourse which is predicated on *the recognition of her behaviour as an intended instance of a request*.

In reflexive discourse cultural members articulate their conception of what they are doing, have done, will do, tried to do, might do, etc. By 'articulate their conception', we simply mean here that, from time to time, they speak of their own or others' behaviour as instances of culturally-recognized communicational acts. And they reply sensibly to another's characterization of their own or someone else's behaviour in such terms, e.g., as a request, as 'asking for' something, as being about something, as meaning something, as offering, as being the same as what someone else just said, as making sense or not making sense, and so on. If a speaker produced behaviour that had the superficial components of, e.g., a request and yet she could

not participate competently in reflexive discourse – could not, for instance, articulate her conception of what she was trying to do as ‘asking for’ something – there would be salient grounds for doubting whether, as we say colloquially, she ‘knew what she was doing’ in behaving as she was. To put it another way: if this person’s behaviour, because she cannot yet participate in reflexive discourse, shows no indication that she herself conceives of what she is doing *as a request*, then on what grounds can we legitimately insist that, nevertheless, she has produced an instance of a request? What sense is there in attributing the production of a particular cultural-communicational act to someone who has no conception of the act in question?²

The general point being made here is that to request (to ask-for) is a reflexive, culturally constructed, communicational act-category. The act of requesting (asking-for) may therefore be thought of as a hybrid of behavioural activity and cultural construction. The members of English-speaking cultures continually construct and maintain this act-category *metadiscursively*, that is, by speaking, writing, and signing about particular forms of behaviour *as* instances of ‘requesting’ or ‘asking for’ something and by responding in certain culturally familiar ways to such characterizations. A request is not a request *sui generis*: that is, a certain gestural, or vocal, or written complex is not in-and-of-itself a request, simply by virtue of its behavioural properties. For such a behavioural complex to be an instance of a request, the act-category of ‘request’ must already be culturally recognized: that is, acts of requesting and the implications of producing requests must be recognizable topics of the culture’s reflexive practices and metadiscourse.

The reflexive character of requesting leads, in turn, to the conclusion that the ability to produce an instance of a request requires at least some competence in that culture’s reflexive practices. *The competent speaker’s ability to produce an instance of the speech act of request is therefore inseparable from her ability to participate in such reflexive practices.* Furthermore, this general point holds for any languacultural act: referring, meaning something by a word or sentence, using a particular grammatical construction, understanding a sign as meaning such-and-such, saying the same thing as someone else said, and so on. Once we recognize the

cultural construction of these foundational ‘things we do with words’, then it should become clear that learning to do any one of them involves more than just mastering the production of a certain behavioural complex. It also necessarily involves initiation into the reflexive cultural practices in which they have their roots.

It should by now be clear that for a child to learn how to request – to refer, to mean such-and-such by a given word, to use a particular grammatical construction, etc. – it is not enough that she learn to produce conventionalized patterns of behaviour. She must also learn how to participate in reflexive discourse predicated on the recognition of these as particular kinds of communicational acts. The development of her ability to produce instances of these communicational acts depends upon the development of her ability to participate in reflexive practices concerning those acts.

Language has an inherently reflexive character, the implication of which is that language is more than just a natural form of human behaviour – it is a cultural practice. Language acquisition involves *both* increasingly sophisticated and normalized forms of behaviour *and* initiation into the reflexive practices by which the speaker’s culture determines *WHAT* particular behavioural complexes *count as instances of*, including what they mean. Therefore, to learn *HOW* to request (or to refer to something, or to mean such-and-such, or to use a particular grammatical construction), a child must do more than refine her behavioural skills; she must undergo this cultural initiation.

* * *

The clear implication of this argument is that we need to rethink *WHAT* it is that children acquire in acquiring language and, the particular point argued in this paper, we need to resist the temptation to take a ‘retroactive’ perspective on the child’s communicational development. The retroactive perspective is misleading for at least the two reasons discussed here. It invites the researcher to ignore the interactional character of a child’s behaviour at the time and in the circumstances of its production and also to treat as instances of mature linguistic acts early forms of action that may share the behavioural – but not the cultural-reflexive – characteristics of those acts.

This suggests, of course, that acquisition research should pay a great deal more attention than it has to the child's development of metadiscursive skills – that is, to the child's development of the ability to participate (either productively or comprehendingly) in reflexive interactions concerning the kinds of things that people do in communicational interaction. Moreover, the study of the child's acquisition of this ability should look not for the automatic unfolding of some kind of species-determined programme of reflexive awareness. Rather, it should focus on particular instances of the child's integration of metadiscursive techniques into her forms of participation in and contribution to jointly-managed interactions. Some illustrative examples of this approach may be seen in the studies of the child's developing use of sequential knowledge and 'understandings' that are presented in Tony Wootton's *Interaction and the Development of Mind* (1997).

Reflexive abilities are sometimes referred to as 'second order', because they concern *talking about* the products of talking itself. And yet, the point being made here is that the development of these 'second order' abilities is an *inseparable* part of the child's development of her 'first order' abilities, those which have always been the main focus of research on language acquisition: that is, with the child's abilities to request, to describe, to mean, to refer, to use particular grammatical constructions, etc. On reflection, one can see why this is so. For these 'first order' abilities concern the production of particular kinds of linguistic acts the characteristics of which are reflexively constructed and maintained by means of the culture's everyday discourse about what one does and can do in verbal interaction. It follows, then, that developing the ability to produce these acts involves not only learning the behavioural mechanics of producing them but also *WHAT* they are. Learning how to request, to refer, or to mean such-and-such is as much a matter of learning how to behave in certain ways as it is a matter of becoming a competent participant in metadiscourse concerning what someone 'asked for', what they were 'talking about', or what they 'meant'. These 'first order' and 'second order' aspects of language development are inseparable because it is only by means of their integration that there is a 'WHAT the child learns'.

Notes

- 1 For example, a normal child is said to construct an implicit rule for extracting regular inflectional endings from the language that she hears, whereas a child with SLI is unable to construct such implicit rules for morphological processes: all inflectional forms are learned on a case-by-case basis. Thus, whereas the normal child is said to acquire these abstract rules without any formal instruction and to apply them unconsciously, automatically, and effortlessly, the SLI child must memorize regular as well as irregular inflectional endings (i.e., store inflected forms as unanalyzed wholes for regular as well as irregular forms).
- 2 This is the same kind of question that is addressed to claims about the understanding of a chimpanzee who can sign 'apple' when you show him an apple, but cannot respond competently to a signed request for an apple or to the questions 'Where is the apple?' or 'Is this an apple I'm holding?' See Savage-Rumbaugh, Shanker, and Taylor, 1998.

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NAGARJUNA, HERACLEITUS AND THE PROBLEM OF LANGUAGE

Roy Harris

Introduction

With historical hindsight, it might be argued that what integrationists are proposing is not so much a rethink of modern linguistics as a return to a much older tradition in the philosophy of language. A problem which arises quite early on in both European and Indian reflections on language is how to reconcile the idea (i) that words and meanings persist through time, with the idea (ii) that every utterance is spatio-temporally unique. In the Indian tradition, Nagarjuna's *Refutation of Logic* (*Vaidalyaparakarana*) is one example of a Buddhist text in which the author undertakes to demonstrate that there is no continuity which validates the concept of 'repetition' of what was said. An apparently parallel contention in the Western tradition is Heracleitus's claim that one cannot step into the same river twice. Although Heracleitus left no text specifically addressed to the question of language, it seems clear that his claim threatens to undermine the whole basis of grammar as developed by the Greeks and their European successors. In fact, what emerged as mainstream European grammar can be seen as oriented quite specifically towards avoiding or at least circumscribing this problem. The formal engagement of language with time is carefully restricted by the grammarians to the tense systems of verbs and one or two other aspects of deixis. Elsewhere it is excluded absolutely, as if the rest of

language were untouched by it. That paranoid exclusion is Heracleitus's unwitting but enduring contribution to Western linguistics.

It is the same antinomy that has now re-emerged in modern linguistics, where it underlies the theoretical conflict between integrationists on the one hand and structuralists/generativists on the other. Integrationism may be seen as representing the Nagarjuna-Heracleitan legacy, whereas Saussurean structuralism and Chomskyan generativism between them represent the opposing orthodoxy.

The problem of time

In modern science fiction the possibility of travelling through time, whether back into the past or forward into the future, is a popular and – nowadays – even a banal conception. It was exploited more than a century ago in H.G. Wells's famous novel *The Time Machine*, published in 1895. This was the book that established Wells's reputation, long before 'science fiction' was recognized as a literary genre. (In fact, it was not called 'science fiction' until 1929.) In this novel, the hero has constructed a machine which enables anyone inside it to escape from the present. This machine, or time-craft, can fly off at the press of a lever into the future or into the past. Wells was writing well before Einstein's theories challenged the Newtonian conception that the time interval between events is independent of the motion of the observer. Nevertheless, Wells already refers to the fourth dimension in a new way. In his novel, the Time Traveller proclaims: 'There is no difference between Time and any of the three dimensions of Space except that our consciousness moves along it.'

Now the reason why time travel seemed such an intriguing prospect in the late nineteenth century was that the spectacular progress of science and technology had recently made possible many things that an earlier generation would have thought impossible. The traditional view is summed up in Wells's novel by one of the sceptics who says bluntly: 'You *can* move about in all directions of Space, but you cannot move about in Time.' That is the proposition which the Time Traveller – as a good empirical scientist

– is committed to refuting; not by logic, but by practical demonstration. The Time Machine of Wells's book constitutes this demonstration.

There are many aspects of this fictional demonstration that I cannot comment on here. Perhaps the most ectopic is the notion that in order to escape from the present you need a form of transport. In other words, travel in time is here conceptualized on the analogy of the more familiar travel in space. Motor cars and aeroplanes are clearly the avatars of the Wellsian Time Machine. But it is difficult, to say the least, to reconcile imaginatively the notion of time travel with that of a physical machine, built in the here-and-now, which is nevertheless impervious to the passage of time. Once you stop to think about Wells's scenario, the first question that occurs to you is: why did not the Time Machine, as a material object, become subject to metal fatigue, or battery failure, or any of the other physical failings that commonly go with the passage of time? Did it have enough power to get the Time Traveller as far as the eight-hundred-and-third millennium AD?

In turn this is part of a more comprehensive conceptualization of time on the analogy of space. When we think of time as proceeding in a single straight line with no loops in it we are applying the image of a spatial configuration to temporal sequentiality. The conceptual tension between this linear time and some alternative view of time is, of course, the basic mechanism which enables Wells to construct such a fascinating imaginative adventure. Later on I shall suggest that this same tension is also part of the appeal of Saussurean linguistics. It lies at the root of Saussure's distinction between 'synchronic' and 'diachronic' linguistics. It is no coincidence that Wells, Einstein and Saussure belonged to overlapping generations. They all have a hang-up about the problem of time relative to the observer. And, if my diagnosis is right, it is the same hang-up, even though its manifestation takes quite different forms.

I shall claim that there is a way of looking at language, which depends on *not* reducing time to some kind of metaphorical extension of space. That difference is what ultimately separates integrationism from the orthodox linguistics of the twentieth century. I shall also argue that this makes integrational linguistics not just a minor offshoot from the mainstream tradition but a radical

departure in philosophy of language. In order to establish this case, I shall need to clarify just how Heraclitus and Nagarjuna (taken as typical representatives) pose a threat to the notion that what is said can always (in principle) be repeated.

Wells and Heraclitus

In Western literature the river is commonly used as a metaphor for transience, as perhaps Heraclitus intended: we speak of ‘the river of time’, the ‘flow of events’, and so on. According to the words of a well-known hymn: ‘Time like an ever-rolling stream / Bears all its sons away.’ But the point of the Heraclitan dictum goes beyond that. It suggests that what we think of as ‘the same river’ is actually an illusion: it is never the same river from one moment to the next because the water is always changing. Without that ceaseless passage it would not by definition be a river at all. Heraclitus did not say: ‘You cannot jump twice into the same lake’, although that too he doubtless would have maintained. But the difference between a river and a lake is that it is obvious even to the lay observer that a river is never still.

It is important to note that the Heraclitan concept of time does not deny continuity. There is nothing to stop you stepping twice into a river: but the second time it will not be the same river. In other words, what you are mistaking for the same river is actually a new body of water flowing where the old water flowed. What confused you was the simple fact that the river had not changed its course. So if my interpretation of Heraclitus is right, ‘You cannot step twice into the same river’ might be glossed as ‘Continuity is not sameness’.

Now how does Wells’s Time Traveller offend this Heraclitan principle? The first point I want to make is that, when we consider the matter more closely, it turns out that he does not. On the contrary, that is exactly what makes Wells’s story grip the imagination. Wells’s narrative does not reject temporal continuity; far from it. The caveat is that we must not suppose that the Time Traveller who returns from one of his journeys is the same Time Traveller who set out. In other words, the crucial question is: ‘Does it take the Time Traveller time to travel?’ Wells gives the reader plenty of clues to show that the correct answer is ‘Yes’. The Time Traveller is late

getting back for dinner; he arrives in a dishevelled state; the Time Machine itself returns travel-worn, with a cracked ivory bar and a bent brass rail. It is no longer the machine that set out from the workshop in pristine condition. So yes: there *were* questions about metal fatigue and battery failure. But all this is perfectly compatible with Heraclitus. When the Time Traveller flies off into the future, what he does, in terms of the Heraclitean analogy (and with Heraclitean technology), is simply get into a boat and row up-river. That is perfectly possible, but it takes time. By rowing up-river, the oarsman encounters a new body of water before that water reaches the place downstream where he had set out. That is Heraclitean time-travel, and there is no mystery about it. Which conclusion, of course, leaves us a little puzzled as to what we thought was so impossible about Wells's story in the first place.

Heracleitus and Plato

Before pursuing that question further, I wish to draw attention to a quite different way of envisaging time that we also encounter early on in the Western tradition. It is to be found in Plato's *Timaeus*. There the creation of the world is presented as an attempt to model an eternal reality in perishable, transient materials – an almost self-defeating project. The role of earthly time in this model is, precisely, to simulate eternity in terms of sequence. It is the nearest approach to infinite permanence that can be accommodated in a finite, transitory structure. And here we find the notion of time directly linked to language, and specifically to the semantics of the verb *to be*. According to *Timaeus*,

before the heavens came to be, there were no days or nights, no months or years. But now, at the same time as he framed the heavens, he [i.e. the Maker] devised their coming to be. These are all parts of time, and *was* and *will be* are forms of time that have come to be. Such notions we unthinkingly but incorrectly apply to everlasting being. For we say that it *was* and *is* and *will be*, but according to the true account only *is* is appropriately said of it.

(*Timaeus* 37e–38b)

Here we have God's-truth prescriptive semantics at its most fundamental level. We are deceiving ourselves with language if we think that the future tense can be used of eternity. Timaeus continues:

Was and *will be* are properly said about the becoming that passes in time, for these two are motions. But that which is always changeless and motionless cannot become either older or younger in the course of time – it neither ever came to be so, nor will it ever be so in the future. [. . .] And what is more, we also say things like these: that what has come to be *is* what has come to be, that what is coming to be *is* what is coming to be, and also that what will come to be *is* what will come to be, and that what is not *is* what is not. None of these expressions of ours is accurate.

In these remarks I think we see Plato's critique of Heraclitus. What is being pointed out here, obliquely and in passing, is that Heraclitus made a linguistic mistake about the nature of time. The mistake lies in the initial premiss that there *is* a river. That is wrong, according to Timaeus's semantics. A river flows unceasingly: we cannot say that it *is*. And once that initial premiss is rejected, the whole conundrum about stepping into the river collapses. If there *is* no river, it is meaningless to puzzle about whether it *is* the same river on two occasions. The question does not arise. As Cornford puts it very neatly in his book on *Plato's Theory of Knowledge* (although commenting not on the *Timaeus* but on the *Theaetetus*): 'Plato's intention is to accept from Heraclitus the doctrine that all sensible objects are perpetually changing – a fundamental principle of his own philosophy. But to Plato sensible objects are not "all things". He will later point out that the unrestricted assertion, "All things are always changing", makes knowledge impossible.' (Cornford 1935: 36)

H.G. Wells would doubtless have agreed. The whole point about the Time Traveller's journeys is that he brings back if not knowledge then certainly fragments of information about the future. Plato never claims that this is impossible in principle.

Time and reality

It is a truism to say that concepts of time are inextricably bound up with more general concepts of reality, both in Eastern and in Western traditions. Historians of Buddhist thought commonly contrast the Buddhist view of time with that of the Indian ‘realist’ schools, who held that time is a substance. For example, Stcherbatsky in his *Buddhist Logic* tells us that whereas ‘the Indian realists, just as some European rationalists, considered Time and Space as two all-embracing receptacles containing each of them the entire Universe’, the Buddhists, on the other hand denied ‘the separate reality of these two receptacles’ (Stcherbatsky 1993: I.85). This denial was based on the concept of reality as that which is causally efficacious. The domain of reality stands opposed to the domain of the fictitious or imaginary. Time in itself is not causally efficacious: only things existing in time have that property. The everyday notion of duration was criticized as concealing a contradiction. ‘One real thing cannot exist at the same time in many places, neither can the same reality be real at different times. [...] If a thing is present in one place, it cannot at the same time be present in another place. To be present in another place means not to be present in the former place. Thus to be present in many places means to be and at the time not to be present in a given place’ (Stcherbatsky 1993: I.86). We can, to be sure, *imagine* a thing present in many different places at once. I can imagine myself being causally efficacious in some way – for instance, driving a motor-car – simultaneously down Fifth Avenue in New York and round Piccadilly Circus in London, even though these two locations are three thousand miles apart. But that – so the argument goes – is precisely what distinguishes fiction from reality. In reality, that is impossible.

We should note that if this is the Buddhist view, then Wells’s Time Traveller is a good disciple of The Enlightened One; for none of his extraordinary travels conflicts with this conception of time and reality. He never appears in two places at once or contrives to produce effects which have no temporally antecedent cause. Yet his adventures, clearly, run counter to our lay intuitions about time, and are intended to do so. This points to the fact that there is something

missing from any theory which denies reality to time simply on the basis that only what is causally efficacious is real. In effect, this begs the question by presupposing that causal efficacy can be exercised only locally and that getting from one location to another, however rapidly, takes time. Furthermore, it presupposes that causes cannot be effective retroactively; in other words, the notion of causation invoked here already has a temporal directionality built into it. On both grounds, the rationale for denying reality to time is flawed.

Buddhist thinkers drew a very different conclusion than Timaeus did from the phenomena of transience. It led them to the doctrine of *ksanika-vada* (or the theory of ‘instantaneous being’). Transposed into Timaeus’s terms, what this means is that ‘*what is*’ is captured only in the fleeting point-instant, which has already gone almost before we can grasp its actuality. Stcherbatsky (1993: 81) quotes Kamalashila as maintaining that an object can be causally efficacious only when it has reached the last moment of its existence, which is also its unique real moment, and that all other moments are non-efficient. Thus, for example, when a seed turns into a sprout, this is done at the last moment of the seed’s existence, and all the time when it lay dormant in the granary counts for nothing. But this poses no obvious threat to the lay logic of time.

What does pose such a threat, although a subtle one, is Nagarjuna’s account of how we measure or evaluate things:

In this world the measure and the measurable are not independently given, since only when the measurable exists does the measure measure, and only when the measure exists is the measurable measurable. The measure requires the measurable for its establishment, and likewise the measurable the measure. Thus the measure itself becomes what is measured by the measurable, while the measurable in turn becomes the measure of the measure. Only in virtue of this mutual dependence is either conceivable. Each of the pair is both measure and measurable. Therefore neither is independently knowable.

(*Refutation of Logic*, Section II.)

What I have quoted is my own adaptation of the Tola-Dragonetti translation from the Tibetan (itself a translation of the Sanskrit original now lost). I have preferred ‘measure’ to ‘means of valid knowledge’, ‘mode of proof’, ‘criterion’, etc. because these more usual Western technical translations of the term seem to me too far removed from the (metaphorical) Sanskrit *pramana*. This too has a relevance to language, although the connexion may not be immediately obvious. It emerges, however, in the following later passage where Nagarjuna discusses the notion of verbal repetition. (Again, I have adapted the Tola-Dragonetti translation.)

What is said cannot be repeated. For repetition would require further words. But unless the further words were identical with those previously uttered, the attempted repetition would fail, granted that difference is incompatible with repetition. However, unless there *is* difference the words are not additional to those originally uttered, but the same. Therefore, what is said cannot be repeated.

(*Refutation of Logic*, Section LXX.)

A typical modern Western reaction to this argument would be to object that it is based on a confusion between types and tokens. But in my view such a reaction would be inadequate. For granted that the original utterance is spatio-temporally unique, and that any subsequent utterance which is claimed to be a repetition is likewise spatio-temporally unique, the question that is being raised is the basis of comparison between the two. And here we go straight back to Heraclitus and to Plato. Are we deceiving ourselves when we assume that, in some sense still to be explained, ‘the same words’ were uttered on two separate occasions?

Time and linguistics

The only Western school of modern linguistics, as far as I know, which would entertain the possibility that Nagarjuna is right is the integrationist school (Harris and Wolf 1998; Harris 1998). On this subject they have been severely criticized by more orthodox (Platonistic) Western thinkers. They have even been accused of

logical lapses. For instance, it has been argued that even to state the integrationist position on repetition involves a self-contradiction. I quote one such criticism:

To make the point that there is no repetition of *well* or *How's that?* we actually have to use, overtly or covertly, and not merely mention, a type, category or form *well* or *How's that?* We may not be able to give a plausible derivation of all the utterances which have counted or would count as utterances of *How's that?* and we may be in the dark about how speakers and hearers produce and understand such utterances; but that they make use of some iterable form not defined by temporal co-ordinates seems presupposed by the very critique of that idea.

(Pateman 1987: 251)

It is important here to note the phrase 'not defined by temporal co-ordinates'. I have drawn attention elsewhere (Harris 1996: 186ff.) to the way in which this objection echoes the notion of iterability as interpreted by Jacques Derrida, who maintains that the meaning of a text must be independent of any intentions on the part of its author, and hence, presumably, independent of any circumstances in which the text was originally produced. Derrida is orthodoxly anti-Heracleitan in the sense that he believes that before anything can be said at all we have to admit that 'la langue est déjà là'. (On Derrida's misrepresentations of Saussure, see Harris 2001: 171ff.)

I doubt whether Derrida has read Nagarjuna or any translation of Nagarjuna. But if he had, I imagine that he would dismiss it with some version of the types-and-tokens argument. For integrationists, however, this will not do. For, once again, it begs the question: the question being whether we can make sense of a form of words we have never encountered before. Precisely this issue is at the heart of both Saussurean and Chomskyan linguistics in the twentieth century. Saussure addressed it somewhat more crudely than Chomsky, but in all essentials their answers are indistinguishable. Saussure solved the problem by postulating a communal synchronic system to which all individual members of the linguistic community had access. Chomsky solved it by postulating a mental system of rules enabling

the individual to compute the meaning of any previously unheard sentence on the basis of previously heard sentences plus a biologically given ‘Universal Grammar’ (with the circular proviso that the previously unheard sentence already conformed to the rules of Universal Grammar). As with Derrida, *la langue est déjà là*.

Saussurean synchrony

Saussurean synchrony is a time-slice from which change is hypothetically eliminated. That is what enables the same words to be repeated in *parole*, to occur in different syntagmatic combinations, and hence allows the foundations of linguistic structure to be established. In diachrony, on the other hand, there is no repetition. Words may be linked etymologically across a time-gap of centuries; but they are never ‘the same word’ (contrary to what earlier etymologists had assumed). Synchrony, in short, is a way of stopping time for the linguist’s own theoretical purposes.

But the price paid for this manoeuvre is theoretical confusion elsewhere. We see this in Saussure’s concept of ‘linearity’, which he erected into one of the two fundamental features of the linguistic sign. This feature, he asserted, was ‘borrowed’, from time (*Cours de linguistique générale*, p. 103). That is putting the cart before the horse. Only if time is conceptualized as a continuous straight line in the first place can there be any question of linearity as a property transferred to language. Worse still, Saussure also maintains that writing copies speech by preserving linearity in letter sequences. So this imaginary characteristic of time is projected on to writing at one remove. The confusion is complete. Nowhere is Saussure willing to admit that the basic parameters of the spoken sign and the written sign are fundamentally different, because that would have necessitated the establishment of two different forms of linguistics.

On the other hand, Saussure insists that all synchronic notions and all diachronic notions are mutually irreducible. Yet ‘time’ is a notion common to both categories. What is going on here? The answer is that Saussure is trying to make time do two different jobs at once. Synchronic time is the time it takes to utter any given *signifiant*. But this is inadequate when it come to explaining the time it takes for a later *état de langue* to replace an earlier *état de langue*. For this we

need diachronic time. But what is the relation between the two? In desperation, Saussure is forced to admit that an *état de langue* is actually a period of time ‘during which the sum total of changes occurring is minimal’ (*Cours*, p. 142). But how there can be changes *within* a state without invoking diachronic time is never explained. And why it does not follow that a single comprehensive state may have a duration that includes a series of short-lived states is not explained either.

For integrationists, these barefaced intellectual compromises are evasions of the issue. I think that Nagarjuna would have regarded them likewise. The question is: why are they necessary? And the answer is that without them, grammar (whether in the sense of Panini or of Dionysius Thrax) is immediately threatened. *Homo grammaticus*, grammatical man, desperately needs a foundation for his pathetic belief that one can actually ‘set out’ the rules of grammar in some quasi-definitive form, even while conceding that tomorrow the rules may have changed. This is what underwrites Saussure’s absolute division between ‘synchronic’ and ‘diachronic’ linguistics. There is no definitive account of diachrony, because diachrony is adventitious, not systematic. (Which is another way of saying that it is not predictable.) So here we have the ultimate Platonic paradox: language captures only the here-and-now. And that is already a puzzle, because by the time it has been put into words we have already marched forward to the next temporal event. The river has flowed on. It is no use the football commentator screaming at the top of his voice ‘It’s a goal!’. He should have said: ‘It *was* a goal!’.

We find the similar contentions in non-Buddhist traditions of Eastern thought. For example, it crops up in China in the ‘Mohist Canons’. Here we encounter such claims as that ‘Spatial positions are names for that which is already past’ (Yinzhi 1986: 208). This chimes well with the perspective proposed by Timaeus. It calls in question the whole concept of a *synchronic* grammar.

Grammar and education

It is not only grammarians who need be upset by the outrageous concept that repetition is an illusion. The whole basis of education,

both in the East and in the West, rests on the notion that texts can be passed on from one generation to the next. It does not matter whether this transmission is oral or written. The same logic applies to both. Learning by rote is a canonical classroom procedure in both East and West: and learning by rote is learning by repetition.

But if there is no trans-temporal access to what the texts mean, then education itself is illusory. The very fact that texts attract commentaries, exegeses, explanations seems to suggest that in both Eastern and Western traditions there is recognition of a semantic problem in interpreting at a later time what was originally put into words at an earlier time. Plato himself casts doubt upon the reliability of writing. Not because there may be errors of transcription. His misgivings go far beyond that. His view, if it is correctly reported in *Letter VII*, is that:

any serious student of serious realities will shrink from making truth the helpless object of men's ill-will by committing it to writing. In a word, the conclusion to be drawn is this; when one sees a written composition, whether it be on law by a legislator or on any other subject, one can be sure, if the writer is a serious man, that his book does not represent his most serious thoughts; they remain stored up in the noblest region of his personality. If he is really serious in what he has set down in writing 'then surely' not the gods but men 'have robbed him of his wits'.

(*Epistles* 344; Hamilton 1973: 140–141)

Plato is here quoting Homer (a form of repetition), perhaps ironically. For Plato was no less implacably opposed to treating the poems of Homer as a repository of wisdom than to taking written compositions in the same vein. But in spite of Plato's protests, the preservation of written texts was to become the basis of Western education in the medieval universities. It would be no exaggeration to say that the universities of Europe, like the Church, regarded themselves as the guardians of a textual tradition. How the texts were interpreted was another question.

When conflicts of interpretation arose, the Church claimed the final word. When the Church authorities condemned the writings of

Peter Abelard, and forced him to throw them into the fire with his own hands, they were making as much a linguistic as a theological point. The message was: ‘The Devil take all your Latin learning if you use it to dispute the Church’s interpretation of what a Latin text means. Religious truth takes precedence over linguistic truth; and if you put Latinity before Christianity then you are a heretic, and you will go into the fire along with your books.’

Grammar and logic

In spite of all doubts, from Aristotle onwards the (Western) rules of logic were held to be valid *across* synchronic grammars and their rules, and *in spite of* variations of vocabulary and changes of meaning. Until the twentieth century there was no Western logic which took time into account. (My former colleague and neighbour, the late Arthur Prior, in addition to inventing the infamous sentential connective *tonk*, was the first to introduce logical operators for past and future tenses: Prior 1967.) The classical syllogism was timeless. This was an intellectual tyranny no less absolute than that of the Church. The two often combined. We are not talking here about trivial matters of conformity to what the neighbours say (although there is that too). We are getting to the core of what counts as a rational being, made in the image of God. In the Western tradition there is a curious process of abstraction which elevates ‘the sentence’ into the timeless ‘proposition’ (*The Language Connection*, 1996).

Suspicious about this process bring us back to H.G. Wells. He is a much underestimated figure in modern thought (because he can so easily be dismissed as writing pot-boilers for a popular market – which he did). Taking a less superficial view of Wells involves seeing that, for all his limitations, he had spotted that Western culture had come to the end of its reliance on a stable, predictable, on-going world of the classical Newtonian kind with a timeless logic, guaranteed by an omniscient and benevolent divinity. That doubt itself might be taken as prescience of huge social and scientific upheavals that were yet to come. But Wells’s social novels never intrigued the general public as much as his science fiction. I left unanswered the question as to why the exploits of the Time

Traveller confound our lay expectations about time. It seems to me a mistake to think that this is because these exploits conflict with the traditional Heracleitan perspective: they do not. But what they do assume is something no less perplexing; namely that within the flow of time there are different time-tracks available. There are fast lanes and slow lanes, as on a motorway. That is what enables the Time Traveller to visit the remote future and get back in time for dinner.

Wittgenstein wrote, during Wells's lifetime: 'It is as impossible to represent in language anything that "contradicts logic" as it is in geometry to represent by its co-ordinates a figure that contradicts the laws of space, or to give the co-ordinates of a point that does not exist' (*Tractatus*, 3.032). If Wittgenstein had read Wells, he might have modified this pronouncement. What Wells did, in advance of Wittgenstein and in a quite different domain of discourse, was to make the point that we cannot take the everyday language of time on trust (as Timaeus had already insisted many centuries before). It is this, even more than the possibility of time travel (recently accepted by some mathematical cosmologists), that upsets our usual expectations of language. We take it that it makes sense to ask someone to repeat what they just said. Our courts of law require us to 'repeat the oath' as the clerk dictates. We take it that there are criteria for deciding whether that instruction has been complied with. And if we are told that all this cannot be assured, then we are hard put to it to reconcile this with the way we conceptualize the workings of language. A language in which it was impossible to repeat the word *well* or *How's that?* or what the clerk of the court said is seemingly beyond our comprehension. It would not be, we might feel inclined to object, a language at all. Wittgenstein wrote: 'The limits of my language mean the limits of my world' (*Tractatus* 5.6). He should perhaps have added: 'And the limits of my language are the limits of my sanity.'

Integrating time

As an integrationist, I do not see here any set of paradoxes to be dissolved or an impasse from which we must urgently seek an escape. If there is any impasse, it is one constructed by the intellectual engineers who were supposed to be clearing the road for

us. Nagarjuna was right in that he recognized what integrationists many centuries later call the principle of ‘cotemporality’ (Harris 1998: 81ff.) This requires us to suppose that ‘what is said is immediately relevant to the current situation, unless there is reason to suppose otherwise.’ But how that ‘current situation’ is conceptualized, where its boundaries lie, and within what limits we can participate in it, are matters that require contextualization. To that extent they are controversial, i.e. open to verbal ripostes which call them in question. This applies not only to what we say but to everything we do. In this respect there is complete parity of status between linguistic acts and other acts (hence ‘cotemporality’). Linguistic acts do not have some special temporal status of their own.

This has been obscured by the way in which orthodox modern linguistics has lent its authority to the notion that linguistic signs are ‘types’, which automatically guarantee the possibility of producing tokens *ad infinitum*. This conviction seems to me to be based on the tacit assimilation of temporal configurations to spatial configurations. The assimilation is already built into the Peircean term *type*, originally introduced into the theory of signs in order to explain how we count words on a printed page. The marks of writing are spatial configurations, and we compare them by matching up their spatial features. In this process time is ignored as an irrelevance. It does not matter where we start or in what order we take the marks. It does not matter which word was printed first. Spatial likeness abstracts from time.

Orthodox twentieth-century linguistics assumes that this holds for speech as well. In that respect, it treats the spoken word as an extension of the written word, an assimilation encouraged by the endemic scriptism of the Western grammatical tradition (Harris 1998: 123). Integrational semiology rejects this assimilation. It rejects ‘medium transference’. It treats writing as a quite different form of communication from speech, with a structure that is *sui generis* (*Signs of Writing*, 1995). In this way integrationism avoids the theoretical incoherence that is at the root of the orthodox treatment of writing.

The type-token distinction cannot be grounded in the postulate that tokens (of a type) are identical in all respects *except* those

ensuing from their independent temporal existence. That is doubly incoherent, for (i) there is no way in which all and only those respects could ever be identified, and (ii) there is consequently no way in which a complete characterization of the type could ever be given. A thing or an event cannot be compared to itself, since comparison requires two (or more) comparanda. Nor does it make sense to say that the *only* respect in which two things differ is their separate temporal existence. For if *that* differs, then so too must their relations with other existing things, and hence indefinitely many distinctive properties of each that depend on those relations. I think Nagarjuna saw that.

Where Nagarjuna got it wrong, from an integrationist perspective, is that his argument as presented in the *Refutation of Logic* depends on the proviso that difference is incompatible with repetition. For integrationists, we all make and re-make our meanings as we go; and repetition is one of them. We make it by recontextualizing what was said before. (But 'before' is defined not by reference to 'linearity', nor to Newtonian absolute time, but to our experience of continuity.)

If the limits of my language are the limits of my sanity, then I have no fears for the latter, only for the former. And those limits are threatened by the notion that my words and what I can mean by them are always bounded by some enduring set of rules or relations, in the way that both Saussure and Chomsky proposed. But, *pace* Pateman and like-minded critics, that does not mean that I have to embrace some Platonistic alternative, where the parameters are *not* 'defined by temporal co-ordinates' but set by eternal verities (or assumptions about them) which transcend the particularities of here-and-now. Nor am I moved by the objection that appealing to 'here-and-now' already presupposes a conceptual framework in which the present moment leans for its validation on eternity, or if not on eternity then at least on some larger time-frame than is supplied by any Buddhist point-instant.

The right way to look at problems of signification is the other way round. Integrational semiology is unashamedly lay-oriented. It assumes that we do not need philosophers or theorists to interpret our own everyday experience for us. And that experience tells us that it is not possible to skip tomorrow or to have yesterday back again. Sleeping for twenty-four hours does not eliminate the day after.

Remembering what happened does not re-instate the day before. Neither oblivion nor recall escapes temporal sequentiality. The future and the past are both extrapolations from the here-and-now, one based on imagination and the other on memory. And language, rightly understood, reflects and enables those extrapolations.

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