1 Breaking us in two

Cracks in the canvas

There's craic *in everything;*
To let the light get in.

Had Leonard Cohen come from Belfast, and if he had been more cheerful, the lines above might form part of the refrain to his elegiac song 'Anthem'; they would scan just as well. But he didn't, and since I can't afford to republish even two lines from the original, they will have to serve as a placeholder.[1]

♫ Leonard Cohen, *Anthem* (1992)

When teaching a course on language and mind – whether it is on language processing, language acquisition or language disorders – the first thing I have students do is watch a BBC *Horizon* documentary, originally broadcast in 2009, with the title 'Why Do We Talk?' As it turns out, this 50-minute programme offers no clear answers at all to *why* we talk, probably because this is an unanswerable question: philosophers, theologians, poets and other thinkers have been trying for millennia to come up with a viable theory of human action – why we do anything – and the results so far have been less than encouraging. Free will is a troublesome concept, for one thing. What the programme provides instead is an articulate introduction to some core research questions in the area of language and cognition, many of which I aim to re-examine in this book. These include, in some particular order: the question of whether the ability to acquire and use languages appropriately depends on a special kind of mentally represented knowledge – a 'language faculty' – or whether this ability rests on more general cognitive capacities; supposing we all possess such a language faculty, where the knowledge it instantiates comes from; the extent to which the environment – more specifically, naturalistic linguistic input – shapes grammatical development; the extent to which language comprehension and language production skills are dissociable; the evidence for a genetic basis to language; the problems of learning artificial languages; and the possible mechanisms of human language evolution. A slew of fascinating topics, then. The broadcast also introduces viewers to some of the

key figures in research on language and cognition, and provides a passing glimpse of the experimental methodologies that have been employed to elicit data from language users. All things considered, it is a remarkably useful, engaging and compact piece of television, which is why I have students watch it. You should too, if you can obtain a copy of the programme.

However, to someone who has worked in the field for over twenty years, it is not the information that emerges from the interviews with language researchers that is particularly surprising. If I wasn't reasonably familiar with the general research questions and the results that have so far been obtained, I shouldn't be the one writing this book. Instead, the most astonishing feature of the programme is the implied consensus among researchers concerning the answers to these key questions: through careful narration and skilful editing, the programme-makers create the impression that linguists and psycholinguists broadly agree on all the answers to the core issues in language and mind. This is airbrushing on a massive scale, and about as far from the truth as one can get without actually misrepresenting any of the individual contributors to the programme. In the 'real world' – that is to say, the *academic* real world of psycholinguistics – what is mostly observed is not consensus, but ambivalence, controversy, dissent and energetic disagreement about nearly all of these questions, not to mention a fair degree of bitter antagonism and hair pulling. It is often one's own hair that is pulled, but still: in terms of social and political discourse, linguistics is more saloon than *salon*, more gladiatorial arena than forum.

There is a positive thing in some respects. A useful analogy here is to seismology. If you want to study earthquakes, two good places are Los Angeles, California, where I attended graduate school, and Kōbe, Japan, the city where I now live. Seismologists don't spend a lot of time in Belfast, the city where I grew up. In both regions, only just beneath the surface, the ground is fragmented by myriad fissures and thousands of minor fault-lines; it is also more obviously fractured along major fault lines, immediately visible to the naked eye. In California, the most significant seismic fault is named after a Catholic saint, San Andreas, whilst in this part of Japan, it is the more prosaically named Japan Median Tectonic Line (中央構造線 *Chūō Kōzō Sen*), a branch of which – the Nojima fault – was responsible for the Kōbe (Great Hanshin) earthquake in January 1995, which resulted in more than 6,000 deaths and caused over 10 trillion yen in damages.

In psycholinguistics the most significant fault line is anonymous, but if one were to name it, it should rightfully be called the *Noam Chomsky* fault. Though he is not himself a psycholinguist, still less a saint, Chomsky has had a more profound, and divisive, impact on the field than any other academic researcher. To gloss over the major and minor fault lines in language and mind, as the *Horizon* programme does, is to ignore much of the volatility and friction that makes the subject as treacherous – but also as vital – as it is.

The impression of consensus that the programme conveys also seems to pre-empt further research, which is precisely the opposite of what I want to achieve with this book. By clearly exposing some of the fault-lines of psycholinguistic research – especially some of the smaller cracks, where it is easier to make progress – I hope to encourage readers to develop new experiments of their own. To let the light get in.

Of course *this* truth, that the field is fractured, should hardly come as a bolt from the blue. Issues in language and mind are probably no more contentious than those in any other area of theoretical or empirical enquiry. Nor is it the case that psycholinguists in general are more vituperative than any other group of academics.[2] What's more, if the issues were as done and dusted as the programme's narration implies, the field would be intellectually moribund, and researchers with any talent would long since have moved on to something more challenging. As in all the other sciences, progress in psycholinguistics – the development of better theories and models – generally comes about through strong competition between alternative sets of hypotheses and interpretations. In principle, this competition could be dialectical in nature, the opposition of thesis and antithesis leading to a *synthesis* – a resolution – of the two opposed views. In practice, psycholinguistic arguments too often end in 'winner takes all' outcomes, which may better satisfy a journal reviewer or a funding agency but which do less to uncover the truth of the matter.

A direct consequence of the intellectual fragmentation of psycholinguistics is that many textbooks offer a partisan view, variously downplaying or dismissing – more often than not, completely ignoring – relevant research from the other side(s). These biases are particularly stark in discussions of language acquisition, but the problem of bias also extends to books on adult language processing and language disorders. One only need contrast introductory works by Guasti (2004) or Crain and Thornton (1998), for example, with those of Harris and Coltheart (1986) or Tomasello (1992), to observe systematic biases of reporting more typically associated with ministries of propaganda than with reasoned academic discourse.

My aim here is to provide a more balanced, 'well-tempered' treatment,[3] considering the research questions from several different angles, giving credit to researchers operating from different theoretical viewpoints, and drawing together some of the key experiments that have brought us to present-day conclusions. This approach closely reflects my own education and training. Over the past thirty years I have been fortunate enough to learn from a wide range of teachers, colleagues and students, some committed generativists, others equally passionate functionalists, still others 'pure psychologists' with no particular view of linguistic theory. See Acknowledgments, credits and permissions for details. Almost without exception, I have found these people to be intelligent and intellectually honest researchers, academics who respect empirical results

whilst nevertheless disagreeing sharply on what counts as empirical, or even on what they consider to be legitimate research questions. What I have not observed is any obvious correlation between deep understanding and ideological commitment: theoretical zeal can occasionally lead to significant insights, but just as often to remarkable blind spots and arrogant bloody-mindedness. The truth is a grey area: only children, zealots – and some formal semanticists[4] – believe otherwise.

A core distinction is drawn here between two highly contrastive research perspectives, between what might be called the 'two souls' of classical psycholinguistics (to borrow an expression from Gennaro Chierchia 1995):

> On one side we find a COMPETENCE-BASED perspective, inspired and often directly informed by theoretical developments in Chomskyan grammar; the primary concern of competence-based researchers is with the mental representation of linguistic – especially grammatical, and most especially *syntactic* – knowledge, as well as with the question of how such knowledge comes to be in the mind of adult native speakers;
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> On the opposite side of the intellectual fault line lies a PROCESS-ORIENTED ('information processing') approach, which lays emphasis on how speakers comprehend and produce spoken language in real time, in real situations, how children and adult language learners come to acquire the full array of language processing skills that are necessary for the fluent use of particular languages, and on the ways in which these abilities change throughout our lifespan.

These two souls of psycholinguistics are distinguished in the following quote, from Seidenberg and MacDonald (1999: 570):

> *Instead of asking how the child acquires competence grammar, we view acquisition in terms of how the child converges on adult-like performance in comprehending and producing utterances. This performance orientation changes the picture considerably with respect to classic issues about language learnability, and provides a unified approach to studying acquisition and processing.*
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> Mark Seidenberg and Maryellen MacDonald, 'A probabilistic constraints approach to language acquisition and processing' (*Cognitive Science*, 1999: 570)

This book is written largely in the spirit of that quotation, while still trying to do justice to the issues that competence-based theorists consider important. It must be for the reader to decide whether I manage to square that particular circle. I also discuss research questions relating to the MENTAL LEXICON, where issues of abstract representation and information processing are more

intimately linked than in the syntactic or phonological domains, and where ideological commitments are less in evidence. Finally, at various points in the discussion I will touch on experimental research at the interface between psycholinguistics and other aspects of human cognition and brain function, as well as on language typology, change and language evolution.

A(n) historical overview: dividing the soul of psycholinguistic theory

Before setting out these parallel tracks, it is worth noting that the field of psycholinguistics was not always so riven. Interest in psychological aspects of language and/or linguistic aspects of psychology predates the latter half of the twentieth century,[5] but it was only in the 1930s and 1940s in Europe, and the 1950s in the United States, that experimental psycholinguistics emerged as a discipline in its own right.[6] In the immediately preceding decades, the predominance of Behaviourism in psychology, in alliance with the strict empiricism of contemporaneous philosophy (LOGICAL POSITIVISM) and linguistics, had produced a virtual neglect of language in experimental psychology, as well as of psychology within linguistics (especially mainstream North American linguistics). Prior to the 1950s, language was mostly thought of as something 'out there', sometimes exotic, often chaotic and mysterious: linguistic behaviour was regarded as unpredictable as it was unstructured. Most American linguists of the period were concerned with the description of the indigenous languages of North America and elsewhere: many of those who worked in this anthropological tradition proceeded from a theoretical assumption that languages '… [could] differ from each other without limit and in unpredictable ways' (a much-cited, now infamous, quote from Martin Joos (1957: 29). Scholars at the time also generally adopted the methodological position that native speakers' metalinguistic judgments about their own languages were inherently untrustworthy: as Sampson (1980: 64) reports, linguists were minded to 'Accept everything a native speaker says in his language, and nothing he says about it.'

The conceptual impetus for a radically different, universalist and exclusively internalist approach to language in linguistics and psychology was provided by Noam Chomsky, whose early proposals for transformational generative grammar – see especially Chomsky (1957, 1965) – revolutionised thinking about language in many parts of the academic world, and who retains a preeminent influence in the general area of language and mind. McGilvray ([1999] 2014), offers a clear, if partisan, overview.[7]

The 'Chomskyan Revolution' in linguistics came to the attention of psychologists through Chomsky's (1959) critical review of B. F. Skinner's *Verbal Behavior*, which had been published two years earlier, and which had outlined

a strictly externalist account of linguistic behaviour, centred around the notion of OPERANT CONDITIONING. In more hagiographical discussions of Chomsky's work, it is standard to assert that this review article provided clinching arguments that signalled the death-knell of Behaviourism as a viable psychological theory for the study of language (or, indeed, for much else).[8] A case of David vs. Goliath – at least before Malcolm Gladwell's anti-heroic re-interpretation of the Biblical story (Gladwell 2013).

On closer consideration, the issues are considerably less clear-cut, as Palmer (2006) discusses: strip away the polemic, and the arguments against any reasonably nuanced interpretation of Skinner's proposals are less persuasive than they are often held to be; see also Schlinger (2008). Indeed, as Matthew Saxton (2010) makes clear, Chomsky's position at the time was in some respects 'more Behaviourist' than that of Skinner himself: see also Radick (2016). With respect to the role of imitation, for example, Saxton reminds us that it was Chomsky, not Skinner, who claimed that 'children acquire a good deal of their verbal and non-verbal behaviour by casual conversation and imitation of adults and other children' (1959: 42), and who later observed, with regard to grammatical intuitions, that 'a child may pick up a large part of his vocabulary and "feel" for sentence structure from television' (1959: 42). This latter assertion is highly questionable, as subsequent research has shown; see, for example, Kuhl, Tsao and Liu (2003). Yet such remarks have been lost to revisionist history.

Even supposing that Chomsky's critique had delivered a fatal blow, Behaviourism was not killed off overnight. Paradigm shifts in science, like historical grammatical changes, can take several decades at least to work through – centuries, in the case of some syntactic changes. This is the case even though they might appear abrupt in retrospect, and even where they have a clearly pinpointable year of origin; 1066, say, in the history of English. See I is for Internalism below. Moreover, whereas Behaviourism in its classic form might have died as a theory, it has survived well as a methodology: contemporary cognitive psychology inherited from the Behaviourists a concern with rigorously controlled experiments, careful quantitative analysis of elicited data, and replicability as essential aspects of good research practice. (It is another matter, of course, whether such concerns are justified: see Bauer 1994.)

I've learned from my mistakes and I'm sure I could repeat them exactly.

Peter Cook, 'Frog & Peach' (*Behind the Fridge*, 1973, republished in *Tragically I was an Only Twin*, Peter Cook 2002)

Viewed in the round, it seems likely that Chomsky's critique simply nudged Behaviourist psychology in a more internalist direction rather than dislodging it entirely; see also I is for Internalism. Psychologists of language also

generally retained the idea that linguistic behaviour was a worthwhile object of study in its own right, something that Chomsky soundly rejected: whereas generative linguistics abstracts away from 'performance-related' issues and focuses rigidly on static grammatical competence, cognitive psychology is still fundamentally concerned with the contingencies of language performance, and especially, with the constraints imposed by time. See Eysenck (1984), also C is for Competence~Performance, for discussion.

An awareness of temporal constraints on language is by no means restricted to cognitive psychology. As C. S. Lewis, another Belfast native, observed:

> *[A] grave limitation of [spoken: NGD] language is that it cannot, like music or gesture, do more than one thing at once. However the words in a great poet's phrase interinanimate one another and strike the mind as a quasi-instantaneous chord, yet, strictly speaking, each word must be read or heard before the next. That way, language is as unilinear as time. Hence, in narrative, the great difficulty of presenting a very complicated change which happens suddenly. If we do justice to the complexity, the time the reader must take over the passage will destroy the feeling of suddenness. If we get the suddenness we shall not be able to get in the complexity. I am not saying that genius will not find its own way of palliating this defect in the instrument; only that the instrument is in this way defective.*
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> C. S. Lewis, *Studies in Words* ([1960] 2013: 313–14)

Yet much of what seems crucial about language to other writers, philosophers or/and psychologists is largely ignored by most theoretical linguists, not only generativists.[9] See A is for Abstraction, H is for Homogeneity, I is for Internalism, O is for Object of Study (in Part III), for further discussion; cf. Poeppel (2014), amongst others.

Whatever was the true impact of his review of Skinner's work, Chomsky's own theoretical proposals (Chomsky 1957, 1965) did much to kick-start the field of competence-based psycholinguistics as a separate discipline, shifting general scientific attention from the more directly observable aspects of linguistic behaviour – spoken and written utterances, and the corpora derived from them – to the (putative) set of implicit grammatical rules that allow native speakers to acquire and use language productively. More generally, Chomsky redirected psychologists' attention to the 'tacit knowledge' that underlies LINGUISTIC CREATIVITY, which enables speakers to use language in ways that project beyond the primary linguistic data which they are exposed to as children; cf. Sampson (2015). Within generative grammar, the theory of this implicit grammatical knowledge has come to be referred to as UNIVERSAL GRAMMAR (or UG, for short); for clear discussion, see especially Crain and Pietroski (2001). Although the precise characterisation of UG has been revised continuously since Chomsky's early work, claims about its fundamental nature have remained largely unchanged: by hypothesis, UG is internal, implicit

(inaccessible to consciousness), INTENSIONAL [with an 's', see below] and domain-specific. Most crucially, perhaps, for many researchers in this tradition, UG is also innate:[10,11]

UG is used in this sense … the theory of the genetic component of the language faculty … that's what it is.

Noam Chomsky, *Poverty of Stimulus: Some Unfinished Business* (2010)

Consequently, for psychologists and philosophers persuaded by Chomsky's approach, theoretical interest in language resides not in linguistic behaviour *per se*, nor in the study of different languages (in any ordinary person's understanding of that term), but rather in a hypothesised mental organ – the innate LANGUAGE FACULTY – that is supposed to make linguistic behaviour possible, and whose epistemic content is assumed to set strict formal limits on grammatical variation.

Initially, psychology appears to have greeted Chomsky's proposals enthusiastically, with many experimentalists of the period setting out to test the 'psycholinguistic reality' of the theoretical constructs of TRANSFORMATIONAL GENERATIVE GRAMMAR (TGG). A key construct of early TGG, which received a good deal of attention from psychologists, was the distinction between the 'surface structure' of a sentence and its 'deep structure', these two levels of representation being related by a set of TRANSFORMATIONAL RULES, which moved, inserted or deleted phrasal constituents.

This model can be illustrated by considering the English passive construction. In TGG it was proposed that active and passive paraphrases of a given proposition such as 'Alice drank the potion/The potion was drunk by Alice' shared a common deep structure, but differed in the number of transformational rules necessary to derive the two surface variants, with more transformations applying to the (ostensibly more complex) passive structure.[12] Early psycholinguists reasoned that if these theoretical constructs were psychologically real, and if they were isomorphic with the processes of language comprehension and production, then grammatically more complex sentences – for instance, those involving more transformations in their derivation – should incur greater processing costs relative to derivationally simpler sentences. An additional premise was that these costs should be directly measurable in terms of increased RESPONSE LATENCIES – informally known as 'reaction times' – and/or higher error rates. This reasoning formed the basis of what became known as the DERIVATIONAL THEORY OF COMPLEXITY (DTC).

In spite of some early apparent successes, e.g. Miller and Chomsky (1963), Miller and McKean (1964), the DTC foundered rather quickly, as further

empirical work failed to show any transparent relationship between derivational complexity and processing costs.[13] A much-cited quote from Fodor, Bever and Garrett (1974: 368) summarises the state of play by the mid-1970s:

Investigations of DTC … have generally proved equivocal. This argues against the occurrence of grammatical derivations in the computations involved in sentence recognition.

Jerry Fodor, Thomas Bever and Merrill Garrett,
The Psychology of Language (1974: 368)

With hindsight – and especially given the crucial distinction between levels of explanation outlined in Marr ([1982] 2010), which I'll come to in a moment – the absence of any direct correspondence between transformational depth and reaction times or error rates is unsurprising. It's easy to be clever after the fact. Nevertheless, the alleged failure of the DTC was one of the factors that led many psychologists interested in language to turn away from the grammatical theories offered by formal linguists. Many never turned back.

There were several other reasons. For one thing, generative research was – and generally remains – restricted to the level of the canonical sentence, that which begins with a capital letter and ends with a full stop; see T is for Sentence, v is for von Humboldt. By contrast, experimental psychologists were typically more interested in smaller or larger units of speech: for example, in the problems of real-time word recognition, the role of lexical frequency in acquisition and processing, or the interplay of grammatical and pragmatic information in the interpretation of spoken and written discourse, to cite just a few relevant issues. Two key findings of the period were those of Sachs (1967), whose results suggested that listeners do not retain any conscious memory of the surface syntactic form of an utterance, though they do retain its meaning, and slightly later, the work of Bransford and Franks (1971), whose experiments implied that the final interpretations that listeners derive from sentences involve (inextricable) inferential content that is not represented anywhere in the deep structure of the sentence; in other words, listeners are unable to disentangle assertions from inferences.[14] Many psycholinguists came to interpret findings such as these as suggesting that transformational grammar had little empirical – or even heuristic – value.

One final consideration was as much sociological as it was empirical: psychologists turned away from theoretical linguistics because generativist theory was almost wholly unresponsive to their results, supportive or otherwise; see Cutler (2005). *Plus ça change*. Formal linguistic theory (generative theory, at any rate) has developed considerably since the 1960s, and especially since the mid-1990s, but this has invariably been in reaction to internal theoretical

arguments – to a lesser extent, to new intuitional data – rather than to the empirical results from the types of group studies favoured by psychologists. The University of Massachusetts linguist and acquisitionist Tom Roeper summed it up nicely back in 1982 (in a quotation cited by Newmeyer 1983):

When psychological evidence has failed to conform to linguistic theory, psychologists have concluded that linguistic theory was wrong, while linguists have concluded that psychological theory was irrelevant.

Tom Roeper, 'Review of *Linguistic Theory and Psycholinguistic Reality* (1981), edited by Halle, Bresnan and Miller' (1982)

It could be argued that generativists' dismissal of relevant data is a near-inevitable consequence of strictly deductive modes of reasoning. By definition, the inductive mode of enquiry favoured by most psychologists is more responsive to new data than the deductive approach pursued, in its purest form, by Chomsky and others at the vanguard of generative research. This Galilean (hypothetico-deductive) style can be clearly appreciated by watching a recent lecture, recorded at CNRS in Paris in 2010.[15] See Box A below for an excerpt from the transcript; for a detailed critique, see Appendix B (website). Over 120 or so minutes, Chomsky develops a logically compelling discussion of I-LANGUAGE and UG – compelling, as long as one grants all of the segues from description to theory, and all of the necessary auxiliary assumptions, very few of which are presented with supporting evidence. More generally, Chomsky is often dismissive of the use of certain kinds of empirical data – particularly quantitative data – in counter-arguments to his theoretical positions. The following comment from another article is typical of his response to evidence-based challenges from non-generativists:

[Galileo] dismissed a lot of data; he was willing to say: 'Look, if the data refute the theory, the data are probably wrong.' And the data that he threw out were not minor.

Chomsky, Belletti and Rizzi, *On Nature and Language* (2002: 98, cited in Behme 2013)

It is hard to see that such a stridently anti-empiricist position is defensible, let alone commendable: see Yngve (1986) for a diametrically opposed view.

Whatever the relative weighting of these various factors may have been, the psycholinguistic paradigms of generative linguists and those of psychologists had largely drifted apart by the mid-1970s, giving rise to the 'two souls' situation that persists to the present, and which is reflected in the partisan publications mentioned above.

♫ Leonard Cohen (words), Sharon Robinson, *Alexandra Leaving* (2001)