Inside Psychology: a science over 50 years

Edited by

Pat Rabbitt (Emeritus Professor, University of Manchester, UK)

Senior Research Associate, Department of Experimental Psychology, University of Oxford, UK
and
Senior Research Fellow, Department of Psychology, University of Western Australia, Perth, WA

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Psycholinguistics in our time

Anne Cutler

A nostalgic vignette

It's hard to deny that some of the most radical changes in our scientific lives over the past decades have been in the mundane trappings of our daily work. The last words on the last page of my—beautifully formatted—PhD thesis (Cutler 1975) are: 'This dissertation was typed by Arlene Walker'. That is how we prepared documents in 1975—remember? We had someone else do it. Now, I venture to suppose that all chapters in the present book were typed straight into a computer by the authors.

Like other branches of science, the practice of psychology thus no longer offers employment to substantial numbers of secretaries. Careers like that of Arlene Walker don't happen any more (having put herself through college with part-time secretarial work, she did a PhD at Cornell with Eleanor J. Gibson, and is now full professor of psychology and associate provost at the University of Montana: http://psychweb.psy.umt.edu/www/faculty).

Of course, psychologists are not alone in having had their daily existence transformed by technological advance. Perhaps more than many others, though, we endure change in the very content of our work as a result of what technology offers; the computer as a model information processor is one obvious case. Psycholinguistics, my niche in contemporary psychology and in this book, has certainly been immensely technology-driven. See the section 'Technology in the driving seat' below for more on this. But first, some background on psycholinguistics and why it is a little different from many other areas of psychology in our time, and an account of how, somewhat against the odds, I found my perfect niche there.

A brief history of psycholinguistics

The biggest change involving psycholinguistics is that it exists now, and 50 years ago it didn't. As I write this in 2007, it is 50 years since the publication of Chomsky's *Syntactic Structures* (Chomsky 1957). By launching the notion of a grammar as a device for generating the sentences of a language,

Chomsky made linguistics suddenly exciting in a way it had never been before, and this new excitement in linguistics was one of the reasons for the growth of psycholinguistics as a recognizable field of study in its own right. Each of the parent disciplines, psychology and linguistics, boasts an independent research tradition going back centuries, but psycholinguistics itself was born only in the second half of the twentieth century.

Psycholinguists want to know how language structure relates to language use. A psycholinguist like me is primarily a psychologist, seeking to understand the mental structures and processes involved in the use of language. Other psycholinguists who are primarily linguists are more concerned with the patterning of language itself. The common factor that makes us psycholinguists, though, is the cross-disciplinary contact. Thus, I need to wonder about why language has certain universal characteristics, how it can vary in language-specific ways, and how these aspects of structure impinge upon the way language is processed; my linguistic colleagues must be interested in explaining patterns of language performance, and must also be open to evidence from laboratory studies with highly controlled processing tasks. We all need to be interdisciplinary if psycholinguistics is to succeed in solving its core problems.

The relation between psycholinguistics' two parent traditions has changed several times over the years, and has differed across different areas of the field. Language acquisition had a long and strong tradition, for observational techniques predate experimental labs. This early research tradition, from the nineteenth and early twentieth centuries (think of Stern and Stern, think of Piaget), viewed language acquisition as part of the general cognitive and social development of the child. Many hold this view today. But a separate parallel line of research (also still going strong) arose from Chomsky's proposal of an innate and universal 'language acquisition device', and this, in principle, made acquisition a central topic in the study of the human language faculty. It also provided basic assumptions that strongly influenced all the rest of psycholinguistics at the time.

Indeed, owing to the Chomskyan revolution, linguistics was able to set the tone as adult-language psycholinguistics got started in the 1960s. Much empirical research was aimed at deriving processing predictions from linguistic models, in particular from grammar models. The Derivational Theory of Complexity is the best known of these. It proposed that the complexity of grammatical derivations of sentences in transformational grammar could directly predict the processing complexity of the same sentences. Experimental support for this proposal was found (e.g. Miller and McKean 1964), and psycholinguists of the time also did their best to test rival grammatical theories against one another (e.g. Clifton and Odom 1966).

This period ended when the linguistic theories changed—solely in response to linguistic argumentation and not at all in response to the growing body of processing evidence. This was, understandably, not a little frustrating to psycholinguists, who had spent years gathering the relevant evidence. The result was a period when psychological studies of language processing tried to maintain independence from linguistic theory, with the tone set by psychological issues alone. Linguistics returned to psycholinguistics only in the 1980s, with a new growth of research in sentence processing, including processing models that were intended as linguistic proposals (e.g. Frazier and Fodor 1978). Current psycholinguistic research is more integrated still.

Two formative influences

In most universities the parent disciplines I referred to are located in separate faculties, so that psycholinguists generally come to the field via courses taken in a psychology department or a linguistics (or language) department. But the establishment of such courses began in America only in the 1960s, and elsewhere even later. When I started my university course, psycholinguistics was unheard of in Australia.

Recent neurophysiological discoveries suggest that an ability to discriminate foreign speech sounds depends in good part on white matter endowment in the auditory cortex (Chee *et al.* 2004; Golestani *et al.* 2006). Armed with this new knowledge, I now view my grandfather's enthusiastic involvement in the early days of radio in Australia and my own secondary-school foreign-language results as offshoots of the same genetic heritage. But at university all that was on offer for the bearer of such a heritage was preparation for a school teaching career. The associated school teaching scholarship further constrained my choice of courses. Psychology was not so much a choice (my choice would have been biology, but the timetable made it impossible) as a necessary evil to fulfil the 'science subject requirement' in a general arts degree.

The introductory psychology lectures at the University of Melbourne in 1962 were chiefly aimed at convincing the suffering recipients that psychology really was a 'science subject'. Rats (not known for communicating in language, of course) figured largely. The main message to me was clear: psychology was not for me.

Enter, quite by accident, formative influence number 1.I was swotting for the final exam in the library but couldn't take one word more of what I was reading—my memory tells me it was Cohen and Nagel (1934) on scientific method, which is unfair to an excellent treatise, but revealing about the sort of department that expects first-year students to read it. Wandering around the library I came across the new acquisitions shelf, where I picked up a big

blue-green book. It proved to contain: an article about an American Indian tribe who expressed concepts like 'house' and 'water' as verbs; an account of how to produce intelligible speech from patterns painted on glass; proof that children could invent the correct plural forms of new words that they had never heard before, such as *wug* or *wuck;* a grammar machine that could generate an infinite number of sentences, with the further proposal that speakers could be thought of as embodiments of such a machine. And much more. So—all this was psychology too? Was it perhaps worth going on with this subject after all?

It was years later that I worked out that the book must have been Saporta's collection of readings called *Psycholinguistics* (a title that would have said nothing to me at the time). Blessings upon the University of Melbourne library for having a book published in 1961 as a new acquisition in November 1962, and for putting the new acquisitions on a shelf where the first-years could freely read them.

Saporta thus ensured that I stuck with psychology (and the many more rats) right through to my degree. My ignorance of psycholinguistics, though, was still not disturbed by any of the courses I took. In America, by now, a few forward-thinking universities had started cross-faculty PhD programmes sponsored jointly by linguistics and psychology departments (not easy then, just as it is not easy today). One such programme was at the University of Illinois, and one of its early graduates was Australian, Ken Forster. He returned to Melbourne as a postdoc and taught a series of lunchtime seminars on psycholinguistics in my final year, and he needed a research assistant who could speak German, exactly at the moment that I needed some short-term employment to bridge a gap between my exams and my departure on a scholarship to Europe. That RA time was formative influence number 2—see Forster and Clyne (1968) for evidence that it wasn't time wasted. Those who know Ken—among them a substantial population of Australian psycholinguists he has launched in the field—will know his gift of talking to everyone at the same level. I cultivated the habit of greeting him each day with coffee, to receive in return remarks like: 'What do you think of this idea I had this morning...'. There was no chance that he ever got a useful comment back, but golly, did I learn a lot about how to formulate and test scientific ideas.

The Forster experience sent me, after some necessary detours (for school teaching scholarships have to be paid off in teaching time), to America to do a PhD in psycholinguistics. It was a good time to be a psycholinguist interested in spoken language, and with a polyglot background, because there were few like me. It was a good time to be in this new field, because it had attracted the attention of the Max Planck Society, whose institute for psycholinguistic

research was set up roughly coincident with my emergence from graduate school. That institute (where eighteen years later I became a director) was and still is dedicated to bringing psychological and linguistic expertise together, and without it, advances in psycholinguistics would have been much harder to achieve.

Technology in the driving seat

Technological advance prompted many of the changes in the field. Procedures for chronometric analysis in experimental psychology had first prompted research interest in language processing anyway. The tape recorder made possible controlled and replicable research on spoken language, and from the 1970s onwards digital signal processing techniques made computer-based analysis, storage, and presentation of speech possible. While visual word recognition, based mainly on evidence from lexical decision and word naming, started first, and continues to be a minor industry in itself, word recognition is now almost as well studied in the auditory as in the visual modality.

Besides the early visual/auditory imbalance, there was another. So dominant was language comprehension over language production as a research topic in early psycholinguistics that it was possible for another of this volume's contributors to maintain in the *Annual Review of Psychology: 'The fundamental problem in psycholinguistics is simple to formulate: what happens when we understand sentences?* (Johnson-Laird 1974). The reasons for the dominance of comprehension studies were obvious: control over the conditions in which an experiment is conducted is paramount, and control over stimuli presented for comprehension is trivially easy to achieve whereas control over spontaneous language production seems at first glance nigh on impossible. But some concentrated efforts, especially by MPI founder Levelt and colleagues (Bock 1995; Bock and Levelt 1994; Levelt 1992), produced new techniques for studying the production of words, phrases, and sentences. Research on production is now competitive with research on comprehension.

Back with technology, another revolution was brought about by computer-readable vocabularies and large language corpora. They provided a reality test for models of spoken-word recognition and sentence processing. Models of word recognition that were modality independent (e.g. Morton 1969) had been joined, in the late 1970s, by models that attempted to capture the temporal nature of spoken-word processing in particular (Cole and Jakimik 1978; Marslen-Wilson and Welsh 1978). These models saw speech understanding as a sequence of word recognition acts; as soon as one word was identified, it would allow the beginning of the next word to be located for its processing to begin. However, the availability of the electronic dictionaries, from the

mid-1980s, pulled the rug out completely from under this view (Cutler and Carter 1987; Luce 1986; Pisoni *et al.* 1985). Vocabulary analyses showed that words in speech were hardly ever unique objects. Thus *recognition* begins with *wreck* followed by *a;* how would a simple sequential processor know *not* to recognize *wreck?*

Technology provided the way out of this problem too. The programming techniques developed in engineering and mathematics—in particular, connectionist modelling—altered the type of modelling undertaken in all cognitive psychology, psycholinguistics included. The growing knowledge of vocabulary statistics called for new models that could simultaneously entertain the multiple possibilities that speech turned out to consist of, and—right on cue—connectionism provided them (e.g. McClelland and Elman 1986). For the past 20 years, all models of spoken-word recognition have allowed for concurrent activation of multiple word candidates, with some form of competition resolving the eventual selection (Gaskell and Marslen-Wilson 1997; McClelland and Elman 1986; Norris 1994). In such models, *recognition, wreck, a,* and many more fully and partially supported candidates *(rest, wrecker,* etc.) could all be evaluated simultaneously, and support for any one of them could automatically modulate the support received by the others without the need for intervention by a separate decision process. The past two decades in spoken-word recognition research have been unusually harmonious as a result of the agreement on this fundamental architecture (though not on the flow of information within it; see Norris *et al.* 2000, and its 31 largely dissenting companion commentaries).

This harmony may disappear as new types of model challenge the currently accepted structure (e.g. Norris and McQueen, submitted). In addition, today's main technological driving force, in psycholinguistics as in all areas of cognitive psychology, is supplied by the techniques of neuroscience. Cognitive neuroscience methods are currently being embraced by psycholinguists, as by researchers in all other branches of cognitive psychology, and imaging evidence is almost as desirable in linguistics as in psychology (though the first linguistic model based on such evidence is yet to be seen). As yet, neuroscience has had no effect on the structure of psycholinguistic models. Alas, the reverse is also true; I am probably not the only contributor to this book to hope that the structure of psychological models will come to have greater influence on cognitive neuroscience research in the future.

The universal substrate

The biggest change *within* psycholinguistics concerns the interpretation of the shared basic assumption—we saw it already in the section on history—that

the language-processing system is universal. The most significant fact about language acquisition is that the language a child acquires is the language the environment makes available. The child's specific genetic endowment brings no leanings towards one language rather than another. This suggests that the processing involved in language acquisition—to whatever extent it involves innate specialization for linguistic structure, or exploits general cognitive abilities—is much the same in all humans: universal rather than language specific. By extension, the basic architecture of adult language processing (for instance, the multiple activation and competition of spoken-word recognition described above) should be common to all.

As translated into the experimental practice of the 1970s, this seemed to imply that the characteristics of the language-processing system could be studied in any language to equal effect. Although the acquisition of language-specific structure was obviously an important topic for investigation (e.g. see Slobin 1985), the basic goal was an account of the universal system that dealt with the variable inputs. In consequence, early studies of adult processing were conceived as, in principle, independent of the language in which they happened to be carried out. It was entirely possible for an experiment carried out in one language to be followed up, supported, or countered by an experiment in another language, without any reference being made to whether the difference in language might play a role in the processing being examined. Example 1: the lexical ambiguity effect in phoneme monitoring, established in English (Cairns and Kamerman 1975; Foss 1970; Foss and Jenkins 1973; Swinney and Hakes 1976), but attacked via experiments in French (Mehler *et al.* 1978). Example 2: the debate on 'units of perception', in which experiments were variously carried in English (e.g. Foss and Swinney 1973; Healy and Cutting 1976; Savin and Bever 1970), French (e.g. Segui *et al* 1981), and Portuguese (e.g. Morais *et al.* 1979). The language in which the experiment was done was never referred to as an important factor in any of the cited papers.

Things have changed now, and I was there as it happened. When I graduated there were few people working on spoken language, and not many of the ones working in America had a polyglot background, as I have already described. My PhD topic was the processing of stress, and it doesn't take a very wide acquaintance with other languages to realize that English-like stress is absolutely not a universal phonological feature. So I came to wonder about how to fit my stress findings into a universal framework. While I was wondering, Jacques Mehler and his colleagues put forward a claim (Mehler *et al.* 1981) that speech is segmented for lexical access in terms of syllabic units. My own findings convinced me, however, that segmentation of speech

was based on stress. Moreover, my first reaction on hearing about Mehler's experiment was that it would not work in English. The short version of the story is that indeed it didn't; English listeners not only didn't produce the same results with materials in their native language, they didn't even produce the same results with the original French materials from Mehler's study. Extraordinarily (to us, then), however, French listeners did produce the same syllabically motivated pattern when presented with the English materials.

This suggested that listeners from different language backgrounds command different routines for processing speech, and against the background of psycholinguistic universalism the finding was startling enough to appear in *Nature* (Cutler *et al.* 1983). Clearly it was no longer possible to assume that every part of adult processing should be shared by all language users; French listeners segmented in syllabic units, but English listeners used stress. Presumably, some parts of every listener's processing system might be language specific. But the argument for a universal basis retains its force—all children begin from the same point, so in some sense the system must be universal. The next challenge, therefore, was to seek the underlying universal commonality that is susceptible to language-specific implementation. For stress in English and syllables in French, the common factor is language rhythm. For instance, French and English poetic forms are, respectively, based on syllable patterning and on stress beats.

This universal substrate of language specificity is what I've spent the last quarter of a century working on, one way and another. I am far from the only one, because just as there were others in the 1980s who were looking at the psycholinguistic implications of cross-linguistic variation (e.g. Byrne and Davidson 1985; Werker and Logan 1985), so there have been many since, across a wide spectrum of psycholinguistic approaches, who have wrestled with the reconciliation of the universal and language-specific (Bowerman 1994; Emmorey 2002; Grimshaw 1997; Imai and Gentner 1997; Newmeyer 1998; Thornton *et al.* 1998). The difference with the point where I started is that it would be unthinkable now to counter an experiment in Language A with an experiment in Language B without any mention of the language switch; even more satisfyingly, cross-language comparisons are found in all the psycholinguistic journals. The general 1970s' acceptance of a universal common processor has been replaced in psycholinguistics by widespread recognition that cross-linguistic differences might be the key to understanding the possible variation in, and thus the true universal nature of, the system.

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