ADVANCED PSYCHOLINGUISTICS

A Bressanone Retrospective for Giovanni B. Flores d'Arcais

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avances in psycholinguistics . . .

Preface

More than a quarter century ago, my friend Giovanni B. Flores d'Arcais, 'Ino' for short, made me his ally in a bold initiative. He had put his mind on convening a first major psycholinguistics conference at the European continent, first that is since World War II. More specifically, he intended to invite the 'young Turks' who were then zealously advocating a new partnership between psychology and linguistics and to mix them thoroughly with the cream of continental psycholinguistics. Many of these young Turks Ino and I had met during our postdoc at Harvard's Center for Cognitive Studies (1965-'66), where Jerome Bruner and George Miller had irreversibly changed the scientific conception of mind. But others had been bred in that exceptional, barely European center of excellence, the University of Edinburgh.

Ino skilfully worked his way through fathomless Italian bureaucracy, eventually collecting generous amounts of *liras*, handpicked the gorgeous old Dolomite city of Bressanone as meeting place, contracted the graceful Elefante Hotel, invited his target participants and was pleasantly surprised by sheer universal acceptance. Eventually, the conference took place in July, 1969. My memory is undecided on whether Ino attended more to the culinary or the scientific well-being of his guests, but surely, he did plenty of both. The conference was exciting, the moods were high, and a network of contacts was forged that survives till the present day.

Though duly satisfied, Ino tirelessly set out to edit the proceedings, again asking me to join him in the effort. And this was really editing! The book was based on the empirical psycholinguistic conference papers, but it got a logical structure of its own, covering the (then) major areas in psycholinguistics. Additional papers were invited where the conference had left noticeable gaps, and we wrote lengthy explanatory texts to increase the coherence of the book. It worked: *Advances in psycholinguistics* appeared in 1970 and was so well received that it had a second printing in 1974.

In retrospect, Bressanone has become a landmark in the history of psycholinguistics, and European psycholinguistics in particular. But who cares about the history of psycholinguistics? There is no written record of what happened to our discipline since the years of the 'cognitive revolution'. Most of our students and younger colleagues are unaware of the battle that was fought to establish cognitive science and about the pioneering role psycholinguistics played in it. Not so Ino. When the 25th anniversary of the Bressanone conference was approaching, he took the initiative to reconvene the meeting in order to consider what had been achieved (and lost) since these early days. Whether it was due to the decline of the Mafia, the cleansing of Ino's ethics by many years of Dutch Calvinism, or the financial suction force of the European Union, the Italian funding agencies approached by Ino didn't give in, and only let him know at a late, too late moment.

Lacking a Bressanone retrospective conference, this book is a second best solution. When Ino's sixtieth birthday (October 3, 1996) came into the offing, I approached all surviving authors of *Advances* with the request to write a short, retrospective paper about how psycholinguistics, and in particular their own work, had evolved since *Advances*. "Back to the future", as John Marshall put it. I neither offered elegant lodging nor culinary reward, still to my pleasant surprise my request too met with sheer universal acceptance. The result is a book, not by young Turks, but by established scientists, many of them main players in the recent history of psycholinguistics. Together, we are offering Ino a *petite histoire* of our field, for whatever it is worth.

This solution is second best, because our special community has not been able to *discuss* the course of a quarter century of psycholinguistics. And indeed, many of the observations surfacing in this book call for much deeper consideration. The ever-going push and pull between psychology, linguistics and the neurosciences is not fully grasped by anyone, but steadily affecting all of our work in unpredictable ways. A better sense of history would not be a luxury for psycholinguists.

The solution is only second best for another reason as well. It is that one major player did *not* contribute: Giovanni Flores d'Arcais. Not only would the editing have been so

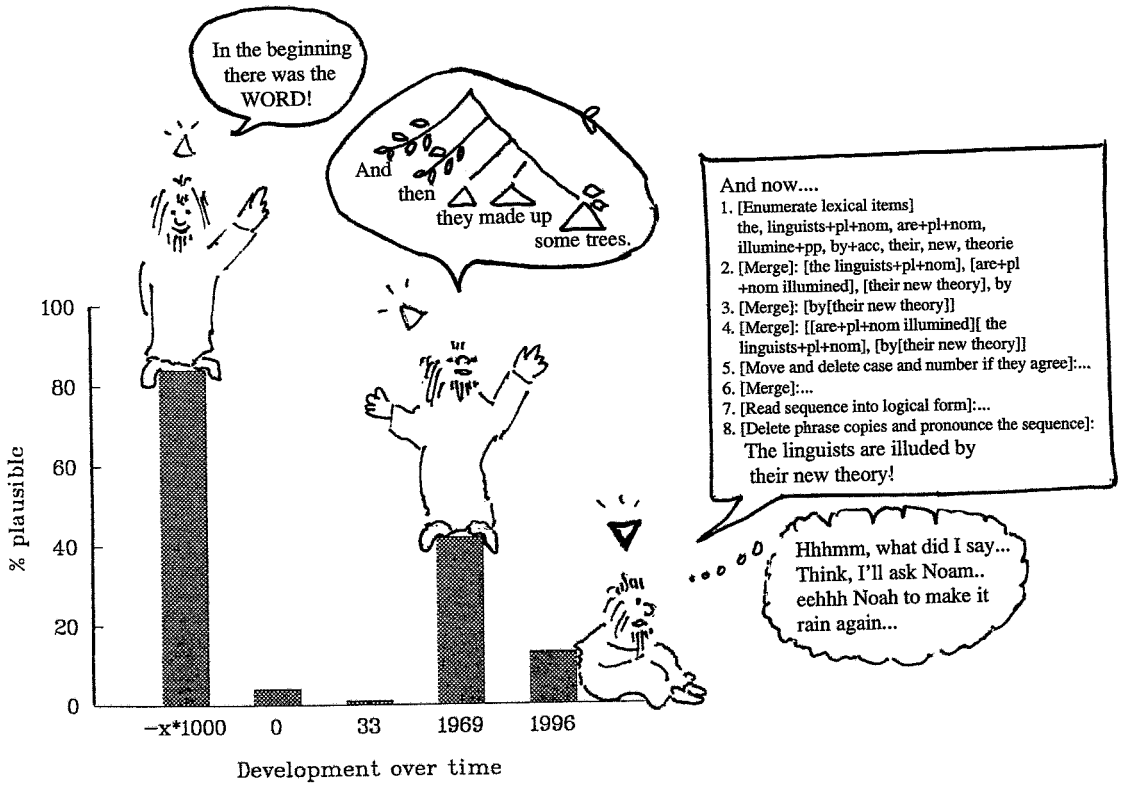
much more imaginative (and more pleasant) if he and I had done it jointly, but also the book would have included a much needed retrospective chapter by the 'Urheber' himself. What would Ino have written about? His own chapter in *Advances* was about the processing of comparative sentences. He reported on remarkable differences in the processing of *more .. than* and *less ... than* constructions and adduced the difference to what he called "the focus of comparison". In a sentence like *A cat is more friendly than a dog*, the subject (*A cat*) is the focus of comparison, whereas in the sentence *A cat is less friendly than a dog* this is not the case, *a dog* being the focus there. The grammatical subject as preferred 'focuser' has been a continuing theme in Ino's subsequent work on sentence understanding and picture verification. Ino would, no doubt, have referred with satisfaction to the recent paper by Lila Gleitman et al. (*Cognition,* 1996) where exactly the same mechanism, the foregrounding effect of the subject, is invoked to explain the apparently asymmetrical interpretation of symmetric predicates, such as *is similar to*. Or would Ino have written about any of the other themes in his rich repertoire: sentence parsing, idiom comprehension, object naming and event description, word recognition, the acquisition of function words and connectives, or about his pioneering work in the reading of kanji and Chinese characters? We'll ask him in due time.

Returning, finally, to Bressanone's future, one should ask, How come that the young crowd over there was destined to leadership in late 20th century psycholinguistics and cognitive science? There are, at least, two possible answers. Maybe Ino, in his clairvoyance, made just the appropriate choice of contributors. Or the meeting plus the writing of *Advances* worked as a latter-day Pentecost, sparking our vocation to study the world's tongues and their use. Either way, Ino did the right thing.

Nijmegen, August 1996 Willem J.M. Levelt

PSYCHOLINGUISTICS IN PERSPECTIVE

Psycholinguistic Theories in Perspective:
God's (1996) plausibility assumption
(reprinted with permission)



1969 - The renaissance of psycholinguistics

Jacques Mehler

The ominous atmosphere of 1968 had not yet been entirely dissipated when Ino Flores d'Arcais and Pim Levelt decided to hold a Conference to examine the recent *Advances in psycholinguistics*. Great changes had rejuvenated this discipline in the USA, and to a lesser extent, in Great Britain. Ino and Pim were not going to allow their European colleagues to ignore these changes. Both behaved as if changing the way science was practiced was a more secure way of ensuring progress than agitation in the streets or debating in endless meetings about how to implement social change – something that many of us, at the time, had failed to remark on. Granted, there were some positive sides to the rowdiness. But those are for a different chapter and a different occasion. At this juncture my writing is motivated by the superb accomplishments of my friend and colleague Ino.

At the beginning of the twentieth century, linguistics and Wundt's Sprachpsychologie had both made great progress and obtained widespread recognition in Europe. Unfortunately, that early success was thwarted when the continent was overrun by the many totalitarian adventures that came about simultaneously in several countries. Who would have predicted that the Brechtian beast would hinder rather than remain indifferent to intellectual ventures. But the Beast was aware that dominating people's thinking about language(s) and psychology is politically very important. It is not by chance that Stalin wrote a number of essays and a book on language. As Maurice Olender has stated in several of his works, the notion of language is of such importance that scholars over the ages have searched for the language which is spoken in Paradise. Obviously, Hebrew was the best

candidate though it did not go unchallenged. Leibniz cites Jan van Gorp – also known as Gropius Becanus – who viewed the German language, which he calls cimbric, as having a greater claim to the title of the 'Ur'language than Hebrew, even if the citations he uses are actually in Flemish! These arguments over our origins have been at the root of many of the political conflicts that have affected humanity.

Post World War II European prostration allowed many of its intellectuals to hold views that were as far from science as those held by the students of the language of Paradise. Many of them had opinions on areas that spanned the entirety of human affairs!

In contrast with the post-war European continent, some Anglo-Saxon countries made great progress in science and particularly in psychology in the same years. Some of their universities actively engaged in research without forgetting that their mission was to educate eager minds. In our ailing Europe most institutions of higher learning were in poor shape. Teaching was often out of date and professors' outlook more appropriate for defending the old order of thing than for constructing the future. In fact, the basic human attachment to truth had become so dangerous during the first part of the century that it was difficult to institute sudden change. University professors reacted by using their authority and titles as a screen for the dearth of knowledge and their inability to accept truth or criticism. How could one accept openminded discussions with so many skeletons in the closet? It was easier to postulate a system whose main claim was that all truth was relative and that expertise, much like good art, did not exist.

The European continent took its time about discovering the growing new field of cognitive psychology. In the late sixties I was personally more familiar with the French scene than any other and this perhaps makes my comments inapplicable to other parts of the Continent. In Paris, the cognitive revolution was seldom even acknowledged. Sometimes, however, it was not ignored and then it was viewed with both misgivings and animosity. Psycholinguistics would ask what generative grammar was all about and why one should have to pay attention to it at all. Was there anything new in this 'soi disant' revolution or, rather, had not de Saussure already stated what the 'converts' were rediscov-

ering afresh? Why should one pay attention to cognitive psychology and generative grammarians of the New World, when in Europe we had our own ways of studying the mind? The notoriety of scholars like Piaget and Levy-Strauss was often invoked to vindicate insulation within the sealed walls of European academic decline. Ino and Pim rebelled against this state of affairs but they used their own tactics to promote change. I have to confess that I only had a dim understanding of these until much later on. Today I know that their plan was a successful one and for this we should all be greatful.

The Bressanone conference was held in July 1969 at the beautiful Elefante Hotel, one of Europe's oldest. Bressanone, a beautiful city, once the home of Nicolas di Cusa (an exceptional mind who helped change the ways of thinking many centuries back) seemed a fitting choice. The city is perched on the sloping valleys of Alto Adige, a region of considerable linguistic diversity and an interesting spot for psycholinguistics to meet. German and Italian are spoken by most inhabitants. The region, like many other parts of Europe, has mixed feelings about whether citizens have the right to use the languages at hand. For some, since Alto Adige has become Italian, inhabitants should speak Italian and refrain from using German. For others, multilingualism is a more befitting state in the late part of this millenium. Be it as it may, we Europeans are just learning to live in multilingual, multiracial and multicultural settings. Yet, ironically, when we met in Bressanone, we were largely indifferent to these issues. The newly renovated discipline of psycholinguistics functioned almost exclusively in English. Furthermore, it tacitly operated under the assumption that all languages are very similar to English, and that the processing routines discovered when studying speakers of English had to be identical to those used by speakers of other languages – if, of course, one ever found the time to do something as odious as to look at speakers of other languages!

Without me knowing it, the choice of Bressanone served as a reminder that psycholinguists maintain an open mind and refrain from assuming that speakers of all languages use identical production strategies. Likewise, although no entry in the book acknowledges the fact that some speakers, e.g., the Tyroleans, are bilingual or even multilingual, this is an

omission that psycholinguistics is currently in the process of correcting. In fact, with the wisdom of hindsight, it is easy to understand why we ignored bilingualism. Indeed, if the same processes were used to cope with all languages, these processes could certainly handle two languages for the same speaker without much adjustment. Things were to change over the nex two decades.

The conference was an opportunity for many European psychologists (a number of whom had already met at the Center for Cognitive Studies at Harvard University where the cognitive revolution had first been sighted), to get together and contrast their views with those of their American colleagues. The outcome of the encounter was an awareness that there was no way in which Europeans could continue to neglect the changes in both psychology and linguistics. I still do not know whether Ino and Pim were in the grips of a Darwinian process that made them select good participants to attend this conference or whether they were more Lamarckian and trusted that the conference would have such a positive effect on the participants that they would become the leading representatives of psycholinguistics simply from being exposed to the new trends. Be it as it may, new trends there were. And that is what I would like to focus on in the last part of this short note in honor of Ino and his considerable efforts to improve our field.

If we look at the structure of *Advances in psycholinguistics* and if we remember how the conference was organized, it is obvious that the editors were fully aware of the great importance of language acquisition. Every section of the book contains a developmental chapter. What is striking today is the age of the children that were the object of these chapters. Not one deals with children under the age of one. At the time of the conference, Eimas et al. were already informing the world that infants only a few weeks old were capable of discriminating phonemes and were behaving more like phonologists than like naive language users. Today, the systematic exploration of neonate dispositions of language has grown so much that the *Advances* volume could not leave out this domain, or at any rate one hopes that this would not be the case. Nor could one ignore the area of brain imaging and its contribution to the study of language. Ino and Pim included a section on the relation of neurology and language.

The two chapters contained in this section are highly informative. Of course, twenty five years ago knowledge came from the observation of patients rather than from a study of healthy subjects accomplishing a task in one of the brain imaging environments. Had such work been available, I am sure that Ino and Pim would have included it. Even today there are treatises that ignore the contribution of these studies. Indeed, in the excellent recent *Invitation to cognitive science*, the survey of the cortical structures related to language does not even mention one of the many discoveries made possible by the use of brain imaging techniques even though they have been published in the best journals. Fortunately, this is an infrequent phenomenon.

Possibly, the most surprising omission from *Advances in psycholinguistics* is the area of speech perception and production. The index does not even contain a single entry for speech, for perception or for production. This is all the more surprising since it is precisely in these areas that the editors of the book and many of the participants would eventually make their greatest contribution.

Psycholinguistics certainly began to change at the turn of the sixties and I have no doubt that Pim and Ino are prominent among those who helped the change of scene. It is true that we look back at those bygone days with nostalgia, but there is also a sense of non negligible accomplishment ... Ino has probably been the one of us who has travelled the furthest to change the scope of psycholinguistics. He is as well known and respected in Japan as he is in the rest of the world because he was one of the first of us to show some interest in the study of distant languages different from his own. Furthermore he was unarguably very instrumental in promoting the study of speech production in Nijmegen. Today, speech production is no longer a poor cousin to the rest of the discipline. Indeed, we are all a bit richer today in the knowledge we have gained both of the domain of production and of that of perception. Thus, at times it does not seem such a bad thing to have grown a bit older. Indeed, Ino has made growing both more fruitful and more interesting for all of us.

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Experimental psycholinguistics: Then, now and thence

Thomas G. Bever

Introduction

In 1968, experimental psycholinguistics faced two questions:
- What is the relation between syntax and linguistic knowledge?
- How does language behavior work?

Construct Validity of Linguistic Knowledge

Syntactic Structures (Chomsky, 1957) launched these questions; each sentence has a computational 'derivation':
a. A 'kernel' structure
   a verb with its arguments, defined by a phrase-structure hierarchy.
b. Transformations
   i) that change the kernel structures, primarily by movement;
   ii) that 'merge' one kernel structure into another.

If such unobservable mechanisms are psychologically 'real', then behaviorism is empirically inadequate for language: thus, the first challenge for psycholinguistics was to demonstrate the behavioral relevance of hierarchical phrase structure, underlying structures, and transformations. Example demonstrations:

Surface phrase structure:
Garrett (1965) and Fodor and Bever (1965) showed that clicks are reported as having occurred between phrase structure units.

Underlying Structure:
Blumenthal (1967) showed greater improvement above free recall for sentences, when prompted by a word (*stove*) which appeared twice (1b) rather than once (1a) in their syntactic derivation:

(1a) *The campers were warm by the stove* (a kernel structure)
(1b) *The campers were warmed by the stove* (derived from an underlying kernel structure: 'the stove warmed the campers')

Transformations:
The construct validity of transformations did not fare as well. At first, the derivational theory of complexity (DTC) was confirmed: more transformations correctly predicted poorer memory (Mehler, 1965). But subsequent careful study showed that DTC is not correct (Fodor & Garrett, 1968; Bever, 1970).

Aspects of the Theory of Syntax – The Super P-marker. To the continual chagrin of psycholinguists, grammatical theory changes. The next model, *Aspects* (Chomsky, 1965), replaced 'merging' transformations with a recursive phrase structure rule:

NP ⇏ S

There was support for the construct validity of the complex deep structures produced in this model. Bever et al. (1969) showed that clicks are reported as occurring between deep structure kernel expansions of *S*, for example, at the points marked # below:

(2a) *The general....defied the troops # to fight*
(2b) *The general....desired # the troops to fight*

Government and Binding – NPtrace. The grammatical theory evolved. In Government and Binding Theory, a set of universal constraints restricted possible transformations and derivations: e.g., the projection principle requires that every argument position of a verb be filled at all levels of represen-

tation. Case theory requires that every overt noun be case-marked. Such constraints force movement. For example, the deep structure of passive sentences looks like (3a).

(3a) *NP be warm+pp the camper+pl by the stove*

the camper must move to the empty NP-position to be assigned case and to fill the empty NP:

(3b) *the camper+pl be warm+pp by the stove*

But now the object position of *warm* is empty, violating the projection principle. The solution is that the movement leaves a 'trace' behind:

(3c) *(the camper+pl)-i be warm+pp (NP-i) by the stove*

Once defined, traces can play a causal role in constraints on derivations. Another empty category, PRO, is coindexed with an antecedent (5), but base-generated without movement, as in (4a,b).

(4a) *NP was likely the tall boy to leave* ⇨
(4b) *(The tall boy)-i was likely (NP-i) to leave*

(5) *(The tall boy)-i was eager (PRO-i) to leave*

The existence of NPtraces affords a strong empirical test of the syntactic theory that distinguishes them. Bever and McElree (1988), MacDonald (1989) and Bever et al. (1989) showed that NPtrace primes recognition of their antecedent: *tall* is recognized as having been in the preceding sentence faster in (6a) and (7a), than in the corresponding sentences without NPtrace.

(6a) *The tall waiter....was attacked (NP-i) by the lawyer....*
(6b) *The tall waiter....was ruined by the lawyer....*
(6c) *The tall waiter....was angry with the lawyer....*
(6d) *The tall waiter....was shouting at the lawyer*

(7a) *The tall waiter....was likely (NP-i) to help the customers....*
(7b) *The tall waiter....was eager (PRO-i) to help the customers....*
(7c) *The tall waiter....was nice to all the customers....*

Sanz and Bever (in press) showed that initial subjects of Spanish unaccusative verbs (8a) prime recognition (e.g., of *apuesto*) more strongly than corresponding unergatives (8b). Distributional evidence requires that the apparent subject of Spanish unaccusatives is actually raised from object position, leaving behind a trace. English does not have strong motivation for this, and Sanz and Bever find no priming difference in the two kinds of verbs in English.

(8a) *El apuesto arbitro....llego (NP-i) con cuidado*
(8b) *El apuesto arbitro....hablo con cuidado*

(9a) *The handsome umpire....arrived with care*
(9b) *The handsome umpire....spoke with care*

The preceding results converge on a demonstration that NPtrace uniquely primes its antecedent, compared with corresponding non-trace positions and PRO. The fact that Spanish but not English unaccusative acts in this way, serves as an important control on discourse and semantic factors; GB, as a theory that distinguishes NPtrace, is empirically supported.

The grammar – today. The minimalist program – back again? As always, however, syntactic theory (r)evolves. Syntactic operations and elements became increasingly abstract in GB leading to the 'minimalist program'. Words – bundles of syntactic, semantic and phonological features – are combined into phrases, then phrases are 'merged' into higher-order hierarchies. At each point, movement occurs to bring syntactically relevant features structurally close enough to check their agreement. There is no specific level of deep structure, but the system remains derivational. For example, the formation of a passive sentence might include the following stages (a-h):

(a) Enumerate lexical items:
the, campers+pl+nom, are+pl+nom, warm+pp, by+acc, the, stove+sg+acc
(b) Merge:
(the campers+pl+nom), (are+pl+nom warmed), (the stove), by
(c) Merge:
by (the stove))
(d) Merge:
((are+pl+nom warmed)(the campers+pl+nom), (by (the stove))
(e) Move and delete number and case if they agree:
the campers are warmed (the campers+pl+nom), (by (the stove))
(f) Merge:
((the campers)((are warmed))(the campers+pl+nom) (by (the stove)))
(g) Read sequence into logical form
(h) Delete phrase copies, and pronounce the sequence:
'the campers are warmed by the stove'

This syntactic model offers several features which are consistent with psychological considerations:
– NPtraces are exact copies. This explains why NPtrace primes its antecedent, but PRO does not: only NPtrace constructions contain a doubled representation of the antecedent.
– There is no specific level of deep structure. The fact that unaccusative sentences prime their subjects in Spanish, but not English, shows that people do not follow the Universal Thematic Assignment Hypothesis (UTAH), that universally matches conceptual thematic roles to the same deep structure configurations. But, without UTAH to constrain deep/surface structure pairs, language acquisition of a GB model would be virtually impossible. The fact that UTAH is false is no impediment to MP-based acquisition.
– The model is framed as the most efficient: 'minimal' mapping between conceptual and motor/sensation systems. This re-unites syntactic and psychological research programs.

The psychological reality of syntax – reprise. Through the changes in generative syntax over 40 years, one feature has remained: Sentences have phrases and derivations that move

them. The experiments I have sketched are consistent with this principle.

How does language behavior work?

There are two enduring facts about actual behavior:
- Most of the time what we do, is what we do most of the time
- A bit of the time we do something new

That is, behavior is habit-driven, with an occasional novelty. Two observations describe how different kinds of knowledge influence each other during behavior:
- Everything influences everything else
- Except when it doesn't

That is, behavior is a compound of potentially modular processes. The role of habits and the extent of modularity have been the driving issues in theories of sentence comprehension. We can use the complexity of reduced relative sentences as a touchstone.

"The horse raced past the barn fell" – a stable phenomenon: Analysis by Synthesis and Perceptual Strategies. Analysis-by-synthesis models can naturally embed a derivational syntax within a comprehension system: such models depend on a rich superficial 'pre-analysis', to limit candidate derivations. Forster (1971) noted that semantic constraints between words can provide such limits. Bever (1970) sketched additional phrase categorical 'perceptual strategies' and suggested that such semantic and syntactic strategies can together account for most comprehension, assigning deep structure to sentences, based on surface cues (Bever, 1970; Fodor et al., 1974). E.g.,

'noun - verb - noun' = 'agent - action - object'.

This accounts for the garden path in reduced relative sentences: the initial NVN sequence is so salient *(the horse raced past the barn)*, that it is unavoidable.

Parsing principles. There were two weaknesses in the cue-based perceptual strategies model:
- Some sentences require the grammar when the strategies fail.
- There was no way to discover cues beyond the obvious ones.

Kimball (1976) and Frazier (1979) proposed actual parsebuilding principles which are filtered by syntactic knowledge. For example, 'minimal attachment' binds a new phrase to an already established node, when possible. This principle explains the garden path in reduced relative clauses; the correct parse requires postulating new nodes dominated by the head noun, while the garden-path main clause interpretation does not.

Modularity or not. Parsing principles apply as 'modules'. Research showing that semantics and context affects parsing has challenged this claim. For example, Trueswell et al. (1994) have shown that the semantic acceptability of the first noun as subject of the following verb affects the garden path in reduced relatives: (10a) leads to less garden pathing than (10b).

(10a) *The evidence examined by the judge....*
(10b) *The lawyer examined by the judge....*

However, the semantic effects appear only after the verb, and thus may presuppose an assigned parse. Indeed, it is hard to disprove modularity claims for two reasons:
- The metalanguages of syntax and semantics are distinct: thus, there is informational modularity between syntax andsemantics, independent of any architectural segregation (Townsend & Bever, 1982).
- Apparent mingling of information types may occur outside the modules.

Lexical based strategies. The best argument against a modular parse-builder is an undifferentiated model that works better. MacDonald et al. (1994) propose that each lexical item

carries its potential syntactic and semantic frameworks. Such a model explains the difficulty of reduced relatives as due to particular lexical probabilities, e.g., *raced* likely past verb, *horse* likely agent, etcetera. This model is consistent with the perceptual strategies model: It is a natural outgrowth of the lexical connectionist architecture for category-based perceptual strategies to emerge.

Habits and modules - Analysis by synthesis rehabilitated.
Probabilistic information guides sentence processing: yet, syntactic representations are ultimately assigned, sometimes in violation of probabilities. This is consistent with a 2-stage comprehension model (Townsend & Bever, 1982; forthcoming):

Lexical and category cue-based strategies assign
a triple: i) lexical sequence
ii) candidate phrase segmentation
iii) candidate conceptual interpretation

Syntactic derivations consistent with the triple are reconstituted and assigned.

Connectionist calculi will facilitate study of hypotheses about valid cues used in the first stage. MP syntax offers a natural model for assignment of derivations in the second stage, since it starts with lexical items and pairs their sequence with a conceptual interpretation.

Conclusion - Plus ça change....?

It is three decades since d'Arcais organized the Bressanone conference: the original questions remain as research guides. What is the construct validity of syntax? Sentences have derivations. How does behavior work? It interrelates structural and probabilistic factors.

So, Ino, avanti!

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Psycholinguistics at the end of the century

Ida Kurcz

Psycholinguistics, if we count its age from 1951 (the year when the term was officially introduced within the scientific community), will be almost 50 years old at the turn of the century – quite an elderly lady in comparison with the time when we gathered for the Bressanone Conference in 1969. At that time she was under 20 – a very active teenager. We wrote *Advances in psycholinguistics*, following which many other readers, manuals, and conference proceedings with 'psycholinguistics' in their titles have made their appearance. Now it is time to write *Post-Advances*. What have been the main tendencies in its development? How far have we gone since then? What is still relevant from our previous discussions? My point of view is that there are several rather quite specialized domains (like speech perception and speech production) where psycholinguistics has deepened and broadened our knowledge, but at the same time, in the theoretical development of psychological thinking, it has lost its revolutionary and provocative character.

As for my own role, I have attempted over these years to introduce psycholinguistics into the Polish psychological literature, trying to keep track of the main trends in this domain in the United States, Great Britain and France and conducting a few experimental studies, some of them of a crosslinguistic character (cf. comparison of reading strategies in Polish and English – Danks & Kurcz, 1984). So my position is that of an outsider who has a different perspective than those directly involved in the field and who is to some extent unable to enter deeply into all the nuances and details. Nevertheless, I would like to present briefly what I see as important in the development of psycholinguistics.

The aim of early psycholinguistics, as formulated by its founders, was to study the psychological reality of linguistic and other theoretical models of language functioning. The

first failure of this endeavour occurred in the fifties when the psychological reality of information theory was being verified. Then the Chomskyan revolution entered the scene evoking great enthusiasm connected with the total, even emphatic, rejection of behaviorism (the typical convert's reaction). I think that most of the Bressanone conference participants were somehow involved in this revolution and in a new way of thinking which resulted in the emergence of the new school in psychology, i.e. cognitive psychology. And here lies the main and unquestionable merit of psycholinguistics as precursor to this new paradigm in psychology. We *Bressanonians* were aware of the new paradigm and full of zeal to create a solid basis for this 'new science'. Studying the psychological reality of Chomsky's theory of generative and transformational grammar became the *en vogue* aim of that time.

Some of us (including myself with my paper in *Advances in psycholinguistics*) were engaged in this endeavour under the heading of the derivational complexity hypothesis. The other hot topic of that time was the interpretation of ambiguous sentences (when does it occur? Is only one of their meanings evoked, or are all of them? In parallel or one after another?). The controversy around the latter topic has had a long life in psycholinguistic papers, and recently some resolution may be found in the application of the dynamical systems conception (Kelso, 1995). The derivational complexity hypothesis in its strong version has failed, because the linguistic proposals for the universal rules have changed several times over the years (in the direction of their reduction) and the implied corresponding psychological operations could not be demonstrated. The idea of studying the psychological reality of any theory has come into question (Chomsky himself has rejected the validity of the conception of psychological reality, as it was used by experimental psycholinguists, cf. Rieber, 1983).

Psycholinguistics, having lost its principal aim, has remained simply the study of people's linguistic functioning in its representational and communicative forms. Its special status from the beginning of the sixties has dissolved in the seventies and eighties in the more general approaches of cognitive psychology and cognitive science. If we use the metaphor of a train, psycholinguistics may be viewed as the

locomotive that made cognitive science move, but then lost control over its further progress.

What has psycholinguistics contributed to the study of these two basic forms of linguistic functioning – representational and communicative?

The representational functions of language have been studied over the decades in relation to other psychological processes like memory, perception, problem solving and so on. The issue has a long history under the heading of 'language and thought' or 'language and cognition'. In the fifties, psycholinguistics took on the issue as the linguistic relativity hypothesis formulated by Whorf a decade earlier (the language we speak influences the way we think and perceive reality). A number of experiments, among them the codability of colors in different languages, were carried out. The results seem clear in the rather obvious sense that the perception of color is independent of the language spoken by the particular group of subjects. But some tasks, like sorting according to color names, are dependent on the color names existing in the given language. The interest in linguistic relativity (which still remains as a political problem, stated so clearly in Orwell's *1984* as *newspeak*) dropped off in psycholinguistics over the sixties when interest in universal grammar was developing. The whole idea of Whorf's linguistic relativity hypothesis has been even ridiculed by Pinker in his *The language instinct* (1994). Nevertheless, the problem keeps coming back under different guises. Let me reflect a moment on this.

Language and thought have been viewed for ages as closely related phenomena, as two sides of one coin. The statement that a given language precludes a given way of thinking had became a very common opinion. However, it is unwarranted. The differentiation of languages and the differentiation of thinking may each have specific and independent causes. Psycholinguists, influenced by the theory of universal grammar and the modular approach to the study of mind, have rejected the above opinion by denying its premise. They do not see any close connection between language and thought or language and cognition. Language is a specific module, organ, or faculty of the human mind and so is thinking, or rather the several intellec-

tual faculties which we call thinking. There are, of course, relations between these different faculties and the language faculty, but they are realized on a higher level of mental functioning, beyond the modular level. Psycholinguists have been particularly interested in studying the specific characteristics of the language faculty which have been viewed as the biological endowment of a universal grammar, without paying attention to any other faculty. They speculate, however, that other mental faculties, including a social one, might have their own specific characteristics on the modular level.

I must admit that I am sympathetic to this way of theorizing about universal grammar and its origin in *homo sapiens* as an evolutionary event – as conceived by Bickerton (1990) or Pinker and Bloom (1992). This universal grammar imposes specific constraints on the development of human languages, but nevertheless permits their differentiation. However, I would like to argue that some differences go beyond the scope of universal constraints and at this point there is a basis for the persistency of the linguistic relativity hypothesis.

First of all, those constraints concern the syntactic component of linguistic systems but not their vocabularies (lexica). Second, the communicative function of language also imposes specific requirements on language use.

The relative independence of the lexicon from the grammatical rules can be traced back to the notion of protolanguage (Bickerton, 1990), which is a sort of vocabulary without grammar (syntax). All languages are equally complex from the syntactic point of view. There is no better or higher grammatical system among languages. But there are quantitative and consequently qualitative differences in their vocabularies. If the vocabulary of any science, technology, or religion is created in one language, it can be eventually transferred to another one just by borrowing those terms. This is the typical case for minority immigrant languages in Western Europe or in the USA and Canada. The lack of scientific vocabulary in many African and Asian languages has become a political and educational problem for many postcolonial states where education on the secondary school level still has to be provided in some European language (Tucker, 1991). Psycholinguists, equipped with a theory of

universal grammar, cannot simply ignore the existing problems for the reason that the theory does not predict them.

Let us now turn to the communicative forms of language functioning. The communicative function of language, treated over the ages as its main function, might be derivative of its representational function (see Bickerton, 1990). Nevertheless, the communicative or pragmatic aspects of linguistic functioning cannot be neglected by psycholinguistics. But our *Advances in psycholinguistics* is evidence of this neglect.

As a reaction to this lack of interest, the notion of communicative competence was introduced by sociolinguists (Hymes, 1972), and a whole series of studies on speech acts (direct and indirect) and conversational or discourse rules have been developed since (for a review see Clark, 1985). The notion of discourse and discourse analysis entered also into the psycholinguistic approaches (the usefulness of these notions for developmental psycholinguistics has been shown in 1995 by Grace W. Shugar in her studies of Polish children).

What I would like to stress here is that by studying these communicative devices we can come back to the problem of the relation between language and thought. Let us return to Orwell's notion of *newspeak*. What does not exist in language cannot exist in thought. The detailed analysis of censored texts during the communist era in Poland (Głowiński, 1990) shows how strong this belief was among the policy makers of those times. But the phenomenon is much broader and, in fact, is an enduring one. With the spread of human rights policies in the USA the overt confession of racism, antisemitism, or ethnocentrism has ceased to be the typical response, at least of a conformist. Social psychologists have therefore been forced to devise new scales for measuring racism, etc., which reveal the hidden forms of these attitudes (the so-called 'modern racism' or 'symbolic racism' scales, cf. McConahay, 1986). Discourse analysis has turned out to be an effective method of discovering those hidden relations between language and belief. An example is provided by several works on discourse analysis by Teun van Dijk (1993) who studied the strategies used by his Dutch respondents to describe their attitudes toward minorities living in Holland. The main strategy was YES, BUT (Yes, all people have the

same rights, but those people slaughtered a lamb in their bathroom).

To sum up, I have attempted to draw attention to the psycholinguistic problem of the relation between thought and language - a problem that remains unresolved and one that keeps reappearing to trouble us.

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Bressanone and after: A short, yet productive period

Bénédicte de Boysson-Bardies

Wer nicht von dreitausend Jahren
Sich weiß Rechenschaft zu geben,
Bleibt im Dunkeln unerfahren,
Mag von Tag zu Tage leben.
(J.W. Goethe)

One should more often ask psycholinguists to consider their past! Encountering again the spirit of the Bressanone conference after twenty-five years allows one to experience the ambitions of 'young' psycholinguistics, at the time inspired by the new developments in linguistics. Though certain illusions have disappeared since then, the Bressanone conference did contain the seeds of the future developments. A first observation stands out: the diversification of the theoretical and methodological domain in psycholinguistics makes it quite improbable today to have a conference that, with such a small number of participants – thirtythree – could cover a research area ranging from 'psychological aspects' of grammar to the study of inference rules as constituent of the language capacity, in passing covering the pathology and acquisition of language as well as phonetics.

In Bressanone, psycholinguistics was still spoonfed by linguistic models. The forms that verbal realizations should take were those given by linguistic analyses. But the transition from an operation to a result belonged to the domain of psycholinguistics. By resolving to study the 'psychological reality' of linguistic models, psycholinguists thus had to abandon the notion of a compatibility between phenomena observed in behavior and the hypotheses engendered by the abstract models proposed by the linguist. In Bressanone, the psycholinguists examined the incompatibilities between 'performances' and certain predictions from linguistic models. Two points in particular announced the psycholinguistic developments in the seventies and eighties: The

interactions with perceptual systems and the role of the lexicon in the computation of phrasal meaning.

'Computation' requires the activation of psychological, in particular perceptual systems. But this turns out to be hampered in the face of certain phrases that are still grammatically correct. This has to be accounted for by performance models. The approaches in Bressanone were very diverse. Today we will appeal to 'post-modular' processing for some of them, but others (in particular T. Bever's) address an aspect of automatic speech processing by pointing out that the perceptual and structural systems of language interfere without behavior being accessed from within the linguistic rules. Speech perception began to be approached in a more 'autonomous' way than seems to be warranted. A first line of research by Kimball and the 'parsing'-movement had tried to integrate models of performance and competence.

At the same time, the question of 'semantics' was raised. The generative grammar approach marginalized the meaning that is gauged by syntax. The latter is the medium in which phrasal computation is realized. Still, lexical analyses show interferences between syntax and the structure of words. The integration of a semantic dimension in competence models must proceed through a description of the lexicon that includes intrinsic syntactic and semantic factors. It is interesting to observe that it was a 'pathologist' (J. Marshall) who in Bressanone was one of the most ardent defendants of this position. The contributions of pathology and of neurolinguistics have always been essential, and will increasingly be so in the coming years, not only because of their behavioral analyses, but also for the establishment of the 'psychophysiological' realities that researchers are more and more keen on.

Perceptual organization of processing units, and the role of the lexicon: those two themes evoked in Bressanone contained the seeds for the studies of processing and mental representations (structure of the mental lexicon and lexical access) that would meet with such success in the years to come. But the hope to see the emergence of a general model of comprehension from models of interaction between more specific systems has faded ... substantially! The rapid evolution of insight in brain functioning has strengthened the idea that language functioning is specifically determined by

a brain that is more modularized than one could imagine even a few years ago. The differentiation of more and more specific systems has at the same time raised the question of modelling unificatory systems. As far as the outcomes of comprehension processes are concerned, psychologists must accept to consider them as not linguistic but cognitive.

The study of development has completely changed since Bressanone. In *Advances in psycholinguistics* the developmental contributions are distributed over three main chapters: *grammar, lexicon and meaning,* and *cognition,* such marking their role of merely drawing attention to issues in the study of adults. The paper by Peter Eimas et al. that appeared the year following *Advances in psycholinguistics* would open a totally new area of research, of which neither the orientation nor the onsets were foreboded in the Bressanone efforts. The discovery of infants' predispositions for speech processing as well as the evolution of these capacities under the influence of experience immediately open other areas of reflection. Studies of babies and of adults mutually enrich one another with, for example, the disclosure of precociously available implicit language-specific procedures. In this way the perceptual units and some representations appear to be adapted to specific properties of languages, and it is during the first year that they get specified.

I am afraid that, twenty years from now, I will not again be able to make such a turn to the past. Will the 'law' of cycles restore reflections close to those of Bressanone or will we then have discovered new ways of coping with the challenge posed by the complexity of the systems involved in our capacity for language?

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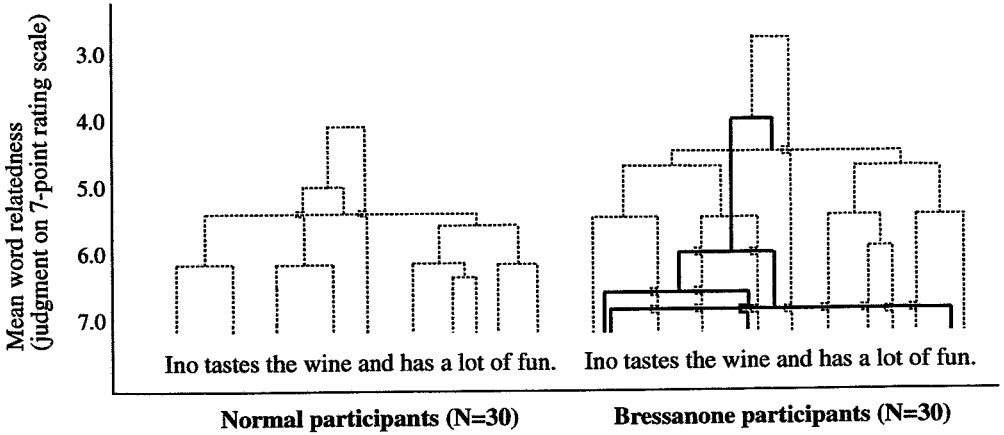
PSYCHOLOGICAL REALITY

Psychological reality
Word relatedness judgments reflect deep insights in...



Linguistic intuitions and beyond

Willem J.M. Levelt

When I wrote my paper for *Advances,* I was fascinated by this 'chapter of psychology' linguists had been writing without asking any of us, psychologists, for permission. In my short career as a psychophysicist/mathematical psychologist I had never come across tree-like representations, but in this chapter they proliferated. What did they represent? Syntactic structures in the mind. And what kind of data were they based on? Linguistic intuitions, grammaticality judgments. The route from judgmental data, such as similarity judgments, to abstract representations was a beaten path for me, but the resulting representations had always been spatial, mostly Euclidian ones (Levelt et al., 1966). Here was a new task for me: collecting syntactic intuitions and developing a mathematical procedure to map them onto syntactic trees. The data I started to collect were subjects' judgments on the degree of syntactic relatedness between the words in a sentence, so-called cohesion data. In the sentence *Ino's birthday is in October* the syntactic relatedness between *Ino's* and *birthday* will be judged to be stronger than the relation between *is* and *in*. This was new stuff (Levelt, 1969), but I missed the boat for the mathematical procedure. Here Steve Johnson (1967) came up with an elegant mapping algorithm that did exactly what was needed.

Would behavioral data on sentence recognition show the same latent hierarchical structure? Yes, when subjects reproduced sentences that were presented in white noise, the conditional probabilities that word *j* was correctly reproduced if word *i* was correctly reproduced mapped nicely onto tree structures. These structures reflected the major constituents of the sentence (Levelt, 1970). Now, clearly, linguists were not waiting for behavioral data of this kind. Till the present day they mostly rely on their own linguistic intuitions. At the time, the tacit assumption was that these intuitions are

an almost direct expression of one's underlying linguistic competence, which after all was the *explanandum* of linguistics. This, I realized, was no more than a convenient illusion. In a simple demonstration experiment I showed that trained linguists were hopelessly confused about the grammaticality of pet examples in their own linguistic literature. I concluded that there is "a complete absence of arguments in the literature in favor of the thesis that linguistic intuitions reveal the underlying linguistic competence" and generously advised linguists on how they could minimize the pitfalls of intuitive judgments (Levelt, 1972). That the major empirical base for linguistics was in deep trouble was recognized by many colleagues (see the marvelous review by Schütze, 1996), among them Tom Bever (1970) in *Advances*, but what should be done about it? Labov (1975) correctly pleaded for the use of primary data, i.e. real utterances, wherever possible. He too gave generous advice about the use of grammaticality judgments, which was often cited but never followed up.

But what if linguists would, by some miraculous development, decide to adhere to normal scientific standards of data acquisition? How could reliable intuitive data bear on their theories? This sounds like laboring an obvious point. To know for sure that *x* is a grammatical sentence and that *y* is not, is the most elementary support for grammar *G* that generates *x* but not *y*. But it is not so obvious. Any grammar *G'* that is weakly equivalent to *G* will be supported by precisely the same data. So, how to distinguish between *G* and *G'*? Grammaticality judgments are to no avail. And I suspect that this is more often than not the theoretical gambit in linguistics. The tension between alternative theories is almost never of the kind "your theory cannot generate sentence *x*, but mine can". Rather, the fight is about structural and explanatory adequacy. What kind of intuitive data would one need in support of a theory's structural adequacy? (I will leave explanatory adequacy to Bressanone reincarnated). Structural intuitions. What are the cohesions we intuit between words or constituents in a sentence? These are just the kind of intuitions I analyzed in my paper for *Advances*. They tell us something about sentence *structure* and at this point different theories come up with quite different solutions. But for cohesion data it is much less

transparent than for grammaticality data how they would bear on a theory or how they would decide between descriptively equivalent but structurally different theories.

This problem I set out to solve in Levelt (1974), Vol. III. Following standard practice in measurement theory, I developed an interpretation theory that could mediate between linguistic theory and observational cohesion data. For a context free grammar, for instance (and leaving details aside), there would be a cohesion function such that the cohesion of a constituent was always smaller than the cohesion of an embedded constituent. Hence, for *Ino's birthday is in October* the constituent *in October* is more cohesive than the constituent *is in October*. I then defined the cohesion between two elements (words, constituents) as the cohesion value of the smallest common constituent. Hence, the cohesion of the pair (*is, in*) is the cohesion value of the constituent *is in October*, whereas the cohesion of the pair (*in, October*) is the cohesion value of the constituent *in October*. Since *in October* is embedded in *is in October*, the pair (*in, October*) should be intuited as more cohesive than the pair (*is, in*). Such order relations could then be experimentally tested. Notice that this goes beyond the application of hierarchical clustering algorithms - in fact they have become superfluous. It turned out that my experimental cohesion data strongly violated a context free grammar. They were on gracious speaking terms with an *Aspects*-type transformational grammar, but most consonant with a transformational grammar with a dependency-type base. This approach, I believe, is still eminently applicable to a comparison of more recent theories, but apart from the excellent doctoral dissertation of Eric Schils (1983) no further work has been reported along these lines.

Throughout these publications I had kept to my claim that linguistic intuiting was a kind of behavior, rather than a clairvoyant window on linguistic competence. And as Bever (1970) remarked, this invites the study of the intuitional process itself. My ultimate flirtation with linguistic intuitions was just that. Various studies had made it likely that in making a grammaticality judgment about a sentence, the subject would imagine a situation in which the sentence could be uttered. The ease of imagining such a situation would (co-)determine the acceptability of the sentence. This

would predict that a high-imagery phrase would be experienced as more grammatical than a structurally equivalent low-imagery phrase. Levelt et al. (1977) showed that to be the case, and strongly so. More important, however, was the question how much of such interpretation was involved in a grammaticality judgment. Real, all-out, full interpretation? To test this, the same materials were used in a paraphrase task, which did require full interpretation. In both tasks reaction times were measured. It turned out that the imagery variable had a substantially stronger effect on paraphrase reaction times than on acceptability judgment reaction times. This shows that grammaticality judgments involve less than full semantic interpretations. But how much less? Nobody knows

I am now reaching my 1500 word limit for this paper, but I am only seven years from Bressanone. What happened during the next two decades? The Max Planck Society provided me with 'a grant for life' and an ever-growing team of brilliant students and colleagues – among them Ino Flores d'Arcais. Linguistic intuitions were still around in our initial work on linguistic awareness in children, but I decided to concentrate on the most enigmatic of all human behavior, speaking. I would have giggled if anyone had predicted that in Bressanone. But have I lost my fascination about that 'chapter of psychology'? No, I haven't. It is still alive and kicking (see Levelt, 1995).

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The psychological reality of an ordering of phonological distinctive features

H. Wilfred Campbell
Cornelis H. van Schooneveld

Introduction

At the Bressanone conference on psycholinguistics (1969), a new phase was introduced concerning the relationship between the role of phonological distinctive features in language description and in speech processing. As is well-known, each phoneme is represented as a bundle of distinctive features with the value + or – ascribed to a given phoneme for each feature (see Jakobson, Fant & Halle, 1952; Jakobson & Halle, 1956; Chomsky & Halle, 1968; Jakobson & Waugh, 1988). Also with respect to speech processing, enough experimental data had been collected on the basis of which a correlation between the description of phonemes in terms of phonological distinctive features and the role of phonological distinctive features in speech processing could be accepted (Miller & Nicely, 1955; Miller, 1956; Wickelgren, 1965 and 1966). The new phase in the discussion was the emphasis on the notion of feature ordering.

In his paper at the Bressanone conference, Campbell presented results based on this analysis of confusion data related to the perception of phonemes, which not only confirmed the above mentioned relationship but also suggested the existence of an hierarchical ordering of distinctive features during speech processing (see Campbell, 1970). However, the feature systems which have been used so far do not provide for a specific feature ordering. Nonetheless, the role of phonological distinctive features in speech processing, accepted already in earlier studies, could then be referred to as 'the psychological reality of phonological distinctive features'. That is to say that the linguistic description of phonemes in terms of phonological distinctive features presents a true reflection of the representation of

the language user's interpretation of the phoneme which is thus seen to take place in terms of distinctive features rather than in terms of directly observable properties of the speech signal.

The ordering of phonological distinctive features

Whenever an attempt is made to consider the psychological reality of a phonological feature system, the problem of the underlying feature ordering should be solved as well. The feature ordering should be an integrated part of the feature system. The possibility of feature ordering was emphasized already in earlier writings of Jakobson (1941). He discussed the question which distinctive features are acquired earlier by the child and which are acquired later as well as which distinctive features are lost earlier and which later in aphasia. Jakobson and Halle (1956) have replaced the classification system of 12 features presented in Jakobson, Fant and Halle by a classification into three sets of features: force (*energy*) features, one propensity/length of time feature (*quantity*) and features characterized by *pitch*. We assume these three sets of features to be ordered: *quantity* implies *energy* and *pitch* has duration (*quantity*) as its prerequisite. Thus the feature ordering assumed is '*energy* < *quantity* < *pitch*', suggesting an inclusion relation among sets of features. Campbell (1974) has compared the feature ordering obtained from experimental data with the one derived from the 'Chomsky-Halle feature system' (Chomsky-Halle, 1968) by using the theory of markedness as a coding strategy to obtain a phonological feature hierarchy (see Postal, 1968). The feature ordering based on the auditory presentation of speech correlated best with the one postulated in the phonological feature description of the phonemes (see table 1).

The major question to be answered therefore is whether a feature system can be found in which an ordering of the separate features can be presented as a basic characteristic of the relationship between these features. In the following an answer to this question is proposed.

Table 1 – *Feature orderings expressing the dominance relation among features for the description/perception of consonants* (from Campbell, 1974, p. 177)

Type of hierarchy	Feature ordening
Phonological hierarchy	*Nasal - voice - coronal - continuant - strident*
Auditory hierarchy	*Nasal - voice - continuant - coronal*
Visual hierarchy	*Coronal - continuant - voice - nasal*
Audiovisual hierarchy I (low noise level)	*Coronal - nasal - voice - continuant*
Audiovisual hierarchy II (high noise level)	*Coronal - continuant - nasal - voice*

Phonological distinctive features and semantic sets

Our proposal assumes that the set of six ordered semantic features of van Schooneveld (1992) which appears to be applicable on the semantic level applies to Jakobsonian phonological distinctive features as well. The basic point of view underlying the assumed one to one correspondence of van Schooneveld's semantic to Jakobson's phonological features is that the distinctive feature does not signal mere otherness of an acoustic segment, but otherness of a specific kind. Consequently, the phonological distinctive feature is both *signifiant* (acoustic signal to indicate otherness) and *signifié* (acoustic signal pointing to the specific otherness of the given feature). Van Schooneveld's semantic features do not describe the referent but rather give instructions as to how the referent is to be identified *(signifié)*. They describe identification techniques, in terms of the very act of identifying the referent. The six features are ordered in such a

way that each succeeding feature is the codification, through generalization, of the instantiation in actual identification acts, of the preceding feature. Each feature is more specific since it implies its predecessor and is hence a subset thereof. Thus, the ordering of the semantic features is one of moving from a larger less specified set to a more specified subset: mathematical inclusion relations hold. This hierarchy of inclusion relationships is autopoietic since it creates itself through its instantiations. The autopoiesis is based on the fact that in an instantiation, the identifier identifies not only the referent, but also the identification event itself (see van Schooneveld, 1992).

The feature system of van Schooneveld (1992) consists of four sets of six features. These sets differ in terms of the identifiers who perform the initial identification of the referent: a feature with single prime (′): any identifier; with double prime (″): obligatorily speaker; triple prime (‴): all identifiers identify simultaneously; quadruple prime (⁗): like triple prime (‴) but in the speech situation. Only features with single prime (′) are discussed here. Table 2 shows a matrix in which the rows represent phonological distinctive features and the columns semantic features (viz., van Schooneveld's semantic theory). For features not discussed here other levels of identifiers appear to apply.

It should be realized that the semantic features as components of phonemes are semantically (functionally) not identical to the abstract semantic generalizations of which they are applications as phonological inherent distinctive features. In the same way, the French preposition *à* is not semantically identical to the French /a/ phoneme, nor is the Russian conjunction *i* (and) identical to the Russian /i/ phoneme. In fact we have used the description of each semantic feature as a separate searchprocedure to look for the acoustic characteristics which are related to a particular distinctive feature, each semantic feature leading to the recognition of one specific distinctive feature.

The distinctive feature *consonantal* is related to the semantic feature *plurality,* The common denominator of the characterizations of consonantalness (low total energy, Jakobson & Halle, 1956; increased damping of sound, Jakobson & Waugh, 1988) is noise. This noise can be tested over and over again, but its characteristics are obviously

different depending on which other distinctive feature it cumulates with in a given phoneme. This reminds one of the semantic feature *plurality'*, which, according to van Schooneveld (1992), concerns a non-limited set of identifications, no matter whether formed by enumeration (an extensional set) or by a property common to its members (an intensional set).

Table 2 – *Matrix of semantic and distinctive features*

	plurality'	*demarca-tedness'*	*enumeration'*	*verification'*	*complement'*	*objectiveness'*
consonantal	x					
vocalic		x				
grave			x			
e/o compact				x		
acute					x	
x/a compact						x

In *vocalicness* we have to do with a sharply defined formant structure (Jakobson & Halle, 1956), accompanied by a longer duration and characterized by a clearcut sonority (Jakobson & Waugh, 1988), distinguishing one phoneme from another. Thus we find in *vocalicness* the semantic generalization of the application of consonantalness (*plurality'*) seen in abstracto, together with the codification of the ability to distinguish one phoneme from another. We find the same kind of distinctness of one referent from its peers in *demarcatedness*, the semantic feature characteristic of intensional sets. The members of such a set have a specific property in common, distinct from a comparable common property characterizing another intensional set, e.g., the

agent of 'to live' or the referent of the object of the preposition *in*. Such a set results autopoietically from the instantiation of a *plurality'* set, which results in the property of having been applied. Tone *(pitch)*, characteristic of the distinctive feature *grave*, individualizes the spectrum just as in morphology the semantic feature of *enumeration*, characteristic of an extensional set, points at the individual meaning carried by a morpheme or as the preposition *behind* is vectorial and requires an individualized referent, whereas such is not the case for the preposition *at*. The feature *grave* relates to a formant structure based on low pitch, which can be considered the fundamental variety like the tonic in musical structure.

Verification' consists of a multiple of identifications of a given identification. Thus, it reidentifies an already identified individualized referent (e.g., *street* in *he went along the street*, where more emphasis is placed on the independent identifiability of the street than *in the street*). We seem to find a phonological parallel in the compactness of /e/ and /o/, whose repetition of diffuseness results in a new, yet similar formant structure. The fact that compactness occurs with both gravity and acuteness on a higher energy level shows that once again we have to do with the codification (generalization) of the application of the preceding feature, in casu tone. We consider diffuseness as a type of signalization of non-A (in casu compactness) (Jakobson, 1988) going back to the non-signalization of A, the zero type of signalization. A comparable relation is found between minimizing (e.g., *short*: zero) and maximizing (e.g., *long*: +plurality) adjectives (van Schooneveld, 1992).

Complement' eliminates an already identified referent and replaces it by its complement. The distinctive feature to be associated with complement is the feature *acute* (the mirror image of gravity). It signals a concentration of energy in the higher frequencies, that is, the complement of a concentration of energy in the lower frequencies (*gravity*) (Jakobson, Fant & Halle, 1952).

The sixth feature is *objectiveness'*, which signalizes a referent potentially at a maximal distance of a point already identified. The distinctive feature characterized by objectiveness is the maximal degree of compactness characteristic of /æ/ and /a/. It is a specific concentration of energy

in the spectrum (Jakobson, Fant & Halle, 1952).

The ordering of the six semantic features is, therefore, *plurality' – demarcatedness' – enumeration' – verification' – complement' – objectiveness'*. We propose as corresponding to Jacobson's first four phonological distinctive features an ordering of six features instead: *consonantal – vocalic – grave – e/o compact – acute – x/a compact.*

Concluding remarks

The above seems to suggest that van Schooneveld's semantic hierarchy of features that appears to be generally applicable to language, is applicable to Jakobson's first four phonological features as well. Space restrictions prohibit us from discussing other phonological features, apparently marked by double and triple primes. To be sure, our present first attempt should be subjected to exhaustive testing.

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THE ACQUISITION OF LANGUAGE

A language acquisition study...

En route to pragmatics

Eve V. Clark

In hindsight, life and research, alike, have a much more orderly appearance than they did at the time. Events may appear to have followed one after the other in an organized fashion, in life and work. Yet at each point along the way, with one's views on a issue still forming, it seemed (and seems) much less clear which path is the one to follow. In this note, I'll try to pick out some parts of the path, in hindsight at least, that led me from the work I reported on at Bressanone to my present position.

In "How children describe events in time", I examined the acquisition of temporal adverbs and connectives in terms of derivational complexity of syntax, order of mention, and choice of theme. Derivational complexity predicted that subordinate clauses embedded after the matrix clause were simpler than those that were preposed; order of mention predicted a preference for descriptions that mirror actual order; and choice of theme limits the options children have for describing a specific sequence. The findings from a longitudinal study of production showed that children first describe temporal sequences with a succession of clauses linked by *and*, or *and then*. Next, they produce clauses introduced by *when*, as in *When you close the door, then I can kick all my blankets off* (D 2;5,3), and only later produce connectives like *before*, *after*, and *while*. They also prefer to describe sequences of events in their actual order: the first clause usually describes the first event, the second clause the second one, and so on. But the event children start with depends on the theme, what has just been mentioned: if it is event-2, then children tend to begin with that and introduce event-1 afterwards, as in *You wear gloves when it's snowy-time* (D 2;6,27, in response to a comment about gloves). Derivational complexity was consistent with the order of acquisition observed in the children's subordinate clauses, but in

general, the theory of derivational complexity failed because it did not take account of meaning-differences among syntactic constructions, and its dependence on a particular theory of syntax led to a rapid demise. Order of mention, based on event- order, governs adult as well as child narratives, and appears to be basic to our organization of events. And the influence of theme in conversational exchanges points to the essentially social context of language acquisition. This 1970 study was the starting point for much of my research since: the gradual emergence of different conjunctions led me to study the acquisition of meaning more generally; the order-of-mention findings suggested that children built on nonlinguistic knowledge when they encountered unfamiliar linguistic material; and children's attentiveness to conversational detail suggested that pragmatics in language use was important to the process of acquisition. In what follows, I'll trace parts of the path I followed to my present position on these questions.

Early meaning acquisition

The findings in Clark (1970) led me first to look more closely at meanings of *before* and *after*, to see what meanings children assigned to them at each stage, and whether these could account for correct uses and errors in comprehension and production (Clark, 1971). Children's acquisition of the conjunction meanings interacted with their reliance on order-of-mention, to produce characteristic errors at each stage as they went from no knowledge of *before* and *after* to adult-like mastery. In pursuing my questions about the kinds of early meanings children assigned to words, I began to read diary studies of children learning a range of languages from the 1890s on: their detailed observations provided an invaluable record of early meaning acquisition in the patterns of word use by one- and two-year-olds. These findings led me to propose the Semantic Feature Hypothesis, SFH (Clark, 1973a). That is, children build up the meanings of words gradually by drawing from a set of universal semantic primitives; they start with general features or components of meaning linked to a word, and later add more specific features. This proposal provoked a

number of studies showing that, within a semantic domain, children acquired words with less complex meanings before those with more complex ones, where complexity could be measured in terms of added components of meaning. This held for terms for space, time, and kinship. Some of these studies are collected in Clark (1979).

But the SFH did not in the long run offer an adequate account of meaning acquisition. Among its weaknesses was the fact that a featural analysis cannot be applied to the lexicon as a whole; analysis of children's early meanings led to the reification of some elements as semantic features, features that played no role in analyses of adult meanings. And the predicted order of general features before specific ones did not hold up. For these reasons and others, I abandoned the SFH in 1983, in favour of a pragmatic approach to meaning acquisition, the theory of contrast, in which inferences about the meanings of unfamiliar forms are based on the child's current repertoire plus systematic inferences about the speaker's intentions in context. Even one-year-olds offer strong evidence that they are sensitive to conventionality and contrast in language use, and rely on them, for instance, to avoid assigning the same meaning to two different forms (Clark, 1987, 1988 and 1990). This approach focusses on speaker-meaning rather than just word-meaning, and locates the process of acquisition within language use.

Non-linguistic strategies

In my studies of meaning acquisition, it also became evident that children rely initially on non-linguistic knowledge about relations, properties, and events, as they respond to instructions. For example, they use order of mention prior to acquiring the meanings of terms like *before* and *after*, and they use a sequence of preferred placements in response to locatives they don't know (e.g., *in* and *on*), with containment taking precedence over support: they put smaller objects into containers whenever possible, and only place objects on supporting surfaces when containment is not an option (Clark, 1973b). They rely on the same strategies in the acquisition of orientational terms like *top*, *bottom*, *front*, *back*,

and *side*. And they look for and choose greater extent on spatial dimensions such as height, length, and width.

Young children's choices of strategy are systematic and may make it appear that they already know the meanings of some words but not others. However, their performance on copying tasks, in response to *Do what I do*, shows that they start out by relying on non-linguistic knowledge, and only later learn the meanings of the pertinent words (Clark, 1979). At the same time, because their nonlinguistic strategies map more directly onto some meanings that others, these studies offer information about which terms are likely to be mapped first. Note, though, that children can only be credited with the relevant meanings once they can contrast two or more terms in the same context, and, for instance, reliably place a small toy in or on a box lying on its side in response instructions with *in* and *on* respectively.

Pragmatics in acquisition

In 1970, I found that children were attentive to *given* and *new* in conversation. And just as they can talk about events out of order, so they may also talk about the object-affected before the agent, or the result before the cause. But when two-year-olds do this, they often mark a demoted agent as a source with *from*, as in *These fall from me* (D 2;2,3) or *I took my temperature from the doctor* (J 2;2). Kathie Carpenter and I found that they also mark other sources as sources, when in non-initial position – causes, possessors, and standards of comparison – with the same *from*. Only later do they learn the conventional assignment of *from*, *by*, *because*, and *than* (Clark & Carpenter, 1989).

My interest in pragmatics had earlier led me to look at the role of conventionality and contrast in the lexicon at large, particularly in the formation of new words (Clark & Clark, 1979), where speaker and address rely on a Gricean contract in working out how to interpret a coinage in context. In effect, a coinage may not carry precisely the same meaning as any existing, conventional term. This is what enables children, eventually, to get rid of over-regularized forms like *bringed* or *goed*: the conventional *brought* and *went*

already carry just those meanings and are the conventional forms produced by adults (Clark, 1987 and 1993).

As children learn word meanings and how they contrast with each other, they must also learn about the meanings of parts of words, the stems and affixes. With Susan Gelman, Barbara Hecht, and Randa Mulford, I looked at when and under what circumstances children coin new compounds, and at the precedence of affixes over word order in compounds like *button-thrower*. Children's analyses of word structure and their construction of new forms appeared to be guided by further principles. Ruth Berman and I therefore embarked on a series of comparative studies, designed to test these principles, and to explore differences between the acquisition of English and Hebrew. Overall, we found that children rely on elements with known meanings as they construct new words (transparency of meaning), but early on, they also prefer to make the fewest possible changes in a word-form (simplicity of form), and they are sensitive to what is productive in the adult speech around them (e.g., Clark & Berman, 1984). Much of this comparative research is summarized in Clark (1993).

To conclude, some of these issues in my Bressanone paper have surfaced again and again: accounting for meaning acquisition, reliance on non-linguistic strategies, and pragmatic factors in language use. Each of these has contributed, separately and together, to my studies of the lexicon and lexical acquisition, as well as to comparisons of lexical development and word- formation in different languages, and to analysis of the role of lexical acquisition in syntax (Clark, 1995).

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True negatives and false beliefs

Margaret Donaldson

When I look back over the data that I presented at Bressanone, one finding stands out for me: of the 155 children we studied, only about one-third proved able to look at an object and complete a negative description of it. For instance, if the experimenter pointed to a blue circle and asked the child to complete: *This circle is not ...* then by far the most common response was *blue*. For children making this error the negative seemed to have no force. The attribute seen was the attribute named.

The children were aged five and six, and the result may not seem particularly surprising. But what struck me at the time was that Klima and Bellugi (1966) had observed children making remarks like: *He not little, he big* at the age of two. Now the developmental gap between two-years-olds and six-years-olds is very great, and I became intrigued. My interest in negation waned but the more general question stayed in mind. I wondered if this developmental pattern would be found widely in cognitive and linguistic activities. If so, I very much wanted a better understanding of its nature.

It then happened that, during the '70s, I had a gifted group of colleagues and students to work with and soon there were opportunities to find out more. We began to test some of Piaget's theoretical claims by varying the tasks he had used. As an example, consider the famous 'mountains' problem where subjects are shown a three-dimensional model of hills marked by various distinguishing features and are asked, while sitting in one position, to choose photographs showing what a doll would see when placed at a number of points around the display. The task is not easy. Even adults can quite readily make errors. However there is one error, common among young children, which adults decidedly do not make, namely that of choosing the picture showing just what they see themselves.

Children below the age of six or seven tend to hold to this error, once made, no matter how the doll is moved. Piaget concludes that they are bound by 'the egocentric illusion' and are genuinely unable to regard their own point of view as only one among others. Consequently they cannot co-ordinate possible viewpoints and construct a coherent three-dimensional representation.

Martin Hughes (1975 and also Hughes & Donaldson, 1979) set out to test the Piagetian interpretation by devising a task that differed from the original in important ways while still keeping the co-ordination of viewpoints as a necessary condition for success. In the new task the child's job was to help a little doll to hide behind various walls from a toy policeman. Later two policemen were introduced, then three. The prediction from Piagetian theory that the children would place the doll where they themselves could not see it, disregarding the positions of the policemen, was not supported. Children aged between 3:6 and 5:0 were correct on very high percentages of trials.

There was – and is – no dispute about Piaget's data. On his task, children typically respond as he says they do. So what is going on?

In my Bressanone paper I gave the results of testing a number of hypotheses about conditions that might facilitate success with negation. These were aimed at manipulating expectancy and the like, and they all entailed such variables as shape or colour of the materials or order of presentation. None of the hypotheses received any support. Clearly they did not get close to the heart of the matter. However, the work that we did in Edinburgh in the '70s began to throw more light. We soon found that the Bressanone pattern is indeed widespread. And before long we began to understand the conditions that determine it. To summarise a long story, it turns out that these have to do with the bringing into play of relevant intentions or purposes.

The first and most potent condition favouring cognitive and linguistic success is the arising of an appropriate purpose, generated by the child herself. When a child spontaneously says *He not little, he big*, she has formed her own purpose: she has detected an error and wants to correct it. Notice that it is possible for an adult to encourage the

arising of such a purpose by deliberately making gross errors, as was shown by Roy Pea (1980). But still the purpose, if it comes, comes from the child. Pea found true negatives being produced to deny false statements as early as 18 months of age, a fact not without relevance for developmental theories about the understanding of false belief. Making a false statement is not the same as holding a false belief but the recognition that people may falsely represent the world is surely needed for an understanding of either (Wimmer & Hartl, 1991). In experimental studies, evidence of this recognition is not usually obtained till around the age of four. However, it cannot be wholly absent from the mind of any child who corrects a falsehood, even if the power of a strongly felt purpose is needed to make it manifest.

By contrast with self-generated purpose – and it is a sharp and important contrast – there comes the case where someone else generates a purpose and invites the adoption of it, as commonly happens in an experiment. For instance, in the Bressanone study we asked the children to look at a geometrical figure and complete a statement about it. We proposed a goal.

Now it turns out to be quite hard to get young children to accept a purpose – any purpose – that is presented in a contrived setting 'out of the blue'. Before the age of three they either won't do this or they can't, as novice researchers soon discover. I believe that they can't, and I shall try to explain why.

First, though, we need to consider what happens as age four approaches and children become better able to take part in planned studies. What then emerges is that performance depends to a large extent on the nature of the purposes entailed and how these are presented. The precondition of success at this stage can be simply stated. It is that the children should see the sense of what they are being asked to do. In later life we all learn through the formalities of our education to do much that, at the time of doing, appears pointless. But young children respond well only when the point is clear to them. Beyond this it also helps if the enterprise stirs some kind of positive emotion, some desire to take part.

Both conditions are particularly well satisfied in the

'policemen' task. The enterprise seems thoroughly sensible to three-year-olds. Also relevant emotion is not lacking. The children are enthusiastically on the doll's side and eager to help. In these circumstances the appreciation of other points of view ceases to be hard at all.

Notice, however, that this task, easy as it is, still makes significant demands of a kind found in all such contrived problems. The children are not actually in a situation where someone is hiding and someone is seeking. Rather they must 'enter into' such a situation through processes of interpretation which amount in effect to imaginative context-building. The toys on the table do not *provide* a context. They merely give support for the imaginative work needed in such circumstances for the making of sense.

Problems vary greatly in regard to the difficulty of the context-building work that they demand. However, children generally cannot undertake this kind of work at all until they are between the ages of three and four. A self-generated purpose is so markedly supportive of performance precisely because, when it arises spontaneously in the midst of ongoing events, there is no need for context-building. The relevant context is given. The child's mind is full of it already. There is no imaginative work to do.

Conclusion

I have discussed these matters and their implications at some length elswhere (Donaldson, 1978, 1980 and 1992). The single conclusion I draw here is the one that bears most directly on the study of development. It is that developmentalists are apt to encounter a great temptation but that we now know enough to recognise it for what it is. Here's how it arises. First we demonstrate with full experimental rigour that, in one setting or even in several, children of age n fail to display some skill or knowledge x (such as co-ordinating perspectives, or producing true negatives or understanding false belief). Now comes the danger: we are powerfully tempted to think we have shown that, in some absolute sense, these children do not 'possess' x. But not so. This is a false belief. It depends for its continuance on inadequate ideas about minds and how they extend their range.

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Semantic development of adjectives

Robin N. Campbell

When Roger Wales and I made our contribution to the Bressanone conference, we had just completed a research project in Edinburgh which surveyed and attempted to map pre-school children's developing knowledge of a range of relational terms (see Donaldson & Wales, 1970, for a more comprehensive report of this project). My particular part in this longitudinal project was to explore knowledge of the size adjectives, in their positive and comparative forms, and knowledge of the terms *same* and *different* as revealed by matching and sorting tasks. We came to a number of conclusions:

(a) *Polar Assimilation*: – that the meaning of the unmarked member *same* of the adjective pair *same–different* was mastered first; that the meaning of *different* was initially assimilated to that of *same* before eventually becoming differentiated as an antonym; that some other antonymous adjective pairs might well be acquired in the same fashion.

(b) *Semantic Field Effects*: – that development of the lexical field of the size adjectives showed not only interesting 'vertical' progressions and relationships of the antonymous pairs such as *big–little* that compose it, but also 'horizontal' dependencies in development between terms of like polarity - *big*, *long*, *tall*, etc. The work of John Lyons – who was at that time based in Edinburgh (e.g., Lyons, 1963) – and Manfred Bierwisch (1967) were potent influences in causing us to think about these adjectives in those ways. For example, in reviewing our study of adjective production, we noted in one of our groups "a fall in the use of *big*" over the period (mean age 41 months to mean age 56 months) of the study and located this fall in the performance of "a few individuals ... who appear to have specialised their use of *big* ... to one particular sort of variation" (p. 392), perhaps as a result of

the contemporaneous development of the other more specialised terms of like polarity (see pp. 258-259 of Campbell & Wales, 1970 for some elaboration).

(c) *Growth 'errors':* – that change in meaning in particular, and development in general, was not a smooth progression in the direction of standard adult states, but often exhibited (superficial) regressions. The general case was argued in the latter part of Donaldson (1971). With respect to the size adjectives, we noted that some children, who had correctly selected the two extreme objects from an array in response to requests for *the longest* and *the shortest* one, etc., 'progressed' over the 15-month study period to a stage of error: having picked the longest object in response to *longest*, they then incorrectly picked the next longest in response to *shortest*. We also noted the restriction in the range of application of *big* described in the previous paragraph, observing that (p. 392) "this transition might plausibly be considered as a developmental advance since it marks the development of the more specialised adjectives, although it leads to the appearance of less adequate responses".

(d) *Principled Differentiation*: – this change of meaning of *big* suggested to us that related size adjectives of like polarity might (p. 394) "proceed initially from indifferentiation of the members of [the] set ... to complete differentiation, ... whether or not these terms ... are completely differentiated in the adult's system".

(e) *Development as Structural Change*: – the animating idea behind all of the ideas just described was that corresponding to structures identified in language there would be homologous structures of mind, and that the formation of such structures was the proper subject matter of language development. This orientation was, of course, derived from study of the works of Chomsky, Fodor, Katz and Piaget.

These were good ideas, although re-reading these old papers forces the painful conclusion that the data we used to reach them was weak and over-analysed. What has become of

these good ideas? I will comment on (a) and (b) in some detail, with asides on (c) and (d).

Polar assimilation

The idea of initial polar assimilation was supported by Eve Curme, who worked in our group as one of Lyons' doctoral students, and by her husband-to-be Herb Clark, who was a frequent visitor. Herb took Eve and the idea to Stanford and the Clarks tried it out in their energetic way (e.g., Clark, 1973). The notion that initial assimilation might be a general feature of adjective development was, however, soon abandoned: most seemed to agree with Carey (1978, p. 278) (see also Richards, 1979) that new adjectives were "first map[ped] onto the comparative core (including polarity)". Our best data, however, – that relating to the pair *same–different* – have not been explained away: there have been several replications of our result under a variety of modified conditions (Webb, D'Olivieri & O'Keeffe, 1974; Sinha & Carabine, 1981; Speer & McCoy, 1982) and the only published failure (Glucksberg, Hay & Danks, 1976) was disposed of by Speer and McCoy (1982). However, until and unless other robust polar assimilations are discovered it is unlikely that this finding will generate new research.

Semantic field effects

The search for semantic field effects in the domain of adjective development has been a constant factor driving research and shaping theory in semantic development from 1970 to the present day. Besides continuing strong interest in the size adjectives there has been much research investigating the field of colour adjectives, stimulated by the influential structural theory of Berlin and Kay (1969). So far as the size adjectives are concerned, the original work together with her own research stimulated Eve Clark's (1973) strong hypothesis that much development could be characterized as semantic feature addition to initally incomplete structures which produced overgeneralization errors. In turn, Clark's proposal led to further studies of the

size adjectives by Brewer and Stone (1975) and Bartlett (1976). While these studies provided some modest support for Clark's hypothesis, their results were open to other interpretations and were not sufficiently fine-grained to show the influence of new vocabulary on established structures. Nor were they longitudinal. Although our suggestions and Clark's hypothesis are best tested by studying individual semantic development and despite a long tradition of longitudinal methodology in studying language development, semantic development has mostly been studied using cross-sectional experiments – perhaps reflecting a different origin in the experiments of Werner and Kaplan (1952) and Brown (1957). The single notable exception to this generalization has been Bowerman (e.g., 1978, 1982), whose monumental and painstaking analysis of her daughters' language development extended the tradition of Leopold (1939-49) and Brown (1973) to the semantic domain.

Susan Carey became involved with Bartlett's work around 1975 and carried out cross-sectional studies of the size adjectives herself. Unfortunately, these studies – sketched in Carey (1978, 1982) – were never published, but they served as ammunition in a fairly vigorous attack on Clark's hypothesis. In some ways that dispute may be seen as metatheoretical rather than theoretical. While Carey argued that initial structures for new adjectives were little more than routines for applying them to certain restricted types of object, it might well be said that such structures are not semantic structures since they do not directly relate one lexical item to another. That is, the dispute may be taken as a dispute about whether and how a boundary should be established between 'sense' and 'denotation' (see Lyons, 1977, and Dockrell & Campbell, 1986). But Carey's contribution to the study of field effects was fundamental: she showed that close study of the acquisition of a new word could reveal effects of established vocabulary on the structure assigned to the new word, neatly turning the focus of research around. Thus, the explicit or implied contrast between the newly-introduced word and known vocabulary in the same field (helped along by a principle of differentiation – see my (d) above) located the new word in the existing field and established much of its meaning. Oddly enough,

Clark's own work took the same turn around the same time with studies of how existing vocabulary limited the formation of novel compound words, although here the 'field effects' cross syntactic boundaries. These two research traditions continue strongly (for review, see Clark, 1993).

A third strong research tradition was begun by Maratsos (1973 and 1974), who assimilated an earlier finding of Lumsden and Poteat (1968) to Clark's hypothesis. Lumsden and Poteat had shown that 5-6 year-olds consistently misunderstood *big* by interpreting it as applying solely to the vertical extent of upright stimuli. Maratsos confirmed this finding but also demonstrated that younger children understood *big* correctly, interpreting this result as a field effect of the acquisition of the specialized adjectives *tall* and *high*, just as Wales and I did with our production data. This spectacular and robust result, achieved with considerable experimental sophistication, is of course a growth 'error' (see (c) above) and has stimulated research ever since in a sequence of around 15 experimental studies (none longitudinal!) – most recently Sena and Smith (1990). The tradition is overdue for review, but here is hardly the place for it.

I cannot resist making a few points, however. The linguistic sensitivity that animated early work (Bierwisch was still fresh in our minds!) is missing from much of the later work – for example, the natural way to refer to variation in the main dimension of the rectangular pieces of card used as stimuli is with *long*, but typically *tall* is used in these experiments when the card is placed upright and (worse) *wide* when it the main dimension is horizontal! Again, Maratsos' simple and direct methods have been replaced by procedures of bewildering complexity (especially to the subjects). Lastly, since the inequality

$$\text{height1} - \text{height2} > \text{width2} - \text{width1}$$

is equivalent to the inequality

$$\text{height1} + \text{width1} > \text{height2} + \text{width2},$$

the 'salient dimension difference rule' discovered by Bausano and Jeffrey (1975) and explored by Ravn and Gelman (1984) is just Anderson & Cuneo's (1978) *height* +

width rule , which 5 year-olds follow — as Maratsos showed — when they are trained to select the bigger object using methods that do not require them to interpret a size adjective!

Development as structural change

Those who subscribe to this principle are now few and widely spaced. The disease of innateness has spread from the large population of infected language development researchers to the formerly sound cognitive development community. For diagnosis and prophylactic advice I recommend reading the obscurely-published Campbell (1988) and Campbell and Olson (1990)!

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Acquiring comparatives and comparing acquisition

Roger Wales

In pondering how the field addressed by Wales and Campbell (1970) has developed and changed, some of the assumptions of where that field was in 1969 need to be made explicit. After a decade of child language study dominated by Chomsky's influence and the centrality of syntactic studies, this study and other's like it (see for example, also in Flores d'Arcais and Levelt, 1970, such studies as Huxley, Clark, and Donaldson) were among the first to take seriously three kinds of issues in child language. The first was the intrinsic difficulty of distinguishing in a reliable way between syntax and semantics at least as such a distinction might be applied to language performance. The second was relevance of the perceptual and conceptual categories that the child might be bringing to the language acquisiton task. The third was the relevance of the way a particular language might realise these relations (see also Slobin in the same volume). These approaches were nascent at the end of the sixties but came to dominate child language studies in the seventies (see for example, Fletcher & Garman, 1979).

By the end of that decade another issue had come to the fore which was to have a fundamental effect of much of the scholarship that followed, namely the issue of 'learnability'. It had surfaced in the psycholinguistic literature of the early seventies and came to have central significance, not least in redirecting theoretical attention to the problems of language acquisition as it applies at the sentential level. The issue of learnability developed in conjuction with that involving the biological uniqueness/contribution of language. For one of the stronger versions of that conjunction see Pinker (1994) .

This commitment was paralled by one (usually in other, and opposing quarters!) to examine the child's language performance in the light of contextual factors. These might

include (again at different times, and often with different scholars) 'contextual factors' such as social, physical or linguistic contexts – the latter either involving considering performance factors above the level of the sentence, or the ways in which the contextual factors might affect the selection or processing of a particular sentential structure.

These trends can be tracked in the literature by examining e.g., the first versus second editions of Fletcher and Garman (1979,1985); Wanner and Gleitman (1982); Ervin-Tripp and Mitchell-Kernan (1982), Kuczaj (1984), Romaine (1984, 1988), MacWhinney (1987), Pinker (1989), Krasnegor, Rumbaugh, Scheifelbusch and Studdert-Kennedy (1991), Gelman and Byrnes (1991), Atkinson (1992), Clark (1993), Gleitman and Landau (1994). There are also, of course, the continuing set of proceedings of key meetings such as the *Stanford Child Language Research Forum*. This is not a random list but it could easily be extended! However this set of references represents much of the state of the art and many of the artists. It would certainly give the reader a fair idea of many of the main events in the language acquistion literature since Flores d'Arcais and Levelt (1970).

Wales and Campbell was a report of preschool children's performance with spatial comparative adjectives in both a comprehansion and production task. The children seemed to go from performing well with the spatial adjectives at an earlier stage, and then do develop increasing errors before further development resulted in 'correct' performance. On close inspection this seemed to be a consequence of the children first acquiring a 'correct' understanding of 'cover-all' antonyms *big* and *wee* (this work was done in Scotland) and the 'errors' were seen as a result of the increasing specialisation of the child's linguistic knowledge as it applies to the spatial domain. Robin Campbell and I raised a number of issues about what these sorts of data might imply, such as the possible utility of generalising attempts in generative phonology to specify 'naturalness conditions'. These included some aspects that were to be developed applied extensively by Clark in her influential 1973 paper: e.g., a relation between being natural and being unmarked; the umarked form being acquired first onto-genetically; an inverse relation between naturalness and the

number of features required for specification. While she has modified and elaborated her views, for instance in the ways in which Clark now argues for a principle of 'contrast' (Clark, 1993) some of the kinds of reasoning involved in pursuing this kind of argument can be seen elsewhere. They can, for example, be seen in some of the arguments presented to argue for the notion that the child is biased toward a 'whole object' – that a word names a whole animal, say, rather than its parts, and that it learns such names as *nose* through a process of 'mutual exclusivity' (Markman, 1994).

It is one of the abiding and interesting puzzles to try and determine to what extent the different 'principles' with which the language acquistion literature is now replete are refering to the same or different psychological processes. A complimentary methodological approach was represented by the late Renira Huxley's paper, in which careful examination of individual children's production 'errors' were used as the basis for the interpreation of the children's knowledge structures. This approach has been most vividly extended by the work of Bowerman (e.g., 1982). One of the earlier foci looked at child language through a categorical semantic lens: hence our interest in spatial relations. Much of that specific interest has waned in favour of commitments which concentrate on other linguistic domains better adapted to to the processing questions in hand. Good examples of this are Bloom (1994) and Soja (1995) their (different) interests in the acquisition of quantifiers. Bloom expertly addresses the question of whether the acquisition is driven from semantics to syntax or vice-versa, and concludes the latter. Soja pursues a complimentary issue, whether children (and their parents) are resonsive to categories of nouns that may be quantified at merely the lexical level or also at the phrase level; again, concluding the latter.

The way in which students of child language construe syntax and semantics (with or without reference to pragmatics) these days, and the possible relations between them is – as one might properly have hoped! – considerably advanced on the art 25 years ago. There is a sense in which the field is starting to feel as if it is finally growing beyond the formative stages of organising the data and addressing more fundamental issues. Of course this is a personal reflection, and there is plenty of evidence still to the contrary!

One of the issues that we signalled 25 years ago which has not (yet) born much fruit, is the relevance of considering the semantic field (as discussed by the likes of Trier and Weisgerber) as a determinant of the process of acquistion and change: i.e. the way in which the acquisition of particular words occurs as a result of the learning and development of other, related, words. In fact our discussion of 'naturalness conditions' was framed in this context. Of course most studies have looked at the acquistion of sets of words, but there has not yet been much rigorous attempt to factor the structural constraints that these sets themselves provide into understanding of the processes that informs the child's acquistion/s. By 'rigorous' I mean something that would look like a solution that was computable, at least in principle. In fact the one concern that probably ought to be raised about the 'principles' of which the language acquisition literature is redolent, is precisely how they might be realised in a computable model.

A final word about the other half of the original title – the comparison of development (discussed in the original in the appendix). In the original paper we argued that it would be useful to have the means of modelling changes in observational states in a way that (a) permits a distinction between 'ability' and 'performance' (i.e. between an underlying state and the observations being used as indicators of that state), and (b) that analyses change simultaneously across a number of relevant observations. There are several developments in statistical models that address these needs. The first is in the area of latent class models, where a variety of change models can be formulated and tested. Typically, the models posit the existence of several underlying states, a distinctive set of probabilities for observations associated with each state, and one or more patterns of change between states (see Collins & Horn, 1991 for examples, some of which are directly developmental in the psychological sense). A second relevant development is a class of response models for repeated categorical observations (e.g., Grizzle, Starmer & Koch, 1969, see also Bishop, Fienberg & Holland, 1975). These models allow the analysis of repeated categorical responses as a function of time and other design factors and so address the need to examine diffrerent observations simultaneously. An example of the application

of such methods to child language data is given by Wales, Colman, and Pattison (1983). No doubt there are other examples, and the progressive application of more appropriate and sophisticated analytic methods to this domain of observations will be one way in which the field will move to a deep understanding of what the underlying phenomena are that need explaining.

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Do deaf children still need passive sentences?

Bernard T. Tervoort

The fact that prelingually deaf children do not acquire spoken language spontaneously has set them apart as long as one cares to go back in history. It then soon becomes clear that educating them has focused on language, in its different modes of communication. Challenging St. Paul's *Fides ex auditu*, churchmen, like De l'Epée (1712-1789) in France, and Gallaudet (1787-1851) in the USA, have, through the use of signs, raised their deaf pupils to reading and understanding the bible, and beyond. Representatives of the Enlightenment, like Diderot (1713-1784) in Paris, have stimulated such efforts, whereas others, like Heinicke (1727-1790) in Leipzig, have stressed the integration of the deaf into hearing society, and, consequently, the teaching of speech and speech reading as primary goals. The issue of signing, as deaf children's natural way of communication, versus speaking, as the obvious prerequisite for their integration, has led to a controversy, recently termed "The two hundred years' war in deaf education". (Tellings, 1995).

The acknowledgement of deaf sign communication as a complete natural language is of a fairly recent date. As an interested linguist, I discovered some linguistic features, not based upon Dutch but upon an unknown visual system in the filmed conversation of deaf 12 year old youngsters among themselves (Tervoort, 1953). Still, after Stokoe's first analysis of sign phonology (1960), and even after his first proposals for a sign grammar (1972), I still considered the question "Could there be a human sign language?" (1973) relevant from a theoretical linguistic point of view. It was only in the decades thereafter that the growing awareness of the true nature of signing has led to the establishment of the existence beyond any doubt of not one, but many different sign languages. American researchers led the way with their analyses of ASL (American Sign Language), but Europe was soon to follow. In her *In search of a language* (1990) Schermer states: "Since the first research into

American Sign Language in the beginning of the 1960s, many different sign languages have been discovered" (p.3), she then lists seven, with their principal authors.

Deaf children are naturally inclined to communicating in signs, based upon visual principles. Before the recognition of such behavior as truly linguistic, the predominant educational philosophy, however, was to discourage their signing and to stress the teaching of 'true' language through speech. Even after the acceptance of signing as a language in its own right, the incompatibility of the two modalities, visual versus auditory, and the interference of the former with the latter, still led teachers to avoiding signing, at least in the classroom, or to just using it as an additional means of supporting language at the most: the so-called use of signed English (signed Dutch, etcetera). That, of course, did not prevent the natural competence of deaf children for signing from surfacing time and again, not only in their private interchange but also in their difficulties with the rules of grammar underlying speech. One of the more sophisticated of these rules is the use of the passive mood, in both Dutch and English. *Advances in psycholinguistics*, as consequences of Chomsky's early innovative analyses of syntactic structures (1957, 1965), and the integration thereof in applied fields (like Lenneberg's 1967), led to the research project at the Institute of General Linguistics of the University of Amsterdam, titled "The understanding of passive sentences by deaf children" (as reported in Tervoort, 1970). The complete population of deaf children of reading age of all five schools for the deaf in the Netherlands was tested. In the years to follow this project was expanded to the understanding of the same set of passive sentences by normal and hard of hearing children as well as by aphasic patients (Tervoort, 1974), demonstrating the same structural difficulties with this grammatical construction, be it to a lesser degree. The focus in those years clearly was on the specific problems of deaf children with the learning of language based on speech, inasfar as these problems could be formulated anew and in greater depth thanks to the framework of the new psycholinguistic theory.

Since then, in the course of some 25 years, no less than a revolution has shifted research related to the language acquisition and further development of deaf children away

from the dominant focus on the learning of language based on speech to the awareness of deaf children's potential bilingualism as basic point of departure. Bilingualism, as interesting phenomenon of spontaneous dual language acquisition (as opposed to subsequent language learning) has traditionally drawn the attention of psycholinguists and linguists mainly otherwise engaged all along (e.g., Stern, 1928; Grégoire, 1937; Leopold, 1939-1949; Jacobson, 1942). More recently, within a few decades, it has grown into one of the most important chapters of developmental psycholinguistics and sociolinguistics (Appel & Muysken, 1987; McLaughlin, 1978; Romain, 1989; Cook 1993). Since educators often prefer accepted values and tested structures, and since it is usually up to them to try out adaptations of research findings to classroom practice, transfer has taken some time (Baker,1993-1995; Mahendra,1995, among others; Garcia & Baker,1995; Lyon, 1996). All educational implications, however, have had the availability, one way or another, of two languages based upon speech as a matter of course and as their basic point of departure. In other words, all languages involved were based upon the acoustic modality, i.e. the perception and production of speech sounds.

The importance of research into bilingualism in its psychological and educational consequences, as an important chapter of psycholinguistics, together with the acceptance of deaf signing as a complete language, have focused the attention of both researchers and educators on the potential of bilingualism of a special kind in deaf children. Again, research paved the way, both in the USA (Maestas Y Moores, 1980; Bonvillian, 1985, among others) and Europe (Tervoort, 1980; Volterra, 1978, 1986 and 1994; Loncke, 1990; Knoors, 1992; Brennan & Brier, 1995). Apart from all differences in scope, design, outcomes, etcetera, they all converge on the one dominant conclusion: signing is the only and unavoidable prerequisite of natural language acquisition by prelingually deaf children. The best proof thereof is to be found in the way deaf children of deaf signing parents acquire their first language in sign, running through developmental stages parallel to those of hearing children of same age. However, next to these correspondences in first language acquisition regardless of the modality

– acoustic versus visual – the basic structural differences between the spoken and the signed language are present from the very beginning. To mention the most important ones: the necessary visual person to person condition, the latency of iconic features, more simultaneity vs. strict successivity. The person to person condition is obvious: signers must see each other constantly in order to communicate. The permanent observation of each other's body, especially the face, results in a specific type of redundancy (Jouison, 1995). Signs are as conventional and arbitrary as words, yet, the obvious iconic origin of some can play a role in vivid expression and faster recognition within the context. In opposition to the strict consecutive order of words in sentences, as one of the fundamentals of grammaticality in spoken languages, overimposition of signs in combination with body language leaves more room for simultaneity (Jouison, 1995).

Apart from the acknowledgement of deaf adult signing as a complete natural language, and, consequently, the interest in deaf children's special kind of bilingualism, the poor overall results of traditional language teaching the oral way – no signing whatsoever – and even of the signed English (etcetera) variety brought about the educators' interest in starting with sign as first language (Gregory, 1996, among others). There was, however, never any doubt in anybody's mind that teaching every normally gifted deaf child the language of the hearing society, and through reading teach the basic school subjects, had to remain the final educational goal. Bridging the gap between the language of sign, as the natural first language, and the language of speech, as the necessary prerequisite for reading and further education, proves to be a staggering task. To begin with: how should hearing parents give their deaf child a language they do not know, in the crucial first acquisition period? Next: how could hearing educators use sign fluently and how could enough deaf signers qualify as teachers? Most important: what programs have to be developed to make possible the transfer from the sign modality to the spoken one? The answer reads: by meeting the challenge head on. Parents and teachers have to learn sign language, from primitive to skilled level. Deaf native signers must be trusted not only as far as teaching their own language is concerned but also in

early guidance and later education. Both in the USA and in Europe experimental programs have been started, with some promising results (Tervoort, 1996). It is evident that the bilingual deaf children of the near future are already here to stay with us. Will they need passive sentences? In the long run maybe, yes. But there is plenty research for the coming time beyond that particular grammatical construction.

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Beyond universals of grammatical development in children

Dan I. Slobin

"...I have no remarkable cross-cultural or cross-linguistic divergences to report here. There are a number of small, but intriguing differences, which may excite the technical interests of the psycholinguist, but what is remarkable at first glance is the uniformity in rate and pattern of develop ment." *(The first glance,* Slobin, 1970, p. 175)

"I would phrase the mission of the crosslinguistic study of child language [as] *the charting of crosslinguistic diversity in language acquisition* and *the empirical determination of its principled limitations." (The long look,* Slobin, 1996d, p.2):

My contribution to the 1969 Bressanone Conference on Psycholinguistics was titled "Universals of grammatical development in children" (Slobin, 1970). The paper reported on the first phase of a crosslinguistic quest, conducted together with Susan Ervin-Tripp and John Gumperz and our students at Berkeley in psychology, linguistics, and anthropology. In the spirit of the times, we were looking for commonalities in structure and process – and we found them. The first epigraph above, from the conference paper, makes the proclamation. In the course of the following three decades our attention was captured more and more by variation – and we have found *that* as well.

The task, 'beyond universals', is to find a common framework for understanding both uniformity and diversity. The second epigraph is from the fifth volume in a series of books that I have edited, *The crosslinguistic study of language acquisition* (Slobin, 1985a, 1985b, 1992, 1996b, 1996c). Those volumes present data on 28 languages from 13 language families, thus providing at least a beginning answer to my

call at the end of the Bressanone paper for "much more data on children's acquisition of various native languages" (Slobin 1970, p. 184). As the database expanded, it became evident that both universals and particulars were psycholinguistically interesting. About a decade after the Bressanone meeting, Lila Gleitman and Eric Wanner organized a conference on *Language acquisition: The state of the art* (in Philadelphia, in 1978). My paper (Slobin, 1982) went one step beyond universals: "Universals and particular in the acquisition of language". The epigraph from Volume 5 is drawn from a chapter that takes the necessary additional step: "The universal, the typological, and the particular in acquisition" (Slobin, 1966d). Universals are now understandable in terms of systematic constraints on variation. In this retrospective look at the years since our 1969 meeting, I will lay out some of the path that led me from the first glance to the long look.

Expanding the database

The most obvious change since Bressanone has been the remarkable increase in the available data – more languages, more types of language, more individual children, more types of data (observational and experimental, psycholinguistic and socio-linguistic, neurological and atypical, spoken and signed languages). In fact, the data have vastly outstripped our capacity to account for them – even to describe them adequately. And there are now several annual conferences devoted to child language every year, in several countries, along with the triennial conference of the International Association for the Study of Child Language. In the Bressanone paper I found universal patterns of early development across a small sample of languages: English, German, and Russian among the Indo-European languages; and beyond that group, only Finnish, Luo, and Samoan. The 'universal' patterns that I summarized came from diary studies and the first observational, tape-recorded studies. Those studies were followed by more extensive fieldwork, carried out by anthropologists and linguists using our *Field manual for the cross-cultural study of the acquisition of communicative competence* (Slobin, 1967). In the '70s, more

refined and systematic experimentation followed using a systematic sample of languages: English, Italian, Serbo-Croatian, and Turkish (summarized in Slobin, 1982). And in the past ten years, together with Ruth Berman, of Tel Aviv University, we have gone on to study connected discourse across a much wider age range – preschool and school-age children as well as adults – beginning with English, German, Spanish, Hebrew, and Turkish (Berman & Slobin, 1994), and now applying the same method of elicited narrative, with international collaboration, to dozens of languages, both spoken and signed. So we have expanded not only the range of languages, but also the age range and discourse framework of child language study.

With the astronomical increase in the range of languages studied, some patterns have become more and more familiar. The Bressanone paper presents a table of "Functions of two-word sentences in child speech, with examples from several languages". That table has been reproduced in numerous textbooks of psycholinguistic and child language development. Overall, I believe it is still a valid representation of early word combinations. In fact, much of what we encounter in new studies of the acquisition of various languages is familiar. In the Introduction to Volume 3 (Slobin, 1992, p. 8) I note that this sense of familiarity in encountering new data reflects sure growth in the field: "This sense of déjà-vu ... is a sign of how much we have learned about child language over the past years. The fact that it is possible to assimilate a large amount of new data to existing frameworks is reassuring evidence for the descriptive adequacy of those frameworks".

At the same time, of course, new findings have often been surprising – laying bare theoretical assumptions and calling for revisions: "The capacity to be surprised by new data ... makes our interpretive frameworks salient. One cannot be surprised without expectations" (p. 8).

One of the expectations in 1969 was "that word order has some salience as an early sort of syntactic rule developed by children exposed to different kinds of languages" (1970, p. 182). On the basis of widespread reports of early 'telegraphic speech', I proposed: "The first sentences are made up of unanalyzed, uninflected words, frequently using some principles of ordering to express semantic relations. In this

sense, syntax comes before morphology: children combine words before they isolate and make use of such devices as prefixes and suffixes" (p. 176). This was one of the expectations that led to surprises: In agglutinative languages like Turkish and Hungarian, inflections are early, and even appear at the one-word stage (summarized in Slobin, 1985c). Furthermore, comprehension experiments revealed that Turkish children as young as 2;0 rely on casemarking inflections to correctly act out all six possible orders of subject, verb, and object – while not attending to word order (Slobin & Bever, 1982). Such findings have made it evident that crosslinguistic generalizations must be framed in terms of typological patterns of the language being acquired.

Universal processes and language-specific patterns

Already in the Bressanone paper, there were suggestions of differences between individual children and individual languages. I suggested that psycholinguistic commonalities might be found on the level of processes of acquisition: "the notion that the child brings certain general and universal organizing principles to bear in the process of discovering the grammar of his native tongue" (Slobin, 1970, p. 181). This idea developed into sets of 'Operating Principles', which, when applied to any particular language, would result in a standard sort of early grammar ('Basic Child Grammar'), while also guiding the course of acquisition of the specific patterns of each individual language (Slobin, 1973, 1985c). That is, the level of universals was to be sought in "a set of procedures for the construction of language" (1985c, p. 1159). The model was a mix of operating principles 'prespecified' for language acquisition, along with general learning principles – all functioning in the contexts of cognitive development and social interaction. Some of these principles have continued to be useful: especially those involved in the perceptual segmentation of speech and the sorting of stored linguistic data into grammatically relevant patterns.

My expectation was that "children move from a *universal* grammar to the divergent grammars of individual languages" (1985c, p. 1160). However, that expectation was grounded

in the belief that there are common semantic starting points for the organization of grammar – across children and across languages. At Bressanone I proposed: "The underlying semantic-cognitive structure of human experience is universal, and these universals of structured experience seem to be expressed in strikingly similar fashion in child speech around the world" (Slobin, 1970, p. 175). It took me a long time to learn to distinguish 'cognitive' from 'semantic'.[1] For example, while children must have some prelinguistic cognitive understanding that objects can be supported on surfaces, hung from hooks, put into containers, and so forth, the languages of the world differ in how such notions are packaged into grammatical morphemes (Bowerman, 1993, 1996). In English, for example, the child learns to put up a cup *on* a table and a jacket *on* a hook, while a Dutch child must distinguish between a cup *op* a table and a jacket *aan* a hook. It has become more and more evident that the relation between cognition and language is two-way: to learn a language is also to learn ways of classifying experiences. The spirits of Humboldt, Sapir, and Whorf have returned – as reflected in a 1992 conference organized by John Gumperz and Stephen Levinson (Gumperz & Levinson, 1996; Slobin, 1996a). My contribution to that conference, 23 years after Bressanone, was to propose 'thinking for speaking' (Slobin, 1991, p. 12):

"[T]he activity of thinking takes on a particular quality when it is employed in the activity of speaking. ... 'Thinking for speaking' involves picking those characteristics of objects and events that (a) fit some conceptualization of the event, and (b) are readily encodable in the language. I propose that, in acquiring a native language, the child learns particular ways of thinking for speaking."

My path from Bressanone has thus led to an enriched and differentiated view of the human language capacity in which universals must be seen in the context of linguistic typology

1. A major guide was Melissa Bowerman, who was writing her dissertation at the time of the Bressanone meeting, and who ended up being a colleague of d'Arcais and Levelt at the Max Planck Institute for Psycholinguistics in Nijmegen (see Bowerman, 1985, 1993, 1996).

and the semantic patterns of particular languages, interacting with cognitive and communicative development. (The most recent way-station along the path can be found in Slobin, 1996e).

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PRODUCING AND UNDERSTANDING LANGUAGE

producing and understanding languages . . .

Good timing: Twenty eight years of team research on language use in time

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Sabine Kowal

When Hans Hörmann died in the spring of 1983, both of us were proud to be the representatives from Berlin at his funeral. We had much to be grateful for. He was the one who had suggested our initial collaboration on the O'Connell, Kowal, and Hörmann (1969) research reported in Bressanone, and he had steadfastly supported our research efforts with his encouragement and friendship right up to the last year of his life.

In the 28 years between '69 and '96, we have coauthored some 40 publications in psycholinguistics together. Nonetheless, we have not comfortably identified ourselves with mainstream psycholinguistics nor with any particular version thereof. In fact, for some time now, we have come to prefer the term 'psychology of language use' to that of 'psycholinguistics', largely in order to emphasize a broader scope of research and an independence of linguistics and of current psycholinguistic theories.

Despite a number of changes in the topics of research, the abiding focus of our empirical approach remains time – in all its various manifestations: speech rate and articulation rate, and variations of both; pauses within one speaker and between speakers: their duration, location, frequency, and function; all sorts of fillers, hesitation phenomena, discourse markers, and other transitional phenomena; and even the overall amount of speech in terms of syllables, words, or seconds. Along with this methodological focus goes the assumption that, from a psychological perspective, language in use is language used *in real time*. An adequate theory of language use has thus to take into account duration, tempo, and sequentiality or simultaneity of the linguistic behaviors of all the interlocutors involved in a meaningful communication.

The seventies

Originally, we began our cooperation with a laboratory experiment on semantic structure. German university students read narratives aloud and were asked to retell the stories. We analyzed the temporal organization of what our subjects read and said in order to assess the impact of meaning on language behavior when syntactic structure was kept as constant as possible. By the mid-seventies, we had already moved away from the study of semantic structure and never again returned to it. But we retained our interest in narratives for a while, including their development through childhood and youth as well as cross-linguistic comparisons between German and American subjects.

The readings and retellings of experimental narratives led to an interest in the rendering of some more artistic and meaningful material: drama, poetry, and homilies. We wanted to find out how well-trained professional speakers by comparison to untrained experimental subjects perform texts which are meant to move or persuade an audience. We were able to show that expressiveness takes time and that it takes specific abilities to be able to take additional time for linguistic performance when this extra-time is not used to correct errors.

The eighties

With the research on expressiveness, we gradually moved out of the laboratory into more field observation and next into more dialogical settings, wherein speech (both spontaneous and prepared in advance) with truly communicative purposes could be observed in a natural ecology. We found expressiveness a doorway to rhetoric, first with poetry, then with radio homilies, and finally with political discourse, both speeches and interviews. The latter in particular made us quite critical of a concept expounded by the Clarks (1977): the *ideal delivery*. Among other things it implied the principle of pausing only at grammatical junctures as the 'correct' way of executing a sentence. Perhaps more than any other doctrine within mainstream psycholinguistics, this one drove us into heresy, since it

really did mean the death knell of otherwise lively, spontaneously (and/or expressively) delivered sentences. Implicitly, of course, this principle also made the well-formed sentence into the official unit of performance in psycholinguistics, and this too we found unacceptable.

The decade also made explicit our inclination to engage in critical analysis of the whole tradition of research in the temporal organisation of speaking. Some of the traditional tenets of psycholinguistics which fell prey to our pen included: Goldman-Eisler's (1968) concept of *articulatory pauses* as determinative of a 250 msec cut-off point for pauses – because it eliminated pauses which are both perceptible and psychologically functional; Boomer's (1965) *phonemic clauses* – because they were found to be ill-defined and inappropriate as encoding units; and Butterworth and Goldman-Eisler's (1979) *cognitive rhythms* as units of spontaneous speech – because they were found to be neither rhythmic nor cognitive nor reliably recordable.

At the end of the decade of the eighties, each of us authored a book in the area of psycholinguistic research. O'Connell's (1988) was published as *Critical essays on language use and psychology*, Kowal's (1991) as *Über die zeitliche Organisation des Sprechens in der Öffentlichkeit: Pausen, Sprechtempo und Verzögerungen in Interviews und Reden von Politikern*.

In these same years, our interest was not only with the productive but also with the perceptual aspects of pauses. More specifically, we investigated pause reports, i.e. reports of locations in audio-recorded corpora where pauses were perceived by experimental subjects to occur. Part of our motivation in these projects was to compare reported with measured pause occurrence. We found both false negatives and false positives in the pause reports relative to measured occurrences, the former due largely to brevity of pauses and non-salient locations, the latter due to emphasis of a preceding syllable (by loudness or prolongation) and/or to grammatical juncture.

The nineties

Our concern with dialogical political rhetoric, and in particular television interviews, gradually drove us even more

away from mainstream psycholinguistics and right into the arms of conversation analysis. We found the questions that the conversation analysts were asking quite compatible with our own interest in the analysis of the ingredients to meaningful communication; but at the same time, coming from an experimental psychological tradition, we found basic methodological decisions wanting. Thus, we spent a good part of the first half of the nineties critically dissecting: various systems of transcribing speech performance; their practice of determining pause occurrence by perceptual analysis without physical measurement; and the classic "simplest systematics for the organization of turn-taking" of Sacks, Schegloff, and Jefferson (1974).

Our interest in transcription systems came about partly through our concern for the accurate recording of pauses and partly through James Deese and his student Jean Lindsay who shared with us their conviction that mistakes and biases in transcription were quite systematic. And lawful they proved to be. In fact, transcribing as one specific modality of language use became the topic of some of our more recent research.

On to '97 – and beyond

Before we detail our current projects, we must pause to acknowledge a debt of gratitude to the Alexander von Humboldt Foundation, the Deutsche Forschungsgemeinschaft, the German Fulbright Commission, the German Exchange Service, Georgetown University, and the Institute of Linguistics of the Technical University Berlin, all of which have provided generous support for our research over the years. Currently, we are enjoying our second Humboldt Transcoop Grant (1996-1998).

From our first Transcoop Grant (1993-1995) with its emphasis on more monological rhetoric, emerged a study of the inaugural addresses of United States presidents. Once again, our critical stance has encountered and challenged a number of tenets, this time those of communications theorists. The very nature of *conversational style*, as a concept meant to characterize effective political rhetoric in the media era, is called into question by our analyses of both the

printed text of all the inaugurals and the audio-recordings of the inaugurals from Franklin Delano Roosevelt to Bill Clinton. Eloquence is not predictable from any simple response measure, and the subtle interplay of oral performance on the basis of a text written for a solemn occasion delimits a conversation-like style considerably.

In our second Transcoop Grant, we are moving from our all male rhetorical inquiries into speeches and interviews of women politicians in both Germany and the United States. One of our interests is to critically assess research on gender related language use which has been accumulated over a period of more than 20 years and has come under scrutiny only recently. A number of phenomena about which we know very little as yet are being studied: interruptions in speech, turn-taking rules, emphatic (but anomalous) pronunciation of the definite and indefinite articles in English, use of first personal pronominals (singular or plural), and applause as a means to establish a dialogue in speeches.

Interruptions, for example, have been poorly defined, carelessly identified, and interpreted accordingly. Turn-taking has been interpreted largely agonistically, with the speaker being construed as the active party and the listener relegated to an inactive supernumerary. The model for this approach is reductively monological: Only one interlocutor has the floor at any moment and determines what is transpiring. Applause too turns out to be construable only in a dialogical framework.

Towards a psychology of language use

A year ago, we presented a paper in Berlin on the history of dialogue in psycholinguistics. In a sense, it was an easy presentation to make, because there has been no such history. Psycholinguistics, in the second half of the twentieth century, has been essentially monological. Furthermore, it has been based largely on written-biased, unrealistic demonstrational materials which were nearly always engaged on a within-sentential, noncontextualized basis and experimented on in a equally sterile laboratory setting.

We are convinced, however, that a viable psychology of language use must be just the opposite: dialogically based, field observational, fully contextualized, and ecologically valid. Language use is always communicative, but that aspect has been essentially lost, along with its corollary properties of being culturally integrated, socially immersed, learned, and occasional (man is *not* homo linguisticus). Dare we add that language use is always for a purpose; and it is intentional. We have set about modestly trying to build such a psychology of language use.

And so, we also have much to be grateful for to Ino d'Arcais and Pim Levelt.

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From comprehension to understanding

Herbert H. Clark

Old photographs are often more flattering than old publications. Old photographs of me show a young fellow looking eagerly into the future, whereas my old papers reveal a wordy novice with only a vague idea of where he was going. Still, my old papers offer some comfort. If the photographs remind me of what I once was, the papers attest to how far I have come. That is how I choose to look at "Comprehending comparatives", my contribution to *Advances in psycholinguistics* (1970), edited by Ino Flores d'Arcais and Pim Levelt.

"Comprehending comparatives" was about how people comprehend and recall comparisons such as (1) and (2):

(1) *John is better than Dick*
(2) *Alan isn't as short as Seth*

Sentences like these, I suggested, can be analyzed at three levels. Thematically, (1) is about John, who is to be viewed with respect to Dick, and (2) is about Alan, who is to be viewed with respect to Seth. Presuppositionally, John and Dick are to be viewed on a scale of goodness, and Alan and Seth, on a scale of shortness. Sentence (1), for example, is built on the proposition *John be Degree good*, which also underlies such sentences as *John is so good that he always wins*; *John is good enough to win*; and *John is as good as Dick used to be*. And lexically, *good* contrasts with *bad*, and *short* with *tall*. *Good*, the unmarked term, can be used neutrally, as in *How good is he?* but *bad*, the marked term, cannot, as in *How bad is he? Good* and *tall* are positive. *Bad* and *short* are implicitly negative. All three levels of structure, I argued, influence the comprehension of comparisons. Choice of theme affects speed of following such instructions as *Make it so that the black dot isn't as high as the red dot.* Choice of presuppositions affects speed of reasoning in such problems as *If John is worse than Bill, who is best?* And choice of adjective affects speed of

comprehension: Positive adjectives are comprehended faster than their negative counterparts.

We can view the fate of these ideas from two perspectives. We can trace what happened to specific proposals about comparatives, reasoning, instruction following, and markedness. Or we can trace what happened to the broader themes of comprehension and its relation to thought. I will do a bit of both.

Lexical structure

One premise of the paper is that the structure of word meanings, or lemmas, reflects the way we conceive of things. People conceive of height, for example, as linear vertical extent from an origin, or zero, at the bottom of an object. That defines the positive direction of height. There is a secondary reference point at the boundary between tall and short, and extent in the positive direction defines being tall and in the negative direction, short. These points of reference, and directions of measurement, follow directly from the way we perceive the world.

According to this proposal, positive adjectives should be easier to understand than negative adjectives. I cited some of the evidence for this proposal in the paper, but much more followed in the years afterward. In 1972, William Chase and I extended the notion to prepositions such as *above* and *below*, and in 1973, Patricia Carpenter, Marcel Just, and I applied it to adjectives and verbs such as *present* and *absent*, *many* and *few*, and *remember* and *forget*. Building on work by Peter Wason, Chase and I developed a general model for the comprehension of affirmative and negative assertions, as in *The plus isn't above the star*. A review of all this work appeared in *Semantics and Comprehension* (1976). Nowadays, only a few people seem aware of negation, its comprehension and representation, and almost no one tries to account for it in theories of comprehension.

The idea of conceptually based meaning has fared much better. In "Space, time, semantics, and the child" (1973), I argued for a perceptual basis for spatial and temporal terms. This work is still cited. Eve Clark and I then took this idea in quite a different direction. In such papers as our "When

nouns surface as verbs" (1979) and my "Making sense of nonce sense" (1983), we looked at how speakers create novel words on the spot, as in *My sister Houdini'd her way out of the closet* and *I did a Napoleon for the camera*. We argued that speakers intend these to be understood on the basis of conceptual material they take as common ground with their addressees. Later, in "Interpreting words in spatial descriptions" (1988, with Daniel Morrow) and "Words, the world, and their possibilities" (1991), I applied much the same idea to the interpretation of color terms, quantifiers, and spatial descriptions. Eve Clark's and my work is still cited, but the later work seems to have slipped by unnoticed.

In this area, then, the 1970 paper has had a mixed legacy. Interest in positive and negative adjectives rose and disappeared, almost without a trace, but the issue of conceptually based meaning got expanded, and its present-day descendants are alive and well.

Thematic structure

A second premise of the paper is that the structure of a sentence determines how well it functions 'in the ongoing discourse.' The thematic and presuppositional structure of comparatives, for example, strongly influences how people use them in reasoning and following instructions. The main arena for my demonstrations of these effects was the three-term series problem (e.g., *If John is better than Dick, and John is worse than Bill, who is worst?*). My interest in these grew out of a debate on the role of imagery in reasoning initiated in 1965 by Clinton DeSoto, Marvin London, and Stephen Handel and taken up in 1968 by Janellen Huttenlocher. In 1969 I argued that DeSoto et al. and Huttenlocher's findings were better accounted for by the thematic and presuppositional structure of comparatives than by imagery. This led to a rather rancorous debate between Huttenlocher and Tory Higgins, and me, in 1971 and 1972 in *Psychological Review*, later tempered by Philip Johnson-Laird, Robert Sternberg, and others. Ironically, one of the principal claims about presuppositions in comparatives was confirmed in 1976 by Higgins. Soon, however, interest in the debate

waned, and today, almost no one seems to know about it or about the particulars we learned from it.

Again, the more general thesis has fared much better. My own first move was to consider an aspect of thematic structure called given and new information. Susan Haviland and I argued, in 1974 and 1977, that speakers have a tacit contract with their addressees about their choice of given and new information. In using *What Margaret did was go to the hospital*, I presuppose that you already believe Margaret did something (the given information), but that you don't yet know she went to the hospital (the new information). I further assume that, in trying to understand me, you will take the given information as mutually identifiable and the new information as not yet known. And to do this, you may have to draw certain bridging inferences. Many of these ideas got amalgamated in later work by Ellen Prince and others, and the notion of bridging is still current.

The work on given and new information, however, started me down a path toward a radically different view of language use. The given-new contract is really a proposal, in the tradition of Paul Grice, about how speakers and addressees coordinate their individual actions based on what they take for granted about each other – their common ground. In 1978 and 1981, Catherine Marshall and I developed this point even further (see "Definite reference and mutual knowledge"). But when Deanna Wilkes-Gibbs and I, in all innocence, began in 1981 to look at definite reference in this framework, we discovered what conversation analysts already knew. In conversation, speakers and addressees don't just coordinate: They collaborate in a sequential, interactive, and opportunistic process. That led to a series of papers in the late 1980s and early 1990s with Edward Schaefer, Ellen Isaacs, Michael Schober, and Susan Brennan developing the notions of contribution to discourse and grounding – establishing something as mutually understood. Our conclusion: Language use is, in its primary form, not merely two individuals acting autonomously, but a joint activity like waltzing or playing a duet. This point is developed at length in my book, *Using language* (1996).

I see two fundamental changes in my thinking since the publication of "Comprehending comparatives." One is in the

relation between speaking and understanding. In 1970 I treated them as two autonomous processes; later I treated them as two coordinated actions, and later still as two parts of joint actions. The second change is in the conception of comprehension itself – what listeners do with what they hear. In 1970, I assumed their goal was to build syntactic and semantic representations of the sentences they heard. There was no mention of speakers and what they meant and little mention even of context. I now believe that view is profoundly misguided. As research since then has shown, the listeners' goal is to establish jointly with speakers what the speakers are to be taken to have meant with their utterances. Although that usually entails some analysis of the sentences uttered, it always requires much more. To me it is shocking how many students of language still stick with the 1970 conception of comprehension. Sentences are linguistic entities stripped of the time, place, and circumstances of their use, whereas utterances are signals produced by actual people at particular times and places as attempts to do things with others. If we have learned anything in the past 25 years, it is this. The listeners' goal isn't to comprehend sentences, but to understand utterances.

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Counting the cost: Processing options in symbolic and sub-symbolic systems

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In the 1970 volume that this current volume commemorates and examines, the title of my contribution was "Does ambiguity complicate the perception of sentences?" My answer was 'yes' based on then available experimental and analytical considerations. From contemporary perspective and hindsight, I shall today try to say something about how the ideas that animated those investigations of a quarter century ago have evolved.

History and diagnosis

What was the nature of the problem as conceived in the original paper? There were two motivations: on the one hand, ambiguity research addressed questions about the psychological relevance of grammatical descriptions of language, and on the other, it addressed questions of modularity and interaction in language processing. The latter concern survives to the present, while the former is not usually directly questioned in most contemporary discussions – it is, for the most part, taken as a given, even where there continues to be a question about the particular way in which a given feature of grammatical structure might be represented in mental systems.

The theoretical and empirical scene in the '60s and early '70s was marked by a sense of new problems being unearthed in language processing – problems that were rare and unusual specimens viewed from the perspectives that had dominated psychology (particularly in the United States) for most of the 20th century. Language processing from the perspective of generative transformational grammar marked a sharp departure from behaviorism and

neo-behaviorism and a commitment to a framework of 'mental representations' and 'mental processes' – the new focus was on the mind that drives behavior rather than on the behavior itself. In the effort to devise a theory of mental processes underlying language behavior, a key assumption was that any such theory had to account for the representational claims that grammars expressed.

Thus, an important part of early research in psycholinguistics took demonstration of the 'psychological reality' of grammatical descriptions as its goal. Ambiguity, structural and lexical, provided a test case for the demonstration of structural features of sentences whose very existence was the subject of contention in the '60s – viz., underlying structures, on-line phrasal segmentation, multiple lexical records. Evidence for systematic effects of variables based on grammatical theories of sentence structure was taken as demonstration of the psychological relevance of the grammatical formalisms. This general consideration had force quite apart from any of the specific computational issues posed by particular kinds of ambiguous structure and proposals for ways in which ambiguity might be systematically resolved. The psychological reality perspective was really symptomatic of the transitional stages of psycholinguistic theory – a period when language study was moving from the shadow of behaviorism to an accepted status as a field devoted to the investigations of mental representation and process. But, as the strength of psychologists' skepticism about claims for mental states and processes waned, primary attention shifted to the specific questions of how structural choice points could be identified and resolved, and the global issue of the impact of ambiguity on language processing was absorbed into various aspects of the main pursuits of psycholinguistic investigation. Indeed, syntactic ambiguity is the stock in trade of parsing theory, and the question of whether and where garden paths arise has been a prime diagnostic for the adequacy of proposals for human parsing models.

The enduring residuum is questions about how diverse non-syntactic contextual constraint affects the treatment of choice points, with continued debate over whether the computational costs of wrong analyses can be avoided by suitable prior constraining contexts. This is normally posed

in the context of questions of modularity vs interaction, and, while the test cases vary, the debate continues (see, e.g., Gorrell, 1995; McDonald, Perlmutter & Seidenberg, 1995; Frazier, 1995 for some recent reviews of parsing experiments). This is not the place for a diagnosis of the state of play in that debate, but I will return briefly to this matter at the end.

It's useful to distinguish questions about lexical processing from those of syntactic processing in any retrospective analysis of ambiguity issues. Although the broad question about interaction of form and meaning variables has also been central in both areas, the issues of lexical ambiguity have followed a somewhat different course from the syntactic. Claims for multiple activation of the senses of an ambiguous term in semantically constraining contexts have always carried the force of a claim for an autonomous, form driven lexical retrieval system. Work by Swinney (1979) and his collaborators, and several other investigators using cross-modal lexical priming provided a prima facie case for some form of multiple access (see, e.g., Prather & Swinney, 1988 for a review). Though there is certainly continuing disagreement and various contradictory findings, some rough consensus seems to have formed around a picture that includes activation of lexical records that is in significant measure context free but that reflects intrinsic interpretive biases associated with lexical targets; this is followed by a very rapid assimilation of contextual constraints that selects the contextually appropriate target. The work of Zwitserlood (1989) done in the context of cohort theory is perhaps one of the clearer syntheses of these matters.

Prognosis

The form of the original question about whether ambiguity is a 'complicating' factor in perception is based on a framework in which we can 'count the cost' of various alternative processing patterns. We are able to state in clear ways what the alternative analyses are in terms of claims for the activation of symbols and symbol constructs that compose the mental representations underlying language

comprehension and production. Certain forms of proposals about mental processes based on connectionist architectures are not well suited to such an enterprise (e.g., Seidenberg & McClelland, 1989; see also Forster, 1994). "Counting the cost" in a distributed network representation of the lexicon is not well captured in terms of activation of multiple candidates for the analysis of an input. Nevertheless, it is possible to find certain correspondences to the symbolic framework in some treatments of lexical ambiguity (e.g., Kawamoto, 1993). In part, this is due to the sheer force of the statistical separation of regularities of word form and regularities of word meaning. One cannot get very far with mixed models and the efficient coding of lexical structure preserves the essential character of the distinctions that rationalize a significant range of existing lexical ambiguity claims. But, where the idea of alternative construals of an input cannot be formulated in terms of a set of theoretically motivated grammatical representations, we will find the going difficult. So, for example, in proposals for lexically driven parsing systems for which analysis is the result of constraint satisfaction in a network that encodes the statistical likelihoods of collocations of specific lexical items described at multiple levels (e.g., St. John & McClelland, 1992; McDonald et al., 1995), the multiple syntactic paths defining syntactic ambiguity in symbolic parsers lose their determinate 'countable' character. In a certain sense, this problem devolves to the one that I earlier characterized as 'solved' – viz., the idea that grammars and the structural representations that they afford constitute genuine constraints on the adequacy of mental processing models for language. Processing models like that of McDonald et al. appear to deny that constraint. It is, of course, not a necessary feature of network models that they should do so. The evaluation of processing effects of ambiguity of syntactic representation may again have to do double duty: fodder for claims re the psychological reality of grammar as well as grist for the mill of parsing theory.

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Ambiguity, language, and cognition: Retrospect and prospect

Donald G. MacKay

It was a pleasure to reread my contribution to *Advances in psycholinguistics* after nearly three decades, and to trace its relations to subsequent work. The "Mental Diplopia" (MD) in my original title (subtitle, "Towards a Model of Speech Perception at the Semantic Level") was much too cute, the idiom of a young and unseasoned explorer just setting sail on the sea of psychological thought. However, many ideas developed in MD have withstood the test of time, and some still constitute 'unfinished business' for the field today. I will focus on four such ideas and their connections to current work.

Differing types of theoretical process: Priming versus activation

MD postulated two distinct theoretical processes, and adopted two labels for the first process: preactivation and partial activation. I later came to call this process *priming* (after Lashley, 1917), but MD specified its basic characteristics in detail: a brief, 'tentative', and passive or automatic process that operates interactively, in parallel, and unconsciously. More detailed characteristics of priming were only discovered much later, again by examining effects of ambiguity. For example, MacKay (1992) used effects of ambiguity on speech errors to determine the approximate range over which priming spreads from one unit to another.

MD called the other process *activation* and both the name and the properties of activation have remained unchanged despite subsequent discovery of additional processes: As MD noted, activation has more permanent consequences than priming, is all-or-none, and integrates across the various sources of priming delivered to a semantic unit. Activation also requires a special activation mechanism, and is necessary but not sufficient for awareness (see MacKay, 1990, 1992).

Ambiguity and context effects: Lists versus sentences

MD made a clear call for further research on "factors that affect the activation component," especially the biasing effects of context. My more seasoned 1996 opinion is that these biasing factors still need further research and carry important implications for current theory.

To trace some of these implications and their relation to work on ambiguity, consider lists versus sentences, differing stimulus contexts that characterize work labeled *memory* versus *psycholinguistics*. MacKay and Bowman (1969; see also MacKay, Abrams, Pedroza & Miller, in press) noted that common open class words such as *drive* usually have only a single meaning in sentences, but have many distinct meanings when isolated within a list of unrelated words.Taken in isolation, *drive* has over 26 distinct meanings, each with one or more distinct translation equivalents in Spanish, e.g., *campaña, vigor, incentivo, manejar, empujar, llevar, forzar,* and *obligar,* meaning 'campaign,' 'personal energy,' 'incentive,' 'to drive a car,' 'to transport (passengers),' 'to traverse distance,' 'to push,' 'to compel,' 'to drive off,' and 'to drive away'. However, *drive* allows only a single meaning and only a single translation equivalent (*manejar*) within a sentence such as *Mike learned to drive a car.*

Based on such observations, MacKay and Bowman (1969; see also MacKay, 1982) demonstrated that proficient bilinguals exhibit a semantic level practice effect for translation equivalents in sentences, but not for identical translation equivalents scrambled into lists. When German-English bilinguals read a sentence in one language 12 times at maximum rate with 20s between repetitions, the time to

produce the sentences decreased as a logarithmic function of practice, and the time to produce a word-for-word translation sentence in their other language on the next 4 trials (i.e. trials 13-16) showed perfect transfer: speedup in maximum rate for the word-for-word translations was 17%, and equivalent to 16 practice trials rather than 4, a transfer effect entirely attributable to semantic level processes. However, with identical procedures for the lists, transfer for word-for-word translations was -1% and nonsignificant.

The ambiguity of words in lists also makes sense of more recent discoveries involving mixed-language lists, e.g., *drive reemplazar manejar,* versus sentences, e.g., *Mike aprendió to drive a car and began manejar to work.* Specifically, Altarriba and Soltano (1996) observed semantic facilitation when proficient bilinguals recalled RSVP lists containing translation equivalents (*drive manejar*), whereas MacKay and Miller (1994) observed semantic inhibition or blindness (i.e. reduced recall of a word preceded by a semantically identical word earlier in the sentence) for virtually identical translation equivalents in RSVP sentences. Such contrasts for lists vs sentences pose problems for current theories (e.g., Gathercole & Baddeley, 1993, pp. 8-17; Shiffrin & Nosofsky, 1994; Zhang & Simon, 1985), where short-term memory contains phonological, articulatory, or acoustic representations, but not semantic representations, and call for a new, 'distributed memory' approach (MacKay, in press; Miller & MacKay, in press). Under this approach, short-term memory is not an isolable system consisting of distinct and separate subsystems (e.g., an executive system for sentences versus a phonolgical loop for lists), but instead represents "an umbrella term for a heterogeneous array (of) capacities for temporary storage ... distributed over diverse cognitive subsystems" (Monsell, 1984; p. 328). An example theory within this 'distributed memory' approach is Node Structure theory (MacKay, 1987, 1990, 1992), where mechanisms for storing and retrieving verbal materials in lists are inseparable from mechanisms that have evolved for producing, comprehending, and representing language (see MacKay & Miller, in press a; MacKay, in press).

Inhibitory processes in cognition

MD provided an early set of empirical and theoretical arguments for 'perceptual suppression', an inhibitory process in comprehension of ambiguous sentences. Although others have since suggested a role for inhibition in comprehending ambiguity (e.g., Burgess & Simpson, 1988), I know of no other more detailed account of how these inhibitory processes may work, their consequences for perception, and their relation to psychological data. Moreover, the basic inhibitory postulates of MD, i.e. "less time is required to suppress a meaning the less its probability within a given context," and "perceiving one meaning of an ambiguity requires suppression of the other," have yet to be disproved.

There now exist whole books about inhibitory processes with roots traceable to MD, and many new inhibitory effects have been discovered. For example, MacKay et al. (in press) postulated two theoretically distinct types of inhibitory process underlying repetition blindness (RB), the reduced probability of recall for repeated letters in briefly presented words and repeated words in RSVP lists and sentences. Labeling the two types RB1 and RB2, RB1 is a type of surface blindness: it occurs for letters in words and for words in lists, it is strongly influenced by orthographic and phonological factors, it involves existing units with old or highly practiced connections, and it reflects a theoretical process whereby units undergo self-inhibition (see MacKay, 1990; and 1987, pp. 146-187).

However, RB2 is a type of deep blindness: it underlies semantic blindness (MacKay & Miller, 1994); it is mainly confined to sentence processing; and it is linked to the process of forming new connections between words and phrases in sentences, rather than to a purely inhibitory process, a refractory period effect, or perceptual fusion of repeated words (see MacKay et al., 1994; MacKay & Miller, in press b). Semantic and syntactic factors strongly influence RB2, a postulate supported by several recent results. One is MacKay and Abrams' (1994) demonstration that RB increases in magnitude when repeated words occur in familiar (syntactic/semantic) phrases such as *good night* and *night gown* rather than in lists of unrelated words. Another is Abrams, Dyer, and MacKay's (1996) significant

increase in RB for RSVP screens that were phrase-incongruent, or contained non-phrases, as in *(They wanted to)(play sports but)(sports were not)(allowed)*, versus phrase-congruent, or contained complete phrases or syntactic constituents, as in *(They wanted)(to play sports)(but sports)(were not allowed)*. This effect indicates that RB2 responds to syntactic/semantic factors, decreasing or increasing in magnitude depending on whether RSVP procedures make it easier or more difficult to form word-to-phrase links (see MacKay & Miller, in press b, and Miller & MacKay, in press, for similar 'repetition deafness' effects that likewise comport with semantic blindness, with semantic and syntactic effects in RB2, and, more generally, with the distributed memory approach to language and memory (see Miller & MacKay, 1996).

Generality of the ambiguity problem

The final sentence of MD notes possible implications of ambiguity for other aspects of thought and perception. Subsequent work has sustained these implications. For example, MacKay (1987, p. 138) developed a general, theoretically based definition of ambiguity that applies across all language and cognition, including, e.g., phonological processing in speech perception. To concretely illustrate how ambiguity raises general issues, MD discussed relations between ambiguity, attention, and memory in dichotic listening tasks, ideas later developed in my "Aspects of the theory of comprehension, memory and attention" (1973). "Aspects" focused on three different types of ambiguity as analyzed in linguistic theories of its day, but neither MD nor "Aspects" prepared me for the quite different type of ambiguity (unimagined in 1970) investigated in MacKay and Fulkerson (1979), MacKay (1980a, b, c; 1983), and MacKay and Konishi (1994). Results for this unique type of ambiguity, known as 'generic he', surprised me, overturning the carefully developed and defended processing assumptions in MD (see MacKay, 1980c; 1983) as well as extant views on how pronouns are processed in language comprehension (see e.g., MacKay, 1983) and production (MacKay & Konishi, 1994). The

results even carried implications for effects of language on behavior, and for relations between language and other aspects of cognition, including social attitudes, personal feelings and motivation, cognitive styles, and descriptive versus evaluative thought. In retrospect, MD was overly modest in limiting the theoretical implications of ambiguity to "general aspects of all thought and perception." But then retrospect is so much easier than prospect.

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Semantic integration in sentence processing

Sheldon Rosenberg

We were honored to have had an opportunity to contribute to the original *Advances in psycholinguistics* volume and to the conference on which it was based. The organizers and editors (G.B. Flores d'Arcais and W.J.M. Levelt), by their indefatigable efforts, contributed much to the development of the field of psycholinguistics.

In our (Rosenberg and Jarvella's) original contribution to the *Advances* volume, we examined the role of semantic constraint in sentence processing in sentences at the extremes of what we referred to as semantic integration. Semantically well-integrated (SWI) sentences contained propositions that expressed lexical-definitional knowledge (e.g., *The maid cleaned the house*) or frequently experienced relations (e.g., *The dog chased the cat*). Semantically poorly-integrated (SPI) sentences, on the other hand, were not so constrained (e.g., *The maid bought the chair*). Clearly, the contents of SWI and SPI sentences differ in familiarity. They were selected with the assistance of college lexical dependency norms and matched on such variables as word frequency, length, grammatical structure, imageability (through experimenter judgment), and comprehensibility (again, through experimenter judgment).

The highlight of our *Advances* article was a report of research on the effects of semantic integration on sentence perception and memory. In a sentence shadowing task, under conditions of quiet and noise, recognition of both SWI and SPI sentences was virtually perfect under the quiet condition, a finding which was doubtless due to the simplicity of both types of sentences. Under noise, however, shadowing of SWI sentences was superior to that of SPI sentences, very likely due to an increased use of semantic knowledge when input was partially unintelligible. Consistent with this interpretation was the additional finding that under noise, incidental recall of the content words from SWI sentences was superior to incidental recall of the

content words from SPI sentences.

In our research on verbatim sentence memory, it was anticipated that the words in SWI sentences would be recoded into larger chunks than those in SPI sentences, and as a result, the former would be recalled better and in larger chunks than the latter. These expectations regarding recall performance were confirmed.

The reader will recall that the SWI and SPI sentences were matched on the variables of imageability and comprehensibility through experimenter judgment. Unfortunately, a study that I carried out sometime after the publication of the *Advances* chapter revealed that these judgments were not reliable. Undergraduates rated mixed lists of SWI and SPI sentences on familiarity, imageability and comprehensibility, with the result that while SWI and SPI sentences differed in rated familiarity, SWI sentences were also rated higher than their SPI counterparts in both imageability and comprehensibility, two variables that could have a powerful effect on sentence memory.

Thus, "these findings ... have raised serious questions concerning [many] studies in the literature involving sentences or discourse in which semantic integration, imageability-concreteness, or comprehensibility was varied without normatively controlling for the remaining two variables ..." (Rosenberg, 1987, p. 255; see this article for a sample list of such studies).

The implications of these findings for the variable of semantic constraint in sentence memory were evaluated in a series of studies with undergraduates in which lists of SWI and SPI sentences were closely matched on rated imageability and comprehensibility (and, of course, on other required variables as well). In three studies using free verbatim sentence memory and recall tasks, including one in which I made doubly certain that the subjects would encode the sentences semantically, *no difference was found between SWI and SPI conditions in sentence or word recall, regardless of whether the dependent variable was one of verbatim or nonverbatim (synonymous) recall.* In two of these experiments, however, there was a significantly greater tendency for an SWI sentence to be recalled as a whole than an SPI sentence. However, holistic recall of SPI sentences was substantial (approximately 75 percent) in all three experiments.

At this point, I seriously considered changing my career to something less hazardous to one's nerves than experimental psycholinguistics. However, when it occurred to me that the problem might have something to do with the retrieval task, I returned to the laboratory. Specifically, in a task, in which the subject supplies the recall cues, although the content of a SWI sentence is likely to be assimilated to established lexical-semantic knowledge, its episodic trace may not be strong enough to facilitate item recall. However, the addition of "intrasentence lexical cues during retrieval should provide a source of activation for [SWI] sentences that is not shared by SPI material" (Rosenberg, 1987, p. 260).

This reasoning was evaluated in a final sentence memory experiment in which undergraduates were exposed to either SWI or SPI sentences that had been matched on, among other things, rated imageability and comprehensibility. This time, however, the subject nouns from the SWI and SPI sentences were made available as cues during recall.

The results of this experiment were striking. Not only was intrasentence chunking significantly greater in the SWI condition than in the SPI condition, but so were sentence and word recall, regardless of whether the measure was one of verbatim or nonverbatim recall. In brief, there was little overlap in the recall distributions for the SWI and SPI conditions, and further analyses eliminated guessing as the responsible variable. However, holistic recall was appreciable in the SPI condition.

Unfortunately, I have not had an opportunity to replicate the Rosenberg and Jarvella (the *Advances* and Rosenberg & Jarvella, 1970) study of semantic integration in sentence perception with better controls for imageability and comprehensibility. However, if someone were to do so, given the above findings for cued recall, I would predict, on the assumption of a facilitating effect of spreading activation among the content words in SWI sentences, that recognition performance, and therefore incidental recall, for SWI and SPI sentences under quiet and noise would not differ from what Jarvella and I found earlier.

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Cognitive skills for reading functional texts: The multiple skills needed for reading in every day life

Patricia Wright

In the beginning

The Bressanone conference in the summer of 1969 enabled many people concerned with language processing to meet for the first time. Happy memories remain, particularly of the long evening discussions on a balcony overlooking the valley, that were animatedly continued at breakfast the next morning. As a field of enquiry psycholinguistics was both merging (psychology + linguistics) and emerging – one of the precursors to cognitive science (Osherson & Lasnik, 1990). The book resulting from the conference became a valued reference.

There was a theoretical debate about syntactic effects, and my paper suggested that performance differences reflected, at least in part, memory demands rather than linguistic factors. Disentangling comprehension and memory remained a problem even when research shifted from a focus on shallow syntactic processing to explore the deeper levels of text comprehension where pragmatics, inferences and readers' prior knowledge are all crucial (cf. Garnham & Oakhill, 1994). Research can start from theoretical controversy or from examining people's behaviour. I shifted my starting point and it soon became evident that not only deeper but also broader processes needed to be considered when people were reading texts in order to be able to reach a decision, or follow a procedure, or respond to a question on a form. These reading purposes contrast with reading for pleasure or reading to learn. The documents enabling such reading have been called 'functional texts' (Pander Maat & Steehouder, 1992). Functional reading plays a vital

communicative role both at work and at home. Forrester (1996) points out that this reading is usefully analysed in terms of purpose, strategy and outcome. Relatively little is known about how readers select and modify their reading strategies. My own explorations have shown that there are at least three important clusters of cognitive skills that underlie functional reading: search, interpretation, application. Within all three skill clusters there are strategic options.

Readers start with questions

Functional reading is a dialogue that starts with the reader asking a question that is answered by the text. The specificity of the question may vary, but learning how to formulate questions and search for answers in printed materials is one of the important language skills that adults acquire. As a consequence, readers engage in search activities, jumping around the page or within the document - e.g. zooming in on the total to be paid on an invoice before scanning to find out why it is so high (cf. Dillon et al., 1989). These attentional and filtering processes have no counterpart in listening skills.

Much that is visual about graphic language is crucial to the interpretation of a functional text. Readers parse the visual structure of the page. They distinguish figure captions from body text and keep track of the typographic variations among headings. Readers use this visual structure to disregard information believed to be irrelevant, and can articulate their reasons for ignoring information (Wright et al., 1982). Indeed for most professionals, the well-honed art of not-reading is a survival strategy.

Readers interpret the writers' intent

Interpreting functional texts often requires inference and pragmatic knowledge about contextual factors external to the document. Responding *Oh no it doesn't* to a notice saying *Sherwood forest walks* is to fail to discern that the communicative intent was to label, not to assert. Failures of comprehension in functional documents can have serious

consequences, and these can arise from writers assuming inferences will be made which are not. For example, few readers think *Use in a well ventilated area* means *open the windows* (Frantz, 1994). Comprehension problems also result from writers failing to spot faulty implications in their text. The medication instruction, *Take three times daily with meals* may be thought to mean take the pills at conventional meal times, irrespective of whether food is eaten then, whereas the writer may intend the tablets to be taken only with food. Because no theory can predict the knowledge of particular readers, usability testing is increasingly undertaken to detect readers' misinterpretations. But readers also need to gauge whether or not they have understood the text, and research now includes readers' metacognitive processes (Metcalfe & Shimamura, 1992).

Because functional writing places a premium on the communication of precise meanings, prose is often replaced by visually formatted information, with procedures written as numbered lists of steps, or verbal text is supplemented by illustrations. Studies of readers' skills in integrating text and graphics show that people are unwilling to pause and study an overview diagram while reading, even though their comprehension would be better if they did (Wright et al., 1990). The causal factors underlying this reluctance are still being investigated, but the need for readers to control their access to information within the document is further evidence that functional reading involves skills not needed for other kinds of reading.

Readers apply knowledge gained from the text

People read functional texts in order to apply the knowledge gained. This often involves further manipulation of the mental representations formed during reading. To carry out a procedure readers must create an action plan specifying the sequence of actions to be performed. For the instruction *Press the yellow button under the flap on the lower right hand side*, readers must plan an action sequence having at least five steps, and reversing the order of mention in the written instructions: (1) locate the lower right hand side, (2) find the flap, (3) lift the flap, (4) locate the yellow button, (5)

press the button. Our research suggests that for short sequences, using the natural imperative order works well. Presumably, readers have no problem creating an action plan involving few steps, so performance is dominated by comprehension processes. When the action sequence grows longer, the dominant factor becomes the creation and implementation of the action plan. This is easier when the order of mention matches the implementation order. We have yet to determine where the break-point comes for this reversal, but it shows the advantage of examining how several cognitive processes contribute to a specific reading outcome.

Another important aspect of the application of knowledge gained from reading relates to compliance. Many of us have ignored messages that say *Read me first*, even though we fully understood what the writer meant. Readers have beliefs which modulate the application of their understanding of the text. When texts give advice on safety issues relating to medication or machine operation the consequences of non-compliance can be very serious, and some researchers have found that modifying texts, or their contexts, can increase compliance (e.g., Campbell et al., 1994).

Writing functional texts

The perspective of readers as information-seekers, comprehenders, and deciders, yields implications for document design, because writers can support people's reading activities. Searchers need signposts; comprehenders need help recruiting relevant real world knowledge; decision-makers need memory support, fewer mental gymnastics when creating action plans, and recognition that belief systems may overwrite other goals. Electronic documents increase the scope for supporting readers (Wright, 1991) but creating this support may demand new skills from writers. Certainly non-professional writers tend to rely on prose when graphic forms of communication would be more successful (Wright et al., 1995).

Researchers once treated written texts as derivatives of speech, reminders of what had been said, or might have been. This assumption makes 'writing' a non-issue because it

implies that all speakers who can read must be able to write. Studies of the impact of literacy on culture (e.g., Ong, 1982; Olson, 1994) have shown the extent to which visual texts are not a written form of speech. Even for narrative discourse, oral traditions tell stories within a framework that makes them easier to remember; written stories are less constrained. Functional documents include materials that have no translation into a single speech form (e.g. decision aids such as transport timetables). Yet in spite of the unique challenges that functional reading offers to a comprehensive view of reading, it is seldom mentioned in overviews of research on language (e.g., Carpenter et al., 1995; Britton & Graesser, 1996). If research issues span reading and listening, this may seem evidence that it really is 'language' that is being studied. In contrast, theories of cognitive development now emphasize the interpenetration of perception, language, thought and social interaction (Light & Butterworth, 1992). This interplay is very evident in asynchronous communication skills.

In Bressanone, when the audience were shown that the pattern of performance for answering questions about sentences differed for readers and listeners, they suggested revising the experimental paradigm to remove intrusive memory processes so 'comprehension' could be clearly seen. That advice was not heeded. Instead it seemed advantageous to broaden the view of reading to include the what and the why, and much more fully the how of people's interactions with printed and electronic texts. From an early preoccupation with the surface structure constituents of single sentences, has grown an appreciation of the portfolio of cognitive skills that readers select among and co-ordinate when reading documents in every day life.

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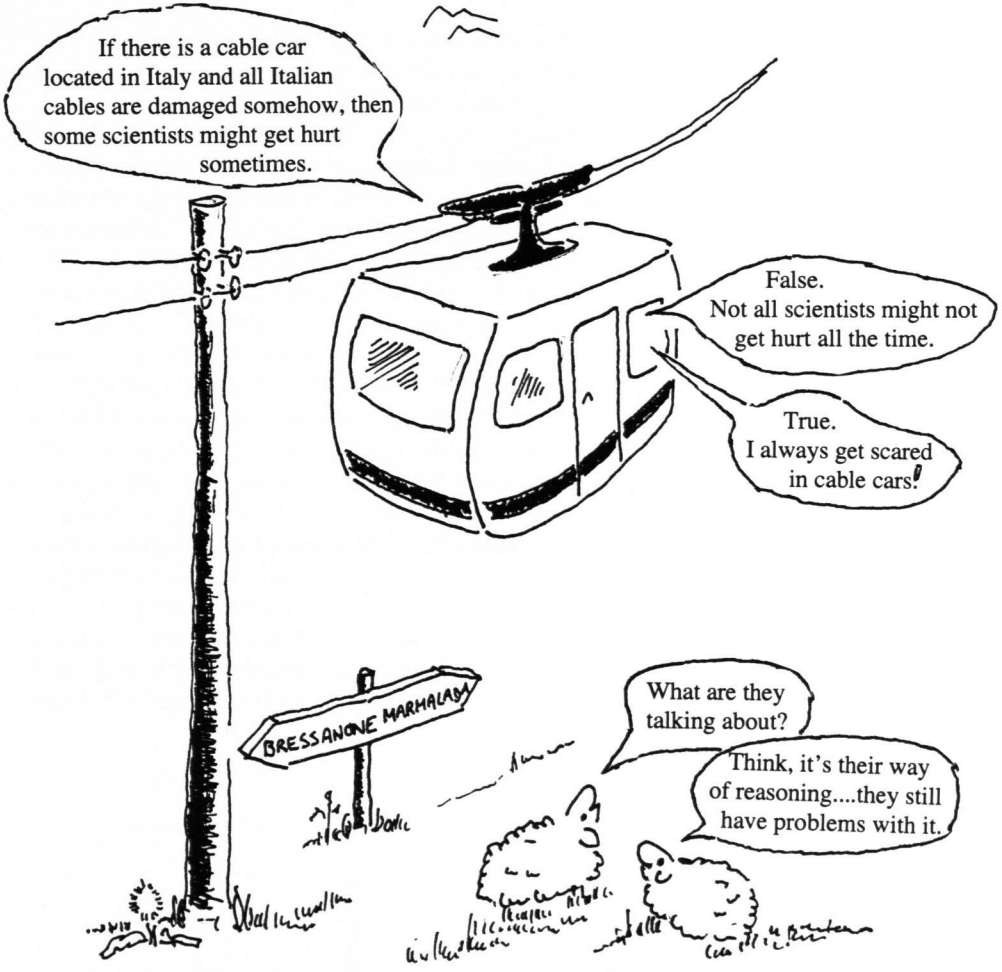
REASONING


If there is a cable car located in Italy and all Italian cables are damaged somehow, then some scientists might get hurt sometimes.
False. Not all scientists might not get hurt all the time.
True. I always get scared in cable cars!
BRESSANONE MARMALADA
What are they talking about?
Think, it's their way of reasoning....they still have problems with it.

The selection task: Beyond the first response

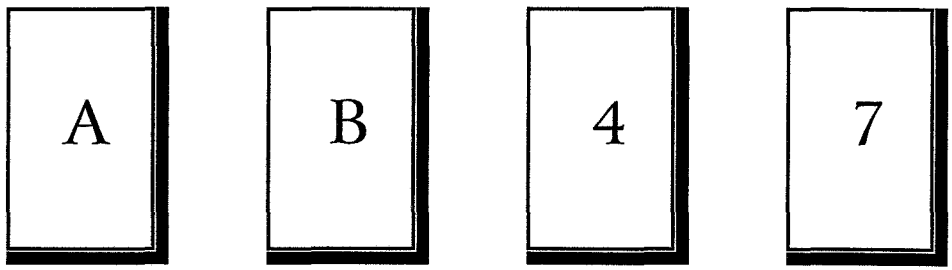
Peter C. Wason

Some people are made uncomfortable by the selection task because it is deemed remote from everyday life. How dull their lives must be if everyday life becomes the criterion for intellectual problems. Even more grotesque is the claim the solution rests on Popper's scientific methodology. On the other hand, it is argued that I founded a British tradition of research on reasoning – with a single task (Evans et al., 1995; Johnson-Laird, 1995). Certainly the selection task has become extremely popular – more so than any other in the literature. The 'Wason Task' has even earned considerable attention in the *Internet*.

The task had a humble birth. Psychologists like to see themselves as *very* scientific. They hardly ever acknowledge the role of luck or play in their own experiments even though they acknowledge them in famous discoveries, e.g., in Fleming's discovery of penicillin. It is humble products which demand an impeccable methodology to enhance any impression of worthiness.

At any rate I devised the selection task as a game to exploit the grasp of truth functions in formal logic. And the function of the conditional turned out to be very rich indeed. Perhaps a sketch of the task will remind readers of it.

Here are four cards:



You know that there is a letter on one side of a card, and a number on the other side. You are then presented with the following claim about these cards: "If there is a vowel on one side of a card, then there is an even number on the other side." Which cards is it necessary to turn over to find out whether my claim is true or false?

That is the basic task although the tokens are, of course, arbitrary. You of course answer: "The A and the 7", guided by your insight that an odd number on the other side of the vowel would falsify precisely in the same way as a vowel on the other side of the odd number. But the vast majority of our intelligent subjects do *not* select the odd number; they select the vowel and the even number, or just the vowel, and often they are very reluctant to change their choice. Sometimes they claim that two identical cards differ in meaning. The selected card has a falsifying status; the unselected card lacks it. Even more strange, they will admit the falsifying status of an unselected card but dismiss it as 'irrelevant'. These defensive strategies are all the more peculiar because of the simple structure of the task.

And it isn't as if the decisions involved are invested with any strong affect, but are concerned with letters, numbers and diagrams which are hardly calculated to arouse passions (see Wason, 1977).

A second line of defence is to maintain that the task is so difficult that it almost defies human wit. I first noticed this in Leo Apostel's 'Negation Group' in 1972 when somebody argued that the solution required a number of 'rotations' concerned with the 'inside' and 'outside' of the cards.

Similarly, Finocchiro (1980) claimed that the logical structure of the task is so complex that it takes pages of symbolic logic to understand it. I think that obscurantist attempts to re-define the task make it into an interesting kind of intellectual projection test.

We argue, on the contrary, that it is not logical complexity that makes the task difficult but deep-seated tendencies towards verification. Put simply, success depends solely on selecting those cards which could falsify the test sentence (Johnson-Laird & Wason, 1970).

What seemed like a break through, involved making the cards not arbitrary tokens, but instances which cohered with

the sentence. Success was achieved when each card represented a journey, with the destination on one side and the mode of transport on the other side (Wason & Shapiro, 1971). But a large number of embarrasing studies failed to replicate our findings. (Unfortunately the original study found its way into a large number of text books).

A more radical experiment achieved a dramatic success when the cards did not represent anything, but were actual constituents (envelopes) in a postal sorting situation (Johnson-Laird, Legrenzi & Legrenzi, 1973). However, some very interesting subsequent work showed that only subjects who had experienced the postal regulation involved in the task, tended to be successful.

It was in the 1980s and 1990s that the selection task was used as a versatile tool rather than a *sui generis* puzzle. The main theoretical ideas which have exploited the selection task are: social contract theory, pragmatic schemas, mental models, mental logic, deontic logic, relevance theory (Wason, 1995).

A quite different mode of development concerns neither the material, nor the structure, but the sources of information available to the subject. In the standard selection task there are four such sources classified (according to logic) as P, ¬P, Q, ¬Q. When I was working with Phil Johnson-Laird in the 1970s, it seemed to me that values of the antecedent (P and ¬P) were superfluous to the cognitive work involved. Nearly everyone selects P and rejects ¬P). Why not eliminate them completely? The new binary task contains no additives; it merely deliniates the essential machinery.

Suppose that the claim is "all the triangles are black". Only the inspection of *black* shapes or *white* shapes is available to the subject over *a series of trials*. The ideal performance would be to request all the white shapes to ensure that they are not triangles: the claim can then be deemed true: no black shapes are informative. The experimenter can provide a strong hint by presenting a *black circle*, when a black object is requested, rather than a triangle.

The initial study (Johnson-Laird and Wason, 1970) showed that *all* subjects eventually exhausted the store of white shapes, and abandoned the search for black shapes.

And they did it with a knowing smile which represented a 'Aha' experience.

Interestingly enough, this study was ignored for over a decade. It was resuscitated by Wason and Green (1983) who repeated the main results over a series of four experiments, and explored some other highly pertinent variables. We called it the 'RAST' (reduced array selection task). The standard task is very difficult, but the RAST is such that most people err initially and then correct as a function of feed-back. In addition, the magnitude of the stores of shapes can be manipulated from unity to infinity. The RAST can also be performed without any stimuli at all, and with (or without) a checklist to obviate memory. Indeed the RAST is so simple that it can be used to study the thinking of children as young as seven (Girotto et al., 1985).

Of course, some of my friendly critics, and some of my unfriendly ones, deplore my dedication to the selection task, and the way I have fostered it in others, as being extremely narrow. They do have a point, especially when my work may have blocked other approaches. And yet the selection task seems to mirror, in a quite graphic way, social phenomena which stultify clear thinking, e.g., prejudice, resistance to change, intellectual fixation, and the Freudian mechanisms of defence.

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The study of deductive reasoning: 1969-1996

Philip N. Johnson-Laird

A generation ago, when Ino Flores d'Arcais and Pim Levelt organized the Bressanone conference, the psychology of deductive reasoning was an immature discipline almost entirely bereft of theory. I was likewise an immature researcher, just two years out of graduate school, and the Bressanone conference was a 'first' for me in many ways. It was the first time I had ever visited Italy – a country with which my wife and I fell in love, and which we have visited nearly every year ever since. It was the first time, I met my future friends and colleagues Paolo Legrenzi and Maria Sonino Legrenzi with whom I have continued to collaborate ever since. This friendship presaged other collaborations with other Italian friends, including Bruno Bara, Monica Bucciarelli, Vittorio Girotto, and Patrizia Tabossi. And it was the first time, I met Ino Flores d'Arcais, that extraordinary one-man European Intellectual Union, and his friend, the editor of this volume, an equally distinguished figure.

With what now seems an unimaginable temerity, I submitted two papers to the conference. One was on Peter Wason's selection task – a task in which subjects selected the evidence they thought relevant to testing the truth or falsity of a conditional rule – and the conference foreshadowed its subsequent importance, because Paolo Legrenzi also dealt with it in his paper, and Peter Wason himself contributed a written commentary on it in the published proceedings. My other paper was on the interpretation of quantifiers – words such as *all*, *some*, and *none* – a topic that continues to be studied by psychologists (see e.g. Rips, 1994).

In what follows, I will sketch what has happened in the psychology of reasoning since the days when we met in Bressanone to discuss these matters, and continued our discussions as we toured the Dolomites and took the cable car to the top of the Marmalada. In 1969, the study of reasoning mainly concerned what causes people to make mistakes.

Psychologists accordingly investigated performance, and neglected competence, perhaps because they had tacitly assumed that it was based on an unconscious logical calculus.

In 1996, the principal change in the field is the existence of several theories of deductive competence (for a scholarly review, see Evans, Newstead & Byrne, 1993). There are three main sorts:

1. Theories based on formal rules of inference akin to those of a logical calculus. Dan Osherson (1974-'76) pioneered such an account, I dabbled with such a theory, Martin Braine and his colleagues carried the idea further (Braine & O'Brien, 1991), and Lance Rips (1994) has developed a theory that accommodates both quantifiers and sentential connectives, such as *if*, *and*, and *or*.
2. Theories based on rules with a specific content. This idea entered artificial intelligence when investigators realized that searches for formal derivations were intractable, and it has led to various systems based on conditional rules (see e.g., Newell 1990). One variant is Cheng and Holyoak's (1985) theory of 'pragmatic reasoning schemas', i.e. conditional rules with a general content concerning what is possible or permissible. Another variant is the theory that reasoning is based on a memory for specific examples analogous to the case in hand – so-called 'case based' reasoning (see Kolodner, 1993).
3. Theories based on mental models. Wason's selection task convinced many investigators that reasoning did not depend on formal rules, because the content of the task had a pronounced effect on performance. In my paper on the selection task, I hinted that 'subjects are probably unable to refer to any coherent or systematic truth table' (Johnson-Laird, 1970a), and Osherson (1974-'76) established the point more rigorously. There seemed to be an impasse: reasoning depended on neither syntax (formal rules) nor semantics (truth tables). The way out was the mental model theory, which I will describe in a moment.

The controversy among the three theories has largely filled the literature since the early 1980s. Unlike some controversies in psychology, it has been fruitful, though some sceptics argue that it will never be settled. Certainly, the main protagonists are unlikely to be converted to their opponents' views – it was ever thus in scientific controversies – but recent work should resolve matters to the satisfaction of everyone else. If

not, then cognitive psychology is likely to pass into oblivion as a hopeless enterprise. Meanwhile, let me outline the mental model theory, and the possible resolution of the controversy.

The mental model theory assumes that people reason on the basis of their understanding of premises, which leads to the construction of models of the situation under discussion (Johnson-Laird & Byrne, 1991). One clue to the form of mental models is (as I found to my surprise in re-reading my 1970a paper): "subjects have a more immediate access to those contingencies that render [an assertion] true than to those that render it false". This assumption lies at the heart of the model theory. Thus, a conjunction, such as 'There is a king and there is an ace', calls for a single model of the true contingency:

king ace

An exclusive disjunction, such as 'There is a king or else there is an ace, but not both', calls for two models of the two possibly true contingencies:

king
ace

where the separate models are shown on separate lines. The principle of representing only what is true applies to individual models: the first of these two models makes explicit only that there is a king, not that it is false in this case that there is an ace; likewise, the second of the two models makes explicit only that there is an ace, not that it is false that there is a king. In other words, people represent an exclusive disjunction merely by listing all the possibilities when the first disjunct is true followed by all the possibilities when the second disjunct is true. A conditional, such as, 'if there is a king, then there is an ace', is represented by an explicit model of the contingency in which its antecedent and consequent are true, and by a wholly implicit model (shown here as an ellipsis) of the case, or cases, where its antecedent is false:

king ace
. . .

The model theory extends to quantifiers – the topic of my other paper at Bressanone (Johnson-Laird, 1970b), though my remarks were distinguished by their lack of semantic prescience. They anticipate some syntactic analyses of quantifiers, but fail completely as a psychological account of how people realize that an assertion, such as:

Not all philosophers understand everything

can express the same thought as the assertion:

Some philosophers do not understanding something.

I would now argue that the principles that people have for constructing models yield equivalent models for these two assertions (see Johnson- Laird, Byrne, & Tabossi, 1989; Bara, Bucciarelli, & Johnson-Laird, 1995).

So much for the model theory; now for the resolution of the contoversy among the three sorts of theory. The first step is simple. Content-specific rules or memories cannot explain general deductive competence, because people can reason about abstract and unfamiliar matters. We are left with just two contestants: formal rules and mental models. The following robust phenomenon is likely to settle the issue.

Suppose that only one of the following two assertions is true about a particular hand of cards:

If there is a king in the hand then there is an ace in the hand.

If there isn't a king in the hand then there is an ace in the hand.

And suppose it is definitely true that:

There is a king in the hand.

What conclusion would you draw? The answer seems obvious: there is an ace in the hand. That's how everyone responds to this problem. But they are wrong. In fact, it is impossible for there for there to be an ace in the hand. This is one example

of many so-called 'illusory inferences'. Their existence was predicted by the model theory, and Fabien Savary and I have shown that they do occur (see Johnson-Laird & Savary, in press). They are distinguished by three properties: nearly everyone draws the illusory conclusion; it seems obvious; but it is an egregious error. Indeed, the illusions are so powerful that even when you understand the answer, it is hard to believe it. Earlier, I pointed out that an exclusive disjunction is interpreted as a list of all the possibilities when the first disjunct is true followed by all the possibilities when the second disjunct is true. Granted that if only one of two assertions is true, they are in an exclusive disjunction, the model theory accordingly predicts that the disjunction above is interpreted to yield the models:

king ace
¬king ace
. . .

where ¬ signifies that there is not a king. The definite assertion that there is a king eliminates all but the first model, from which it seems to follow that there is an ace. What has gone wrong, and creates the illusion, is the failure to represent the false contingencies. In particular, if the first conditional is true, the second conditional is false, i.e. there is no king and no ace; similarly, if the second conditional is true, the first conditional is false, i.e. there is a king but no ace. Either way, there is no ace, and so the conclusion is invalid.

Illusory inferences are relatively rare, and to find them, we use a computer program (based on the model theory) that searches through hundreds of inferences.

Here is one other example. Only one of the following two assertions is true about a particular hand of cards:

If there is a king in the hand then there is an ace in the hand.

There is an ace in the hand.

and it is definitely true that:

There is a king in the hand.

Again, nearly everyone infers that there is an ace in the hand; but, again – as readers may determine for themselves – this conclusion is an illusion. The premises are contradictory, and so any conclusion whatsoever can be drawn, including both the illusory conclusion and its negation.

Illusory inferences should resolve the theoretical controversy. The model theory predicts them, but they refute the rule theories. The reason is transparent. These theories use only valid formal rules of inference, and so they cannot explain invalid deductions that nearly everyone makes. As Niels Bohr remarked, it is hard to make predictions, especially about the future. But, putting prudence to one side – to honour Ino by the breach of one of his most perspicuous traits – I predict that illusory inferences (of which the selection task is just a special case) will resolve the controversy about how people reason.

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Reasoning on Bressanone

Paolo Legrenzi

In my academic career the Bressanone Conference was important for at least two reasons. I had occasion to meet Phil Johnson-Laird and to be invited to work with him and Peter Wason, beginning a long collaboration, mentioned by Evans and Newstead (1995, p. 5): "... a new generation of Italian reasoning researchers (such as Vittorio Girotto, Bruno Bara and Pat Tabossi) emerged from the Legrenzi connection". In 1992, when I was invited, together with Phil Johnson-Laird (twenty years after my first visit) to University College London, the Italian 'connection' was so developed that Phil called us 'the Italian embassy'.

The other reason for which I am grateful to Ino Flores d'Arcais and Pim Levelt, the organizers of the conference and the editors of the proceedings, is that they made it possible for me to present my work on the selection task. It was the first contribution to the literature on the task since its invention by Peter Wason, and since then: "this problem, known most commonly and simply as the selection task, became the single most investigated problem in the history of psychology of reasoning and there is no sign as yet that interest in it is abating" (Evans & Newstead, 1995, p. 4).

In my Bressanone contribution, there were three main ideas that have been developed in the nearly thirty years of subsequent research on the selection task. The first idea concerns 'checking for cheaters'. My account of the task was as follows: "...in a typical selection task, subjects are presented with the sentence "If there is a vowel on one side of a card, then there is an even number on the other side of the card", together with four cards, each of which has a letter on one side and a number on the other side ... the subject's task is to select all those cards, and only those cards, which would need to be turned over in order to discover whether the experimenter was lying in making the conditional statement." Wason had observed that subjects tend to err by failing to select the card bearing the odd number; clearly, if such a card has a vowel on its other side, it falsifies the conditional rule. The idea of 'looking for cheater', hinted at by my description of the task as a search for 'lies', was the starting-point for the growing evidence

that instructions to search for violators facilitates performance in the selection task (see Garnham & Oakhill, 1994, pp. 140-141).

The second idea in my presentation, later analyzed in several papers, was the influence of the linguistic formulation of the rule on the subjects' selections. Two interesting variations, first introduced in my Bressanone paper, were the following:

1. Implicit vs. explicit negation. When the 'q' card of Wason's original rule: "If there is a vowel (p) on one side then there is an even number (q) on the other side", was labeled 'not odd', a majority of the subjects selected the odd card (corresponding to the logical value not-q). This result anticipated the effect that Evans and Linch (1973) studied systematically, and that they called 'matching bias'. The same result was also obtained by using the rule: "No card has a vowel on one side, unless it has an even number on the other side". In this case, 23 out of 30 subjects selected the A and the 2 cards, again perhaps as a result the 'matching bias'. As Evans later observed (1989, p. 57): "when the rule is presented in this general form – If p then not q – most subjects choose the matching cards p and q and thus apparently solve the problem.

2. The relevance of double negation for focusing on the logical choices. In the Bressanone paper, I carried out an evaluation task and a selection task using the rule: "It is not possible for a card to have two borders and not have a circle in the centre", together with four cards (with one or two borders, surrounding a circle a triangle). Most subjects "indicated that the only card which falsified the rule was the one with two borders surrounding a triangle – subjects do appreciate that the doubly negated rule excludes only one type of card" (see p. 327). In a subsequent selection task with the same type of rules, most subjects selected the cards corresponding to the logical choices.

This result was recently replicated by Sperber, Cara, and Girotto (1996). According to them, success in the selection task depends on the interpretation of the conditional rule as a denial of the existence of counterexamples to the rule, i.e. there does not exist [p & not-q]. This condition is easily obtained by a deontic version of the task, but the deontic context is not necessary. It is possible to obtain the right selection even with the doubly negated expression that I introduced into the task. Such expressions are used in everyday life in order to deny explicitly the existence of counterexamples to a rule (for a review of the problem, see Girotto, 1995, p. 337).

The third idea in my Bressanone presentation, much studied in subsequent years, concerned the experimental context in which a conditional rule is evaluated (for a review, see Griggs, 1995). I argued that a conditional is more likely to be taken to imply its converse in what I called a 'strictly binary' situation, that is, a situation in which the subject realizes that the antecedent of the conditional refers to one of only two possible events, and that the consequent refers to one of only two possible events. I demonstrated this point by using such conditionals as:

If the ball rolls to the left, then the red light comes on

in a situation where the ball could roll either to the left or right, and the light was either red or green. This procedure was adopted in many studies of the interpretation of conditionals (for a review see Johnson- Laird & Byrne, 1991, p. 46), and the mental model theory accounts for the phenomenon of different interpretations of conditionals in different contexts. In the initial models of the conditional:

ball-left red-light
. . .

the possibilities are not exaustively represented (the three dots denote a model with no explicit content). The strictly binary situation is likely to trigger people to flesh out the conditional as a bi-conditional (i.e. material equivalence), and calls for both the antecedent and the consequent to be exausted (if and only if the ball rolls to the left, then the red light comes on):

[ball-left] [red-light]
. . .

Indeed, reasoners may construct completely explicit models:

ball-left red-light
ball-right green-light

My impression, in reading again the paper presented at Bressanone, is that the variations of the selection task that I used were perhaps more interesting than the better known 'post office' paper which, two years later, initiated the deontic tradition (for a

review of this tradition, see Evans, Newstead & Byrne, 1993, pp. 118-135). My versions of the task improved performance in completely abstract contexts, using neutral material. Today, it is just such versions of the selection task that are the most challenging for mental model theory and relevance theory. Perhaps the title of the book, *Advances in psycholinguistics*, rendered my contribution (one of few on the psychology of reasoning in the book) less visible. Nevertheless, the two factors that I studied (the linguistic expression of the rule and the context in which the rule was checked) have become one of the focal points in the current reasoning debate (Legrenzi, Girotto, Johnson-Laird, 1993).

Since Bressanone, the sun has always shone on the selection task. I remain always grateful to Ino Flores d'Arcais for introducing me to the selection task and for his wise advice over the years.

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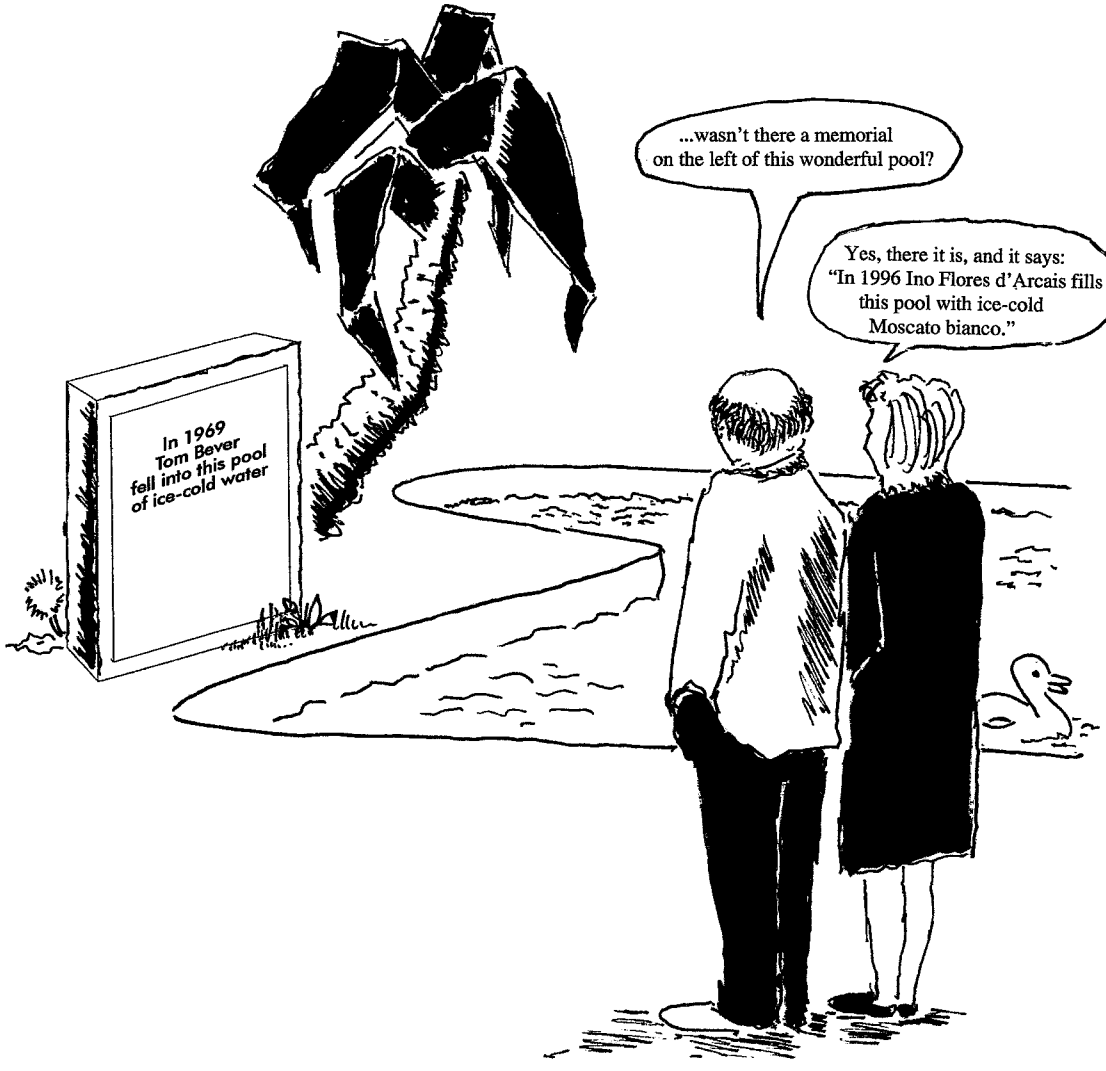
COGNITIVE IMPAIRMENT



...a right brain damaged patient with perceptual spatial hemineglect and a deep dyslexic on a holiday trip in Bressanone...

Raynor's revolution

John C. Marshall

I joined Carolus Oldfield's MRC Psycholinguistics Unit in the mid-sixties. Shortly after arriving in Oxford, I was telephoned by Freda Newcombe. We did not know each other, but I knew she was studying cognitive impairments in brain-damaged ex-servicemen and she knew that I had worked on (normal) reading when a graduate student.

Freda had a patient (George Raynor), who she said, read *bun* as 'cake', *satirical* as 'Jupiter', and *industry* as 'factory'. I enquired whether these responses were made when G.R. was reading text – John Morton, who had been a friend since our days together in the Psychology Department at Reading (sic), had noticed such errors when normal subjects read aloud quickly (Morton, 1961). This was not the case, said Freda. G.R. made these semantic errors when reading individual words on plain white cards, with no time pressure, and under no doubt that the task was indeed reading (not free association).

I informed Freda that such a pattern of performance was impossible (and that she must therefore be mistaken!). The best established theory of reading asserted that print was (necessarily) converted into phonology (by grapheme-phoneme correspondence rules) prior to lexical look-up within the (modality-free) semantic system. This *phonological* representation was the input to (overt) articulation and could hence provide a medium within which errors of sound (but not meaning) might arise in reading aloud.

And so the arrogance and ignorance of youth dissolved the problem. Freda did not put down the phone in disgust (which would have been the most appropriate reaction), but rather inquired whether I would like to examine G.R. myself. There's nothing quite like data for causing a change of mind. G.R. *did* make very substantial numbers of semantic errors in reading and he showed a fascinating part-of-speech effect: Nouns were better read than adjectives or verbs, which in turn were easier than function-words (*witch* → 'witch'; *which* → 'No, don't know'). Context (e.g., reading sentences) did not seem to help:

Going down the river there was a coal barge
→ 'Row ... Thames ... coal ship'

Would you like a glass of sherry?
→ 'Wine, sherry ... one ... one sherry'.

The paper was written, and accepted by *Neuropsychologia* without major changes (Marshall & Newcombe, 1966). But few people seemed to find the case of any particular interest. Exceptions to this state-of-affairs included John Morton (who began rejigging the logogen model) and André Roch Lecours (who kindly sent me some similar results from a French-speaking patient he had seen).

We carried on testing G.R. By the time that Ino and Pim had gathered everyone together in Bressanone (July 1969), we had extended the picture: G.R. showed a massive concreteness effect: He was far better at reading concrete than abstract nouns, a finding that later workers argued was an 'imageability' effect. (I might agree if I understood what they meant by imageability other than a series of marks made on a seven point imageability scale). We also corrected a descriptive mistake. Earlier, we had called errors such as *arrive* → 'arrival' visual completions. The claim was false. Rather, G.R. was displaying errors of derivational (and inflectional) morphology (see Patterson, 1980). And we discovered a tendency for G.R. to preserve markedness when reading (erroneously) adjectives. He was, for instance, more likely to read *little* as 'small' rather than 'big'.

I cannot remember whether these new data (or indeed our prior results) aroused any interest in Bressanone (Marshall, Newcombe & Marshall, 1970). Indeed, I can remember very little of the meeting ... just three things. The weather was magnificent, the company was argumentative, and (most important of all) the wine was spectacular. No-one could ever forget Ino's organization of expeditions to *all* the best local wineries. Is my memory true or false when I recall Tom Bever falling into the swimming pool on our return from one particularly good outing?

A year after the Bressanone meeting, Freda and I had formulated a fuller account of the normal reading system (a two-route model), along with predictions about how it could break down after brain-damage. We illustrated our new taxonomy of the acquired dyslexias with G.R. (who now became a 'deep dyslexic'), J.C. (who was extensively examined by Jane Holmes and became a

'surface dyslexic') and A.T. (who became a 'visual dyslexic'). The paper ("Patterns of Paralexia") was rejected by more journals than I care to remember. All the editors liked it, you understand, but all rejected it (often without benefit of referees) as 'unsuitable for our journal'. The neurology journals found it too linguistic, the linguistic journals too psychological, and the psychological journals too neurological.

The fourth draft was presented by Freda and myself at the *International Neuropsychology Symposium* at Engelberg (in Switzerland) in June 1971. Again, my recollection is that the participants were polite about the work but not overly enthusiastic. The highlight of the meeting was when Jacques Mehler and Athanase Tzavaras broke the bank at the local casino (albeit only a small bank). The desolated croupier was not greatly consoled when Jacques shrugged his shoulders and muttered: "Quand on joue avec un grék et un juif ..."

The paper was eventually published in a then new and unknown journal (Marshall & Newcombe, 1973) and has enjoyed some success (Younger colleagues: Never despair!). But G.R. himself still appeared to be a 'rare and unusual' patient (Younger colleagues: Take no notice of such claims!). So much so that one highly distinguished neurologist continued to claim on ward round that we had invented (or at least encouraged and exaggerated) G.R.'s semantic paralexias.

I mentioned this to Tim Shallice who I met one day in Gower Street in 1974. Tim said that he and Elizabeth Warrington had seen a similar patient whose results they had not published because Freda and I had beaten them to it. Not quite on bended knees, but very fervently nonetheless, I begged Tim to write up K.F. with Elizabeth. Which they did (Shallice & Warrington, 1975).

Subsequently, cases of 'deep dyslexia' have been reported in all the world's major languages (Coltheart, Patterson and Marshall, 1987) and the phenomenon continues to inspire both further clinical studies (e.g., Cossu, da Prati & Marshall, 1995; de Bleser, Faiss & Schwarz, 1995) and serious computational modelling (Plaut & Shallice, 1994). G.R. became a classic case (Barry, 1996). I just hope that we examiners gave George some fraction of the rewards he gave us. And that Ino and Pim will organize 'Next summer in Bressanone'.

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Two separate systems for space cognition

Luigi Pizzamiglio, Cecilia Guariglia,
Daniele Nico and Alessandro Padovani

At the time of the Bressanone meeting the interest of the senior author dealt with the identification of different components of the linguistic competence in aphasic patients. Since then he investigated other domains of cognitive impairments in brain damaged patients. The present paper is an attempt to identify in man separate processings in spatial cognition.

Hemi-neglect patients show specific impairments in the perception of actual stimuli presented in the extrapersonal or in the personal hemispace contralateral to the side of the lesion. This visuo-explorative deficit may be associated to a similar deficit in visual imagery in about half of the cases of right brain damaged patients with perceptual spatial hemi-neglect (Bartolomeo, D'Erme & Gainotti, 1994). In a classic paper, Bisiach and Luzzatti (1978) described two patients with a perceptual neglect, who failed to describe 'by memory' a well known square (Piazza del Duomo in Milan) of their city as it would appear from a given perspective: the meaningful buildings on the right side of the square were always accurately described, while the ones on the left side were constantly omitted.

Two cases with a double dissociation in the perceptual and imaginal spatial domains are described and implications for a theory of spatial representation in man are discussed.

The first patient (B.M.) is a man of 51 years with 5 years of education, studied 4 months after a right temporo-parietal CVA. B.M. exhibits a severe visuo-spatial hemi-neglect in all clinical tests (see table 1); in some tasks requiring the generation and manipulation of visual imagery such as the O'Clock Test (Grossi, Modaferri, Pelosi, et al., 1989) and the Cloud-Like Test (Bisiach, Luzzatti & Perani, 1979), a general difficulty was observed (i.e. patient's performances were below the normal range) but no asymmetry (i.e. worst performance on the side contralateral to the lesion) was detectable.

B.M. was also extensively tested in the square description by imagery: he always described successfully several familiar squares

of Rome from different perspectives, rarely omitting details both on the right and the left side. When required to describe, from memory, the objects' location in a previously unknown room, to which he familiarised for half an hour moving around, the same absence of asymmetry in description was observed (see table 1): the list of objects and their location in the room produced during the description from memory was accurate and symmetrical, even if in the description in the perceptual condition all the elements on the left side were omitted.

In summary, this patient shows a severe neglect in the perceptual domain, and no asymmetry at all in describing by memory squares or complex scenes to which he has been familiarised.

The second case (M.C.), recently described (Guariglia, Padovani, Pantano, et al., 1993), identified an isolated hemispatial imagery disorder in absence of any perceptual impairment.

M.C. is a 63 years old male with a right frontal lobe CVA, studied 18 months after the onset. His performances in a large variety of visuo-spatial abilities were within normal limits and no sign of visuo-spatial or motor neglect was found (see table_1). However, an imaginal neglect was clearly evidenced whenever the patient described by imagery either a very familiar square or a room, to which he was previously exposed for a long time. Nevertheless, performances on other imagery tests were either within normal range, demonstrating a normal ability to generate visual images both from short and long term memory, or failed in showing clear asymmetries on the left side of mental images of objects.

In summary, this second case represents the first description of a neglect confined to a specific form of visual imagery: M.C. behaved normally and symmetrically when he had to generate images of well known objects, both from a working memory buffer (Cloud-Like test) or from the long term memory (O'Clock test), but he failed to describe the left side of a complex imagined scene from a particular vantage point.

How can we account for the striking difference between the two patients? One possible conclusion drawn from the reported dissociation between neglect for the left side of external images, but not for the left side of internal (mental) images, and neglect for the left side of mental images, but not for the left side of external percepts, might imply that "different functional systems may be responsible for attending the two different kind of images" (Anderson, 1993).

Table 1 – *The two patients' results are shown on a standard battery of tests for evaluating visuo-spatial neglect on a set of tasks for evaluating mental imagery and imaginal neglect. The number of correct responses over the maximum score is reported.*

	B.M.		M.C.	
test	left	right	left	right
visuo-spatial neglect				
1. Line cancellation	0/11	9/10	11/11	10/10
2. Letter cancellation	0/53	19/51	53/53	51/51
3. Wundt-Jastrow	0/20	20/20	20/20	20/20
4. Sentence reading	6/6		6/6	
mental imagery				
5. Colour comparison	15/15		14/15	
6. Weight comparison	10/12		11/12	
imaginal neglect				
7. Shape comparison	3/40	9/40	34/40	35/40
8. Familiar square description	15	13	5	23
9. Room description	12	12	5	9

However, this interpretation would not account for the dissociation in imagery observed in M.C., which is confined to tasks requiring the patient to reconstruct by imagery a complex space from a specified vantage point.

In order to better understand this latter imagery dissociation an analysis of the cognitive requirements of different imagery tasks is necessary.

When subjects are to maintain in a working memory two

nonsense shapes (Cloud-Like test), the mental image of the two clocks' showing two different hours, or also the map of a country, they can make reference to distinctive 'cues' (i.e. convex/linear traits, well known spatial location of the hours in the clock face, cardinal points, etc.) that may help them in 'reading' the imagined picture. In general, they can refer to a world-, body-, and retinal framework which codes a particular event in space.

By contrast, when the subjects are required to describe a space such as a square, they may start from a particularly salient item with a particular relation to their imagined position; from that place they 'navigate', direct their exploration to successive points of the same scene, continuously changing the overall organisation. To do so subjects need:

a. a good representation of the global properties of the environment;

b. to move the exploration, coding in each moment the relative position of their body, according to information concerning the direction of their 'movements'.

The latter information might be related to vestibular inputs in a real situation or to locomotion memory activations of these information in imagery.

The existence of a navigational system has been documented by O'Keefe (1991) and Muller, Kubie, Bostock et al. (1991), who have shown that there are two different classes of cells involved in space processing: a. 'shape cells' in the hippocampus, that are sensitive to the overall geometric properties of the environment, and b. 'move cells' in the pre-subiculum, that are activated whenever the animal "points to a particular direction relative to the environmental frame". Furthermore, the move cells "may have vestibular inputs that enable them to update constantly their estimate of the current heading direction on the basis of the animal's movements" (O'Keefe, 1994).

We suggest that the imaginal neglect observed in tasks like the description of well known squares or familiar places may rely on the disruption of a navigational system which is independent from the more explored neural system which deals with an environmentally coded framework.

A partial support of this view comes from two independent studies (Geminiani & Bottini, 1992; Rode & Perenin, 1994) showing that the performance of neglect patients in describing well known squares improves after vestibular caloric stimulation.

However, it must be noted that the same vestibular stimulation improves other performances in neglect patients.

Finally, a 'navigation' ability was shown to develop independently from the capability to integrate several visual and body-centred representations in guiding the child's movement in the environment (Hermer & Spelke, 1994).

As for the neural substrate, while the parietal structures have been shown to integrate retinal and extra-retinal information, lesions either in the fronto-limbic system or in any part of the cortical areas involved with the processing of vestibular inputs or their use in organising memory-guided responses (supplementary parietal eye field and prefrontal eye field, cf. Pierrot-Deseilligny, Israël, Berthoz et al., 1993; Jonides, Smith, Koeppe et al., 1993) may deal with the navigational task in vision and in imagery.

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