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## FOREWORD

The editors of this issue, Teresa Vanderweide, Sandra Kitch, and Lorna V. Rowsell, are pleased to present the sixteenth issue of the *Calgary Working Papers in Linguistics* published by the Department of Linguistics at the University of Calgary. The papers published here represent works in progress and as such should not be considered in any way final or definitive.

Most of the contributions in this issue were taken from a panel discussion at the *Annual Conference on Language (ACOL)*, held in Banff, Alberta, November 7, 1992. The topic discussed was "Research Paradigms and Linguistic Research". Dr. John Archibald opens the panel with his paper *Research Paradigms and Linguistic Research*. Here he discusses "theory-driven" versus "theory-free" data collection, and concludes that there is a place for theory-driven data collection in language acquisition studies. Dr. Bruce Derwing's paper *Theory and Research in Phonology* discusses the dangers of conducting scientific research without an open mind. Dr. Michael Dobrovolsky gives us seven points on *How we do Science - or do we?* Dr. Eithne Guilfoyle presents *Syntactic Theory and Linguistic Research* focussing on Specific Language Impairment (SLI) research in children. Finally, Dr. Gary Prideaux offers *Two Research Paradigms for Discourse Analysis*, specifically sociolinguistic and cognitive discourse analysis.

Three additional papers from undergraduate and graduate students at the University of Calgary are also presented in this issue. Audra Crowe and Jackie Dolson explore the phonetic implications of stuttering in their paper *Properties of Stuttered Speech*. Sean Fulop argues for the existence of a fortis/lenis contrast in his paper *Acoustic Correlates of the fortis/lenis Contrast in Swiss German Plosives*. Lastly, Teresa Vanderweide offers a response to Clahsen's Parameter Constraints in her paper *A Functional Category Analysis of the German Acquisition Data*.

We apologize for the delay of this issue, but promise to continue working towards putting out the next edition of *CWPL* by December, 1994. Papers on all topics in Linguistics are still gladly accepted for further editions of *CWPL*.

The editors wish to thank all the contributors, and especially Vi Lake for devoting so much time, skill, and patience in the preparation of these papers for printing. We would also like to thank all those who helped with proof-reading and other tasks.

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## CALL FOR PAPERS

*Calgary Working Papers in Linguistics* is an annual journal which includes papers by faculty and students in Linguistics and related disciplines, both at the University of Calgary and elsewhere.

The editors would like to encourage all readers to submit papers for future publication. The deadline for submission of papers is August, 30 in order to meet an Autumn publication date. The editors would like contributions on 3 1/2 inch Micro Floppy Disks (preferably Macintosh Apple and Microsoft Word format). We furthermore request that the submissions follow the APA style sheet, use 10x10 point *Times* font, and leave a 2.056" top- and bottom-margin, as well as a 1.75" margin on both the left and the right hand side. In those rare circumstances in which the contributor does not have access to a computer, the editors will accept two typed copies of the paper. Those wishing to submit papers should send manuscripts to the address given below. Postage costs should be included if the disk containing the manuscript or the paper copy are to be returned. Appearance of papers in this volume does not preclude their publication in another form elsewhere.

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# RESEARCH PARADIGMS AND LINGUISTIC RESEARCH<sup>1</sup>

John Archibald  
University of Calgary

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*In the year sixteen hundred and nine  
Science's light began to shine.  
At Padua City, in a modest house  
Galileo Galilei set out to prove  
The sun is still, the earth is on the move.*

Bertolt Brecht, *The Life of Galileo*

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The following papers (Derwing, Dobrovolsky, Guilfoyle, and Prideaux) began as a panel discussion at the annual Alberta Conference on Language (ACOL) held in Banff on November 7th, 1992<sup>2</sup>. The general theme of the panel was *Research Paradigms and Linguistic Research*, and the original panel consisted of Bruce Derwing (U of A), John Archibald (U of C), John Ohala (U of A; Berkley), Eithne Guilfoyle (U of C), Gary Prideaux (U of A), and Michael Dobrovolsky (U of C). Each original talk was about fifteen minutes long.

In this paper, I would like to combine the very brief opening remarks that I made, as well as my contribution: research paradigms and language acquisition research.

## 1.0 OPENING REMARKS

I would like to begin by making some very general comments on the relationship between theory and research, and then give a few examples from fields other than linguistics.

In the philosophy or sociology of science, this question is usually addressed under the heading of *the construction of scientific knowledge*, or the question of *theory-driven* versus *theory-free* data collection. We will note that the question of whether theory-free data collection is desirable, or even possible, arises in the panel discussion following.

Throughout the history of science, we see examples of every possible type of interaction between scientific theory and research practice. Examples usually trotted out in favour of theory-

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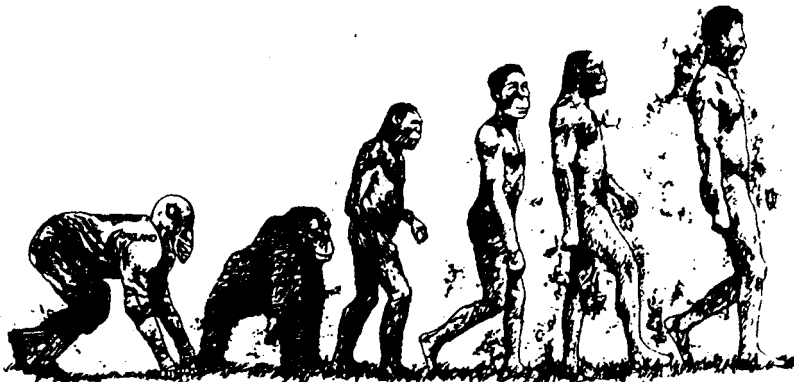
<sup>1</sup> I would like to thank Gary Libben for inviting me to organize this panel. I would also like to thank all of the original panel members for participating.

<sup>2</sup> Some attempt has been made to alter the style of a talk into that of a written paper, but the overall impression will remain informal.

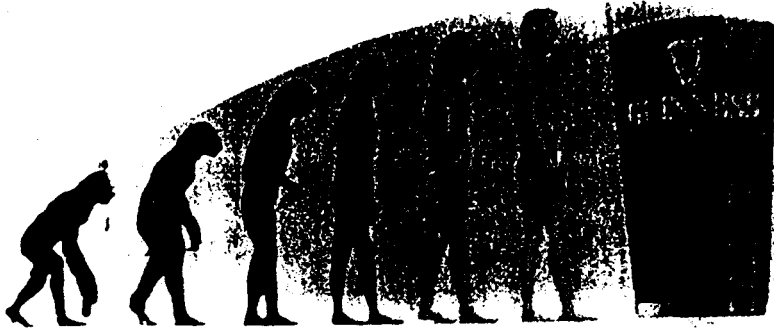
driven data collection are cases where theory predicted certain phenomena which were then searched for and found. Voilà! The vindication of theory. Familiar examples of this are Mendel predicting the existence of a physical structure called a gene; Stephen Hawking predicting the existence of Black Holes; or the discovery of a particular sub-atomic particle. Sometimes the theory requires an independent empirical discovery before it becomes a feasible theory. This was the case with the theory of Continental Drift. It appeared to reconcile a number of diverse observations, but there also appeared to be no way that it could possibly happen. How could things as massive as continents move? Then the theory of Plate Tectonics was proposed and voilà! A mechanism for Continental Drift.

In the historical archives, we also see examples of the experimental paradigm triumphing over pure theory. The arguments that took place between Boyle and Hobbes over Boyle's air-pump experiments were consistently won by Boyle. Hobbes felt that such experiments as Boyle was conducting could not provide evidence that was relevant in any way to natural philosophy. Clearly, though, Boyle's experimental paradigm has influenced modern science greatly.

The final example I would like to present shows, I think, a much subtler relationship between theory and research. Considering our location today (in the Rocky Mountains), it is fitting that it concerns the analysis of the soft-bodied fossils found in the Burgess Shale (just down the road). A man named Charles Walcott was responsible for much of the original measurement and classification of the fossils in the early part of this century. Practically every measurement he made confirmed the predictions of the dominant view of evolution at the time. He made his measurements fit into existing categories. Jay Gould refers to this a *Walcott's Shoehorn*. His measurement and classification was biased by the dominant view of the progression of evolution: the cone of diversity. This view can be illustrated with the following pictures:







There are three major assumptions made here (beyond those made about football players and Guinness):

- (1) evolution proceeds by simple forms becoming complex (progress)
- (2) there was less diversity early in time than later
- (3) evolution proceeds gradually

By not questioning these three assumptions, Walcott's measurements supported the dominant view. Later in the 20th century, other paleontologists re-examined Walcott's work and the fossils of the Burgess Shale and radically revamped our view of life and evolutionary progress. They made the following claims:

- (1) diverse forms were present early; they did not necessarily arise from simpler ones
- (2) many of these forms were decimated
- (3) change is not necessarily gradual<sup>3</sup>; stasis is data

The point that I would like to draw from this fascinating study is that Walcott felt that he was engaged in theoretically-neutral activity; he thought he was *just measuring*. And yet, later re-analyses showed that his measurements were not neutral.

I would like to conclude this section with Geoff Pullum's view on the whole question. He says:

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<sup>3</sup> Referred to by Gould as evolution by jerks.

In this piece I rail against the tendency of linguists to write about the philosophy of science as applied to their subject instead of writing about what languages are like, which is what linguists are supposed to be good at . . . If one found one's Toyota repair mechanic writing analyses of Toyota repair argumentation instead of fixing the damn carburetor trouble, one would naturally and rightly get quite annoyed. And in the actual car repair world this does not happen. But try to keep linguists from philosophizing inexpertly about their craft when they ought to be practising it is like trying to keep a dog from barking at the mailman.

I hope the following papers will show why and how this is a fascinating area for linguists. Maybe we should have put a sign outside the room for Pullum: Beware of the Dog.

## 2.0 THEORY AND RESEARCH IN LANGUAGE ACQUISITION

I would now like to discuss the notions of theory and research in language acquisition (both first and second). There are many similarities in the types of research and the types of controversies that arise in the two fields.

Currently the theory that seems to be getting most of the attention in language acquisition is the Principles and Parameters model. Where the Principles are argued to be innate and hence common to all languages and the Parameters are underspecified rules that are triggered via interaction with the environment. Principles are meant to account for the similarities between all languages; parameters for the differences. This model has succeeded in making language acquisition research an integral part of linguistic theory by placing on the grammar that linguists propose the requirement that it be feasible or learnable. The "learnability" paradigm takes as its starting point an account of adult linguistic competence (usually framed within a GB model) and then attempts to account for how the learner acquires this system of knowledge. This can be contrasted with the traditional research paradigm in language acquisition that owes more to a Piagetian tradition than a Chomskyan. Figure #1 gives an astonishingly simplified account of these two schools.

### Piagetians

- more concerned with developmental path & stages
- non-modular minds

/      \  
L1      L2  
•Slobin    •Andersen  
•Roeper   •Flynn

### Chomskians

- more concerned with initial & final states
- modular minds

/      \  
L1      L2  
•Wexler    •White  
•Brown    •Krashen

Figure 1: Piagetians and Chomskyan Schools of Thought

## 2.1 Theory Construction in Language Acquisition

So researchers differ in the phenomena they are trying to explain and in the assumptions they make regarding the mind. They also differ in their techniques of theory construction. Acquisition researchers fall into two camps on the matter of theory-driven versus theory-free data collection.

The first camp is what Rod Ellis (1986) calls the “Research-then-Theory” school of thought. By “research”, I think he means “data collection”. Researchers in this paradigm would argue that their data collection is theory-free. Data is collected (relevant to a particular phenomenon) and then the researcher attempts to construct a theory to explain the patterns found in the data. I have heard this viewpoint expressed by many a doctoral student in education when I asked what their thesis was about: they would answer, “I don’t know, I’m still collecting my data.” This is, of course, the method that has been referred to as “butterfly collecting” by an anonymous cynic. It has, however, been argued that when an academic discipline is in its infancy, then this *is* a useful manner, indeed the only manner, in which to proceed. But that once a critical amount of data has been collected, then a theory will be developed to account for the data. From then on, data-collection should be theory-driven.

This second stage, or second camp, is what Ellis calls the “Theory-then-Research” school of thought. Researchers in this paradigm draw freely on a particular theory and choose research questions which have the potential of informing the theory. Let me illustrate this school

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<sup>4</sup> Come to think of it, these arguments can apply equally to First Language Acquisition Research.

of thought with the following example from second language research:

## 2.2 Adult Access to UG

One of the questions that is attracting much attention in the SLA research field<sup>4</sup> is whether adults have “access to UG”. What this research seeks to determine is whether the interlanguage grammars of L2 learners are governed by the same principles as primary languages. In other words, do interlanguages violate proposed universals? This is, of course, an often controversial pastime as the arguments as to whether a particular feature of a language is universal or not, are heated and frequent. Person A will claim that certain L2 learners violate principle X or parameter Y. Then person B comes along and argues either:

- (1) that X isn't a principle, or
- (2) that Y isn't a parameter, or
- (3) that the violation is to be expected because A got the markedness facts wrong

At a time when linguistic theory (any model) is so fluid, the problems of applying this theory are myriad. This is particularly true of the *learnability* approach to acquisition. Rather than focussing on the developmental stages in a theory-neutral fashion, the researchers in this tradition attempt to account for the acquisition of linguistic structures claimed to be part of adult competence. For example, we see L1 acquisition articles on such things as:

- Acquisition of binding principles
- Children's knowledge of locality conditions
- The development of long-distance anaphora

And L2 acquisition articles on such things as:

- Parameter setting and the acquisition of word order in L2 French
- Minimal sonority distance and SLA
- SLA and governing categories
- Bounding nodes and subadjacency violations

And, of course, people argue about the linguistic descriptions of these phenomena. But I don't feel that the controversy over the formulation of the theory is a Bad Thing. We have to accept a complex model of *what* is being acquired before we can talk about *how* it is being acquired. And, as in other scientific fields, it takes time, argument, and controversy to establish consensus. It can take a very long time, as I noted this week, that the Catholic church has just pardoned Galileo.

### 3.0 HIERARCHICAL REDUCTIONISM

I think that one thing that characterizes all types of applied research is the adoption of a principle of *Hierarchical Reductionism*. This is a principle adopted by many other branches of science. It assumes that one researcher is incapable of doing everything; some things have to be assumed. A particular researcher may be investigating the physical properties of ball-bearings in collision. It is unnecessary for that researcher to necessarily conduct basic research in:

- quantum mechanics
- soft-bodied objects
- string theory
- calculus
- probability theory

A particular piece of research can fill one part of a puzzle. My own work concerns the acquisition of stress in second language learners. Related questions might well be such things as:

- how is stress physically implemented?
- how is stress acoustically manifested?
- what is the theory of stress assignment?
- is it an aspect of lexical representation or lexical processing?
- is it the same at the word, phrase, sentence, text level?
- how do morphology, syntax, discourse, and phonetics influence stress?
- is it the same for L1 learners?
- etc.

I would argue that it is reasonable, and indeed necessary, to exclude certain types of questions from a particular research design. Hence the term *reductionism*. It is *hierarchical* in that a researcher is entitled to accept the research that has been done on either a higher or lower level from his or her own. So, I do not *need* to address philosophical questions like whether we

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<sup>5</sup> Thanks to Kevin Gregg for the example.

can know anything anyway, or physiological questions like the role of ion transfer in muscle contraction. It *may* become relevant but not necessarily.

The researcher is entitled to make certain assumptions. In many other disciplines, different perspectives on the same phenomenon are valued. We can find out interesting but different things about stomach troubles caused by overindulgence in alcohol from molecular biologists, and sociologists.<sup>5</sup> People working on modelling cognition are entitled to accept research that was done of neuronal structure.

Work in language acquisition has often been criticised for this type of reductionism. I feel it is unjust. Yes, linguistic theory and language acquisition studies must interact. And it is not just that acquisition studies act as tests of particular linguistic theories. Within whatever framework we write our grammars, we want those grammars to be *learnable*; to be *feasible*. Acquisition requirements are now an integral constraint on what *linguists'* grammars can look like (they are obviously a constraint on what *learners'* grammars can look like).

#### 4.0 CONCLUSION

In conclusion, I do feel that theory-driven data collection has a place in language acquisition studies. As I mentioned in my opening remarks, we have to be wary that our theory does not lead us to use, for example, a UG Shoehorn. All science is conducted within a social context. The myth of impartial scientific observation remains only in 1950's movies with sterile scientists in white coats. I would like to close with the words of Jay Gould:

We often think, naively, that missing data are the primary impediments to intellectual progress - just find the right facts and all problems will dissipate. But barriers are often deeper and more abstract in thought. We must have access to the right metaphor, not only to the requisite information. Revolutionary thinkers are not, primarily, gatherers of facts, but weavers of new intellectual structures.

The theory is a light that can blind or illuminate.

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*Sixteen hundred and thirty-three till  
Sixteen hundred forty-two  
Galileo Galilei remains a prisoner of the Church  
Up to the day of his death.*

Bertolt Brecht, *The Life of Galileo*

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# THEORY AND RESEARCH IN PHONOLOGY: A QUESTION OF ALTERNATIVES

Bruce L. Derwing  
University of Alberta

## 1.0 BACKGROUND

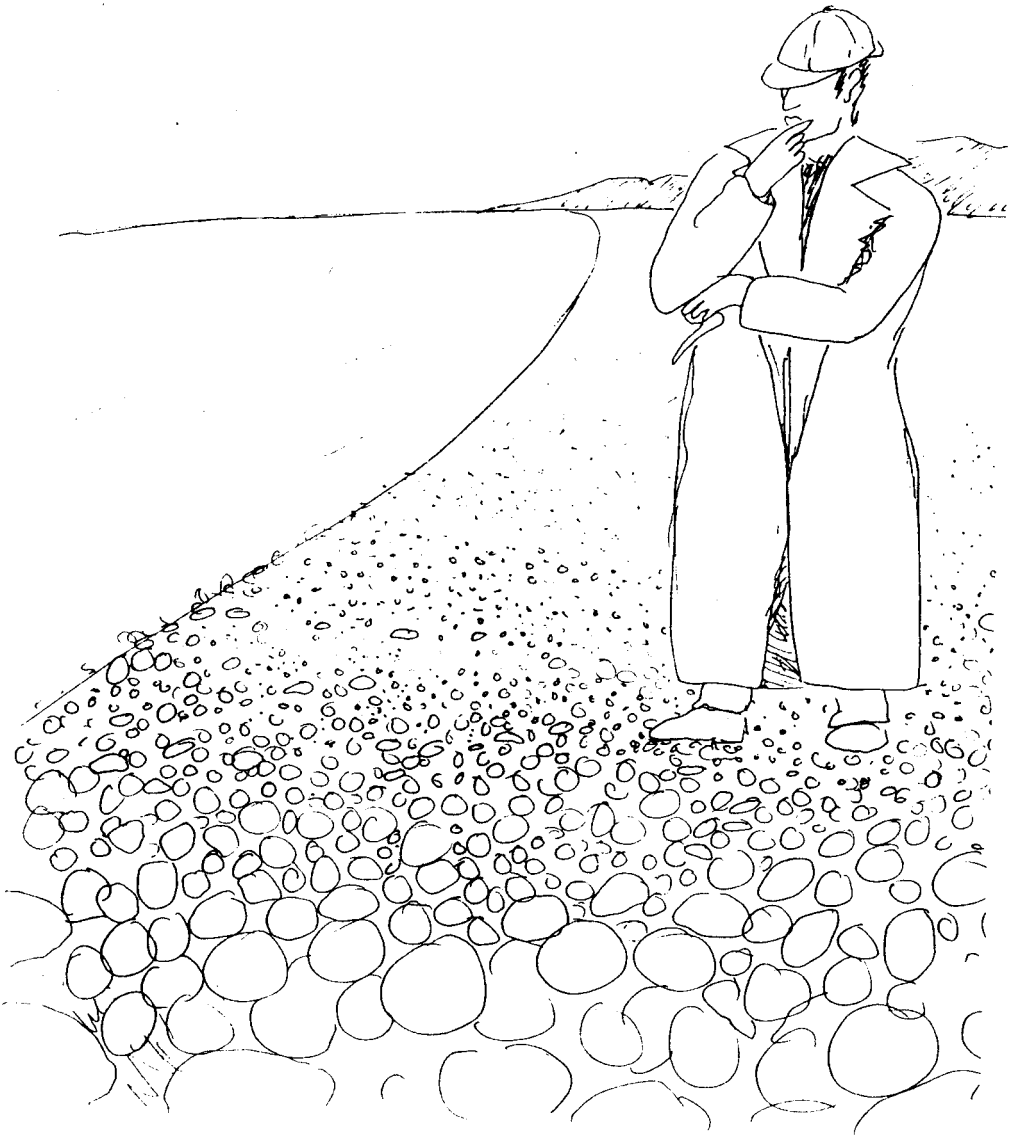
I've asked to go first because I wanted to open this discussion with some very *general* remarks about the relation between theory and research. In fact, what I propose to share with you is what I consider to be the *most important lesson* I've learned about research. I learned it many years ago, and oddly enough I didn't learn it from a linguist, philosopher, or scholar of any official strip. Actually, I learned it from a guy named Sherlock Holmes.

### 1.1 On Objectivity

Chroniclers of the world's first and greatest "scientific" detective", as Holmes is often described, typically make much of the fact that a chief factor in his remarkable *skill* as a criminal investigator was his unique ability to keep an open mind and to let the facts, in a sense, "speak for themselves". There is much justice in this conclusion, too, as Holmes himself repeatedly warned against pre-judging the "singular" aspects of any particular case and spoke frequently of the importance of approaching each new case "with an absolutely blank mind", so that he might be freed from prejudice "to observe and draw inferences from [his] observations". One humorist has countered this overly simplistic account, however, with the cartoon you see, which shows the great detective standing on a pebbled beach, considering the stones that lie around him: apparently there are zillions of them.

### 1.2 Holmes' Strategy #1

Clearly, there is another side to this story, and this can be readily extracted from Holmes' own expressed views. Thus, alongside the citations that I have already read to you, we also read that another and evidently equally fundamental aspect of Holmes' extraordinary success as an investigator was his great knowledge of the history of crime," since he found that there was "a strong family resemblance about misdeeds, and if you have all the details of a thousand at your finger ends [sic], it is odd if you can't unravel the thousand and first. (pp. 13-14)



Portrait of a Celebrated Detective Regretting his Rash Decision to Leave No Stone Unturned

In other words, for all his railing about the importance of *keeping an open mind*, Holmes nonetheless (and not too surprisingly) relied heavily in his work on a kind of preconceived framework in which he could make sense of his myriad past observations and conclusions: in other words, there can be no doubt that one important element in Holmes' approach to criminal investigation was that he had used and greatly benefited from a theory.

### 1.3 Holmes' Strategy #2

A second chief cornerstone of Holmes' methodology, however (judging by the frequency with which it appears in Watson's memoirs) was the following principle: "It is a capital offence [Holmes insists] to theorize in advance of the facts" (Doyle 8:770) because, as he says elsewhere, "It biases the judgment" (p. 18). "Insensibly [he says] one begins to twist facts to suit theories, instead of theories to suit facts" (p.179). In fact, Holmes concludes, "The temptation to form premature theories upon insufficient data is the *bane* of our *profession*" (p.915).

## 2.0 FROM HOLMES TO SCIENCE

To move now from "the science of deduction" to science in general, we find the same kind of conflict at work. It is universally accepted that theories are not only important, but that they provide an absolutely essential guide to inquiry that would otherwise quickly degenerate into random hunting and gathering of endless disordered and disconnected facts, much like Holmes searching under every pebble on that beach. To my mind, this conclusion has never been seriously challenged, nor do I wish to challenge it here.

### 2.1 Theory as guide vs theory as blinder: the methodological dilemma

On the other hand, however, just as we need theory as a *guide* to investigation, it is equally important to recognize the role that theory plays as a *blinder* to it, just as Holmes warned. We seem, therefore, to be faced with a *methodological dilemma* of sorts. On the one hand, theory-free data collection is at best a monstrously inefficient and essentially unproductive enterprise (if not an outright contradiction in terms). Yet, on the other hand, we are still always faced with that insidious natural tendency that we all seem to possess as human beings to see only what we want or expect to see. It is as though with too *poor* an idea of what we are looking for, we never find it, but with too *good* an idea, we *inevitably* find it - whether it is really there or not!

## 2.2 From Holmes to Feyerabend

Once again, however, it is the redoubtable Mr. Holmes who provides us with a way out of this apparent quandary. For him, the lesson to be learned was simply this: "[N]ever lose sight of the alternative" (p. 664). For the truly eloquent statement of this position, however, let me read to you an extended excerpt from an essay by the philosopher Feyerabend entitled, "How to be a good empiricist — a plea for tolerance in matters epistemological" which appeared in 1968:

You can be a good empiricist *only* [he says] if you are prepared to work with many alternative theories rather than with a single point of view. This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an essential feature of *all* knowledge that claims to be objective. Nor can one reset content with a plurality which is merely *abstract* and which is created by denying now this and now that component of the dominant point of view. Alternatives must be developed in such detail that problems already considered 'solved' by the accepted theory can again be treated in a new and perhaps also more detailed manner. Such development will of course take time... It takes time to build a good theory...; and it also takes time to develop an alternative to a good theory. The function of such *concrete* alternatives is, however, this: They provide means of criticizing the accepted theory in a manner which goes *beyond* the criticism provided by comparison of that theory 'with the facts' [and therefore] allows for a much sharper criticism of accepted ideas. (pp. 14-15)

On the important issue of the so-called "relative autonomy of facts", i.e. whether or not facts even exist and are available independently of some particular theoretical position, Feyerabend notes that most discussions of theory validation would indeed appear to present a model in which "a single theory is compared with a class of facts (or observation statements) which are assumed to be a 'given' somehow" (p. 27). This is however, as Feyerabend puts it much too simple a picture of the actual situation. Facts and theories are much more intimately connected than is admitted by the autonomy principle. Not only is the description of every single fact dependent on some theory (which may, of course, be very different from the theory to be tested), there are also facts which cannot [even] be *unearthed* except with the help of alternatives to the theory to be tested, and which become unavailable as soon as such alternatives are excluded. (p. 27)

Feyerabend then proceeds to set up a scenario in which some investigator has decided to adopt a particular theory in his field and has *refused* to consider alternatives. [It is not at all surprising that a person should do this, either, for, after all, as Feyerabend also points out, "[you] can do only so many things at a time and it is better when you pursue a theory in which you are interested rather than a theory you find boring."] He further assumes that the pursuit of the particular theory which this investigator has selected has led to a reasonable degree of success, in that it has served to explain a number of circumstances which had hitherto been regarded as unintelligible.

This gives *empirical support* to an idea which to start with seemed to possess only this advantage: it was interesting and intriguing. The concentration upon the theory will not be reinforced, the attitude towards alternatives will become less tolerant. Now if it is true, as has been argued, that many facts become available only with the help of such alternatives, then the refusal to consider them will result in *the elimination of potentially refuting facts*. More especially, it will eliminate facts whose discovery would show the complete and irreparable inadequacy of the theory. Such facts having been made inaccessible, the theory will appear to be *free from blemish* and it will seem that 'all evidence points with merciless definiteness [to the essential correctness of the accepted theory].' This will further reinforce the belief in the uniqueness of the current theory and in the complete futility of any account that proceeds in a different manner, ... popular science books will spread the basic postulates of the theory; applications will be made in distant fields. More than ever the theory will appear to possess tremendous empirical support. The chances for the consideration of alternatives are now very slight *indeed*. The final success of the fundamental assumptions [of the theory] ... will seem to be assured. At the same time it is evident...that this *appearance of success* cannot in the least be regarded as a sign of truth and correspondence with nature. Quite the contrary, the suspicion arises that the absence of major difficulties is a result of the *decrease of empirical content* brought about by the elimination of alternatives, and of facts that can be discovered with the help of these alternatives only... such a system will of course be very 'successful' not, however, because it agrees so well with the facts, but because no facts have been specified that would constitute a test and because some facts have been removed. Its success [in other words] is *entirely man-made*. It was decided to stick to some ideas and the result was, quite naturally, the *survival* of these ideas. If now the initial decision is forgotten, or made only implicitly, then the survival itself will seem to constitute independent support, it will reinforce the decision, or turn it into an explicit one, and in this way close the circle. This is how empirical 'evidence' may be *created* by a procedure which quotes as its justification the very *same* evidence it produced in the first place. At this point an 'empirical' theory of the kind described...becomes almost indistinguishable from a myth. (pp. 30-31).

I know this is correct, because I have experienced the effect myself. Interestingly, later on in the same book where I first laid out these arguments over twenty years ago, the following statement also appeared (p. 197), in connection with the phenomenon of e-epenthesis in Spanish: "It is a *blatant contradiction* [I wrote] to speak both of Initial Epenthesis as a *rule* and of morpheme-initial /e/ as a *lexical* representation."

Now what in the world possessed me to have ever written such a ridiculous thing as that? It was very simple, really. I was raised on and totally imbued with classical generative phonology, and all the presuppositional baggage that came with it. Thus, even in a book that was largely written as a criticism of the theory, I still had only one concept in mind as to what the *function* of a rule in a phonological system might be. This was the function it had within that generative

framework: to express lexical redundancies, and thus to *simplify* the lexicon. At the time, therefore, I could not conceive of any alternative view, because I had not yet *looked* for one. Soon, I found one, of course (e.g. Venneman's idea that the function of rules is to express generalizations *about* the lexicon, not to *supply redundancies* left out), and at that point the absurdity of my own statement became blatantly apparent.

To give just one other quick example, in later work I became heavily involved in a series of experiments over several years, designed to answer the question, "which rule or rules" are learned to account for the regular or phonologically conditioned variation that we see in connection with such cases as the English plural and past tense inflections. And though my studies seemed quite sensible and logical at the time (indeed they attempted to systematically weed out a half dozen or so *alternative theoretical accounts* on the basis of *critical experimental tests*), they were all at the same time predicated on the unchallenged underlying assumption that 'rules' of a familiar kind were involved. Today, however, we can see that even that notion is no longer sacrosanct, as alternative models have been developed both along connectionist and analogical lines that strive to account for linguistic productivity without resort to rules at all. As a result, a good part of this past work of mine has suddenly become quite irrelevant and the focus of the effort has been forced up on an entirely new plane of activity altogether. Once again, I'd been "caught with my theories down" so to speak.

### 3.0 CONCLUSION

My embarrassing conclusion is that even though thoroughly forewarned of the dangers before I started, I nonetheless got sucked in: I have myself fallen into the trap of premature commitment to an overly narrow theoretical conception on at least two big occasions. To help reduce the chance of that happening again, I now refer often to a card that encapsulates the essence of the problem in a few brief words: "Those who have an excessive faith in their own ideas are not well fitted to make discoveries." An open mind is required to do science, and for a mind truly to be open, it must make a serious commitment to more than one theoretical position. It's just that simple — and just that hard. Unless we, as individual researchers are *each* willing to work *comfortably* with at least two *incompatible* theories, the odds are that we're just fooling ourselves. I know *I* was.

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## HOW WE DO SCIENCE—OR DO WE? SEVEN POINTS.\*

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### 1. WE HAVE KNOWN HOW TO DO SCIENCE FOR THOUSANDS OF YEARS

I begin with this point because I believe we sometimes operate under the myth that science as we know it began in the Renaissance. There is no doubt that some elements of what we call Western science emerged from the cultural practices of that period. But even the most stripped down definition of scientific activity leads to the conclusion that the scientific approach to understanding the universe is ancient.

As a starting point, I take Russell's straightforward observation: '... the test of scientific truth is patient collection of facts, combined with bold guessing as to laws binding the facts together.' Stated in less eloquent contemporary terms, I would say that science is the search for the fundamental elements that make up the universe and for the principles that govern those elements in their combination. We break down observationally unitary activities (speech, matter, personality) into separate and irreducible elements and then ascertain what allows those elements to combine and interact. The scientific approach therefore assumes that the observed complexity of the universe arises from the principled interaction of primitive elements. It follows that these elements are not always perceived directly, and that the principles that govern them may be quite abstract. The scientific frame of mind thus assumes that apparently discrete appearances are in reality complex combinations, and that there is no simple causal relationship among the parts.

Consider as an example of this approach to understanding the universe an early theory of personality. In this theory, the wide variety of observable personality types was viewed as emerging from the interaction of what I will call 'basic type' and 'character traits'. The basic types can be represented by a circular array, and the character traits by individual glyphs that are placed in various positions and combinations on the basic array. Since there are  $n$  basic types and  $p$  character traits that can be placed around the basic array in virtually any spot or grouping, the result is a theory that represents and predicts a wide variety of personalities differing in many small degrees. This contemporary-sounding approach to analyzing and defining personality is Babylonian

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\* Thanks go to John Archibald for organizing the panel discussion on scientific practice in linguistics at the 1992 Alberta Conference on Language. This working paper is a writeup of my remarks on that occasion. Professor Archibald also called to my attention Kuhn 1989.

(Chaldean) astrology; its beginnings date from approximately 4,000 BC.

Babylonian astrology thus structured its theory of personality as a modern science would. Obviously, the theory of causal relations between basic types and season of birth and between character traits and the position of stars and planets in the heavens has not been supported by experimental research in the intervening six thousand years; any observed congruences or predictions based on the method are chance phenomena (see, for example, Gauquelin 1979, who does, however, make claims about other types of birth-related correlations). This fact is independent of the overall sophistication of the theoretical approach, and is not in my estimation a trivial one. Human thinking was not in its infancy at the time this theory emerged. More importantly, the association of planetary influences with variable character traits, which may have followed from observed correlations between seasonal changes and/or lunar phases and behavior, allows for a sophisticated view of the subtle variability of personality.

It matters little to the point I am making here that the predictive power of the theory in its haruspical form is by modern standards inadequate, showing only chance levels of success. I am not saying that how we do science has not improved. I do believe that the central scientific notion of describing natural phenomena in terms of the interaction of elements and principles has a long history and is still essential to how we think scientifically today. Diverse and perverse applications of the methodology were as common then as now—power to a priestly class, money-making activities, misinformation, credulity, and the like; all are associated with contemporary scientific practice as well.

It is also the case that the history of science as we practise it is the convergence of theory structure with the practice of experimentation—that observation and practice also form part of the human scientific tradition. What we sometimes dismissively call technology existed successfully for many millenia. This was the ‘experimentation’ of the past. It could not have been carried out without some level of theorizing about the properties of elements involved. Humans sailed for millenia before the complex and subtle physics behind sailing was ‘understood’. But theory and practice were united by simultaneous modelling of the phenomena in application.

It has been suggested (Ohala, this forum) that what defines science is its openness, the public forums of debate, criticism, and counterproposal. But surely this is true of the arts as well, and we do not hallow them with the label ‘scientific’. Furthermore, openness, public forums of debate, criticism, and counterproposal characterized the theological debates of 12th century Paris; I suspect that few of us would characterize the topic of these debates as science. I would be inclined to suspect that the one thing that sets post-Renaissance science apart from earlier forms of inquiry is our insistence on validation—would be inclined, that is, if validation was not such a difficult animal to produce, given our skill at defending our theories by interpreting data in various ways, and given the inordinate role that prestige still plays in allowing theories central stage space (see below).



## 2. THE FALLACY PERSISTS THAT THERE IS A RIGHT WAY TO CARRY OUT SCIENCE

A common criticism of other peoples' research is that it is 'bad science'. Such attacks are frequently couched as negative observations about the way people arrive at their conclusions. What I'll call here the inductive/deductive wars have been carried on for generations in various clothing. Often cited is Raffaello's splendid painting, *The School of Athens*, in which we see Plato and Aristotle striding toward us deep in discussion, the elderly Plato pointing upwards, and a bearded, vigorous Aristotle pointing downward. Rationalism and Empiricism? Nominalism and Realism? Cartesianism and Lockism? Sapir and Bloomfield? Chomsky and Skinner? Whatever form the debate takes, it seems to tilt back and forth across the fulcrum of how we view the world itself: that there are accessible ideal states that underly and determine the messy surface of things, or that there is essentially the surface of things which can be sifted through to discover the way things work, but never some thing itself.

Such radical differences in outlook (so radical and persistent in human thought that I suspect there is some genetic basis for them that is triggered by something like early experiences with large animals or harsh toilet training) manifest themselves inevitably in pronouncements about method. Consider Bloomfield: 'The only useful generalizations about language are inductive generalizations.' (Bloomfield 1933, p. 20). Rationalists (Cartesianists, Realists, Platonists, etc) can be equally firm in their opposing convictions.

Such extreme positions, thought to be valuable heuristics in scientific debate (on which more below), inevitably lead to hardening of the arteries and an insistence that conclusions reached by other approaches must inevitably be wrong because of the way in which they go about doing science. But each of these extreme positions is vitiated by an inherent fallacy.

The inductive fallacy is that there can never be enough data, no matter what we do. Not only can we never be certain, we can never even know with reasonable certainty that the next datum will not completely change the way we view all the other data. The deductive fallacy is that we do not even require data to construct a productive hypothesis.

What we really do is a mix of both, plus flying by the seat of our pants, relying on intuitions—those messages from our subconscious about stuff it has been thinking about in our absence, plus remembering things that someone else said years ago but that we now understand because we have discovered them for ourselves, even tying together motley strands of thought that we have collected and suddenly see in relation to each other thanks to one more piece of thread, be it an item of data (inductive) or another idea (deductive) that pushes us over the threshold of understanding.

What we really do is observe (making use of what we know about the world, our experience), experiment, report, get criticized, and then—only then—begin to wash away the mud and gravel of criticism (personal, institutional, etc) to see if there are any nuggets of genuine insight. And of course that criticism may be our own. We then obtain prestige by showing that we can collect data or construct experiments or have interesting ideas and write them up properly. Once we have collected enough prestige we can begin to play for bigger stakes, to win or lose. There may be wrong ways of doing science, but there is no one right way.

### 3. THERE IS NOTHING WRONG WITH A WEIRD HYPOTHESIS

At this point in our era this observation may be less than startling, but it was not so long ago that whole fields of study that now obtain generous amounts of funding and produce graduate theses were considered unworthy of attention. For those who point up, the work of those who point down is often considered an unproductive waste of time, and vice-versa.

But hypotheses propel science, and they stick with us in metamorphosed and presumably increasingly sophisticated forms. The astrological notion of external influences determining behavior and personality is an early form of the nurture hypothesis, and was not a bad hypothesis for its time. We assume that successive hypothesizing will become informed by experience over time and so not be wasteful re-inventing of the wheel. But the Viconian spiral is a better metaphor for what happens. Various forms of nature/nurture, of mentalism, of behaviorism, have persisted for generations. The appearance of Skinnerian behaviorism was seen as a weird and unwholesome hypothesis (along with other mechanistic trends early in the century) since it threatened time honored notions of mind along with a world view that encompassed notions of soul and spirit. The revival of mentalism in linguistics by Chomsky and its accompanying emphasis on genetic transmission was considered without empirical foundation. Contemporary genetic science gives nativism at least a peg to hang on and renders its initial weirdness less so. (This change of paradigm cannot be too exaggerated. High school psychology courses in the late 1950's nourished students on a strict Behaviorist regimen, dismissing unexplained behavioral phenomena as 'instinct'. Without contemporary genetics, certain properties of behavior appear inexplicable.)

The problem lies not so much in the hypotheses themselves, but that in the social structures manifested by our brains, we place too much emphasis on leaders and followers, centers of attention, grant-getters, winners and losers. The very prestige we must obtain in order to be heard turns into an encumbrance for the field as a whole when some players acquire too much of it. The result is the perpetuation of some hypotheses for the wrong reasons at the expense of others.

Of course, we might say that this is the sociology of science, and it has not kept research from coming up with the right answers over the long haul. The social matrix of our particular

science in the second half of the twentieth century provides a testing ground for humans that has always been part of our societies. The best ideas survive, the poor ones fall by the wayside. But I have suggested above that this is not necessarily the case. The tough, aggressive, and prestigious survive and thrive, but their ideas are not necessarily the best. As Cuppy 1950 notes about Attila, just about all he accomplished was that the people he killed stayed dead. At least science has provided for the resurrection of ignored good ideas through libraries.

Science does not have to live by the rule of the junior high school playground. Diversity is the key to survival in nature. Things have improved in linguistics, but hegemony is hard to break.

#### 4. WE OPERATE WITH WEIRD HYPOTHESES FOR GENERATIONS

Not only is a weird hypothesis something that should not be nipped in the bud, it is often the essence of a discipline's survival.

One of the most striking examples of a persistent and valuable weird hypothesis was the notion of parallel light rays. Unproven and in the strictest sense untestable, the hypothesis made possible the whole technology of Western celestial navigation. Navigation was in a sense its test, which the hypothesis has served well for several hundred years.

There is at least lip service given to the notion of a useful weird hypothesis. A geocentric universe is accepted as having been useful for a while, since we knew no better. The Ether theory helped hold the universe together for a time, and so on. But the history of science as it is actually practised is replete with such heuristics ossifying into dogma defended against all comers, not only by administrators of priestly grants with a vested interest in them, but by the very scientists themselves who should have an open mind and sharp eyes for new weird theories. We often become locked into a Popperian nightmare of defending our hypotheses at whatever the cost because that is supposed to be the way science is done. And so old theories rarely become extinct; rather, new, alternative schools arise while the old ones await their turn for resuscitation.

#### 5. THE CRUCIAL EXPERIMENT IS NOT DEAD

It has become a truism that the notion of a crucial experiment is dead. The crucial experiment can always be re-interpreted, redone, overhauled, confronted with ever new data, and of course, the crucial experiment itself often raises a whole new set of questions. But I think that the ghost of the crucial experiment still hovers.

We operate as if with a hangover of the crucial experimentation fantasy. First, we still rely too much on machines to make decisions. What I call the fallacy of the machine is the belief that we go to a machine to discover some truth about the universe, as if the machine was a flashlight and we were out looking for interesting things in the dark, when in fact we design machines to discover what we are looking for. The machine does not provide the answer, it provides the kind of data we want. This is not to say that there can't be serendipity effects as one discovery leads to another or as a machine is put to novel (or 'weird') uses—VOT studies emerged from spectrography in an unexpected way, for example. But submitting to the authority of machines will not solve our problems until the machines can start coming up with useful hypotheses.

The 'naive falsificationalism' that Popper rightly discounts was described somewhat wickedly by Einstein as 'verification by little effects'.

"It is really strange that human beings are normally deaf to the strongest arguments while they are always inclined to overestimate measuring accuracies." Letter of May 12, 1952 to Born, from *The Born-Einstein Letters*, cited in Feyeraabend, 1975, p. 57.

For Einstein, the interlocking of a theory's various parts and arguments as a whole made the strongest case for the theory. This is often misinterpreted and misused as an argument from 'elegance'.

A second hard lesson we have to learn sooner or later is that self-proclaimed wholly empirical theories are subject to the same conceptual, organizational, and verificational flaws as wholly 'rationalist' arguments. Poor design, weakly collected data, poorly understood statistical measures and their application, and downright misunderstanding of the issues at stake all contribute to the same kind of hall-of-mirrors argumentation among experimentalists as can be heard at any syntax conference. Einstein was right: we understand something when we understand the whole thing.

## 6. REDUCTIONISM IS ONLY VALUABLE WHEN IT IS APPROPRIATE

An inescapable dilemma of modern science arises from our own technological power. We keep reducing our primitives. This is fine as long as there is an end in sight and we are not simply reshuffling the deck. Reductionism is a handy club for any side. It is easy enough to argue that an opponent's categories are meaningless because they can be reduced to lower-level primitives (Ohala 1974). It is just as easy to say that such an approach is mere reductionism but that the link between reductionism and generality is not justified (Pierrehumber 1990).

Hierarchical systems such as nature guarantee that a blind or mechanistic reductionism will not provide us with satisfying answers to scientific questions. In any hierarchical system, units at one level are made up of units from the level below. In being incorporated into the higher level, the immediate lower-level units lose the validating power they had at their own level. Contrastive features keep morphemes apart, but once questions of the meaning and function of each morpheme come into play, the distinctive power of the feature is no longer relevant to the explanation. Explanations of why vowel harmony does not cross word boundaries are inappropriately reduced to phonetic questions though the answer lies in the nature of words and word boundaries themselves and is properly described and explained at that level. *Appropriate reductionism* is the key. It's really not turtles all the way down.

Practitioners of science have known for a long time that we are looking for the best answer, not the absolute truth. And yet we all at times act as if we are still looking for a final truth. Perhaps we need that faith to go looking. But theories are like tools, and each theory does something another theory cannot do. Each theory serves the degree of idealization required.

## 7. SYSTEM, THEORY, AND PARADIGM

Although Newton legitimized the image of the scientist picking up pebbles along the vast beachfront of the universe, we must do more than that (as he did). Without a broader view of what we are doing—how our data and hypotheses fit together with other pieces of our own work, or with what other people are doing—we are indeed just collecting pebbles.

Kuhn 1986 notes that a true scientific definition of gold defines 'gold' precisely as equal to atomic number 79, and not gold's 'superficial' properties such as yellowness and ductility. But of course '79' is meaningless unless it forms part of a coherent theory, a system of elements and principles (primitives and relations)—unless it is 'atomic number 79', with all that the qualifiers imply about a theory of atomic relations. Otherwise it is an arbitrary label. We require a paradigm in which to work ('... a set of recurrent, quasi-standard illustrations of various theories in their conceptual, observational, and instrumental applications. These are the community's paradigm.' [Kuhn 1970]) but we also require a general view of how all the pieces might fit together. Although in fact we work across many paradigms and hypotheses simultaneously, a dominant one in the field as well as dominant counterparadigms, temporary and sub-paradigms, we must hold hypotheses on a larger scale. We shouldn't burrow.

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# SYNTACTIC THEORY AND LINGUISTIC RESEARCH

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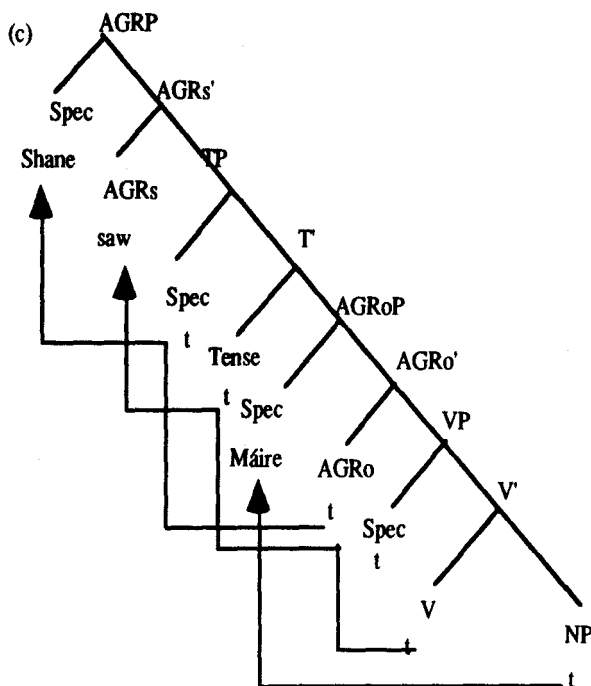
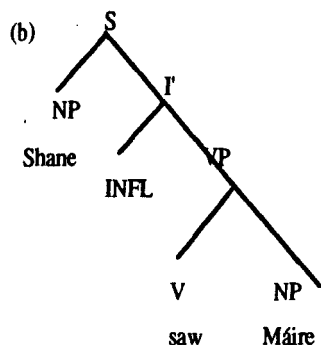
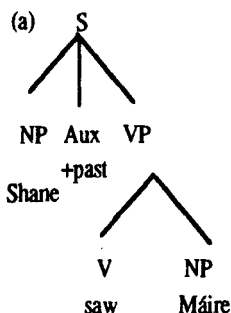
## 1.0 INTRODUCTION

I work on syntax within Government Binding theory, mostly on issues of phrase structure and word-order, and I have a special interest in the phrase-structure of Modern Irish, though I have also done some work on Tagalog, and other verb-initial languages. My second area of interest is the acquisition of syntax, in particular the acquisition of phrase-structure in young children, and I have recently started working with data from young language-disordered children, a population who have received very little consideration from linguistics as a whole, and almost none from those working in a generative framework. In what follows I will discuss a few issues within each of these three areas that most interest me, because they all bear on the central questions of how many syntactic categories there are in natural language, how they are combined, and how children acquire them.

## 2.0 SYNTAX

Up until fairly recently, a very common criticism that has been leveled at generative syntax, is that it is too English oriented; that all languages are treated as if they were some sort of "modified" English and that in an attempt to come up with a "universal" explanation, data from various languages were squashed into an analysis that was devised to account for English. Whatever the truth of those claims in the past, there is much less justification for it now. In the last ten-fifteen years or so, there have been more and more languages studied within the generative framework. This work has led to some remarkable confirmation of certain aspects of the theory, and the rethinking of other aspects. For example one of the more influential Ph.D theses to appear in the Eighties was that of Jim Huang on the syntax of Chinese (Huang 1982) which provided independent confirmation for Logical Form as a distinct level of syntax. More recently, the work of Mark Baker (1988), Peggy Speas (1990), among many others, has led us to rethink our view of phrase structure, and the relationship between morphology and syntax. Without getting into too many technical details, many of the inflectional morphemes that were formally represented in the lexicon are now viewed as having their own syntactic projections, and are classified as Functional Categories (as opposed to the lexical categories, N, V, P and A).

Thus the structure of a syntactic tree has "grown" so that what started out as the structures shown in (a) in the late Seventies, developed to that shown in (b) by the early Eighties, and now many researchers are working with a model such as that shown in (c).



Obviously a tree like (c) has many more layers of structure than either that of (a) or (b). Note the presence of the functional projections AGRs Tense, AGRo, each with its own specifier, and complement. Note also the number of movements that take place to derive the “simple” sentence “Shane saw Máire”. This leads us to the troubling issue of economy: is (c) in any sense more “economical” given that this tree can account for word order in many more languages than the other two?

How do we know what is the most economical theory of clause structure? The one with fewest movements, the one with the least structure, the one that can account for the most languages, or the most learnable. This is an old issue that has recurred again and again in many guises, and is still very much with us (Chomsky 1990, 1992).



### 3.0 NORMAL ACQUISITION: CONTINUITY vs MATURATION

Many theorists (e.g. Hyams 1987, Pinker 1984) working in the framework of Generative Grammar have assumed the "Continuity Hypothesis". Under this view language acquisition is made up of a series of continuous stages. The child moves from one stage to another, and at each stage the grammar posited by the child is determined by Universal Grammar. The motivation for change from one stage to another comes from a trigger in the language environment which causes the child to restructure her grammar, and so move on to the next stage. The Continuity Hypothesis has provided an explanation for the acquisition of many linguistic structures; however, in many instances it has been difficult to explain exactly which data in the language environment act as a trigger, and why they have an effect on the child's grammar.

Recently Borer & Wexler (1987), and Felix (1984, 1988) have proposed that this movement from one stage to another is driven by 'maturational' factors rather than by environmental triggers. As the child matures physically, so do the principles which make up the grammar. When a new principle emerges the child reorganizes the grammar in accordance with the new principle. The Maturational Hypothesis has been criticized for being non-explanatory, as, in theory, any principle can mature.

In recent work Radford (1990) and Guilfoyle & Noonan (1988) suggest a more restrictive form of maturation - one that applies to phrase structure only - but does not affect other aspects of UG. We proposed that in the early stages of acquisition, only lexical categories (V, N, A and P) are present, and that functional categories emerge according to a maturational schedule. However, principles of UG are present from the earliest stages, and the grammar will never violate any principle that applies to the existing structure. Thus the child's grammar is "smaller" than the adult grammar in predictable ways. This hypothesis is compatible with much that is already known about the earlier stages of language acquisition and provides a systematic explanation for the telegraphic property of early child language. The phenomenon of telegraphic speech in early child language arises from the fact that early child grammars are based on lexical grammar, as opposed to adult language which consists of a thematic base, namely Lexical Grammar, built on a functional skeleton (Functional Grammar). Of course, this idea has been disputed, and it is certainly the case that there is a fair amount of cross-linguistic variation in the age at which the FCs emerge (as evidenced by the age at which movement and inflectional morphology first emerges). Some of this may be determined by such notions as saliency of the functional heads in the language being learned. So that for example, the stress-bearing inflectional morphemes of Italian tend to emerge earlier in child speech than the nonstress-bearing inflectional morphemes of English, because Italian children get clearer evidence for these functional heads than do English children. The role of saliency becomes particularly relevant when we look at SLI (Specific Language Impairment).

#### 4.0 SPECIFIC LANGUAGE IMPAIRMENT

Specific Language Impairment (SLI) is a developmental language disorder which has been studied primarily by psychologists and specialists in communication disorders, but until recently received comparatively little attention from linguists. The diagnosis of SLI is primarily one of exclusion. The individual must exhibit a developmental language disorder, and at the same time have no history of hearing impairment, neurological damage, autism, mental retardation, social or emotional deprivation or any other condition that is known to be associated with language impairment. Non-verbal IQ scores must be within the normal range. SLI individuals exhibit the following broad characteristics:

- a. Physically and cognitively normal yet have significant problems with language in the areas of morphology, phonology, syntax, semantics or pragmatics.
- b. Onset of language is considerably later than in the normal developing child. In some cases the first words may not emerge until age four or later, and the child may be unintelligible to non-family members until much later than this.
- c. Once under way, the process of language acquisition proceeds slowly. To date there has been disagreement as to whether the SLI speech is delayed or deviant.
- d. The condition usually improves over time, although for many (even most) individuals the deficit remains into adulthood.

In addition to these characteristics, it should be pointed out that significantly more males than females are affected (3 boys to 1 girl), and individuals diagnosed with SLI often have strong family histories of the disorder. These facts, among others, have led some researchers to postulate that there is a genetic component in at least some forms of SLI (Tomblin, 1991), (Gopnik, in press). There is little agreement however, as to whether or not the deficit is purely linguistic, or is the result of a deficit in some other area which happens to have a linguistic consequence. Many SLI children have significant problems in the area of morphology and syntax, but remain relatively unimpaired in other areas of the grammar, and it is this subgroup that is particularly interesting from the point of view of their phrase structure. Among the questions that interest us are the following:

- a. What is the grammar of SLI? What sort of clause-structure do the children work with?
- b. What is the nature of the deficit? Auditory Processing or a real gap in linguistic knowledge (what do you need to make a grammar anyway).
- c. What is the relevance of SLI to a theory of normal language acquisition and to linguistic theory as a whole? Can they help us understand the process of normal language acquisition?

As pointed out by Gopnik (Gopnik, 1990 & Gopnik & Crago in press) the characterization of this disorder as specific to language provides confirmation for the view that language is an autonomous cognitive system. However it also raises the question as to whether or not the grammar of SLI falls within the constraints imposed by UG. This question is in effect a reforming of the much debated

question of whether SLI grammar would be characterized as delayed or deviant (under the assumption that deviant grammars may fall outside the domain of UG).

In a recent paper (Guilfoyle et al, 1991), I argue that the grammar of SLI can only be adequately characterized if it is considered within a coherent theory of syntax and language acquisition. We suggest that the SLI child (like a young normal child), has a "small phrase-structure", and so lacks inflectional morphology and movement. Unlike normal children however, this population is cognitively mature, and therefore can produce long sentences, but like normal children, their grammar changes over time, though much more slowly.

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## TWO RESEARCH PARADIGMS FOR DISCOURSE ANALYSIS<sup>1</sup>

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### 1.0 COMMON ASSUMPTIONS IN DISCOURSE ANALYSIS

Discourse analysis covers a vast range of types of language use, including conversations, monologues of various sorts (lectures, sermons, political speeches), narratives, jokes, and much else in both oral and written modes. It is not surprising, therefore, that quite distinct approaches to discourse have evolved, often with very different research orientations, methodologies, and data sources. In this short contribution, a characterization is offered of two typical research paradigms in discourse analysis. One approach derives primarily from concerns of a sociological and sociolinguistic nature and the other from the perspective of experimental psycholinguistics and cognitive science. The characterization of each includes not only an indication of the major research focus, but also the types of data used, the kinds of questions addressed, the theoretical positions taken, and the overall goals of the research. Finally an indication is offered of the kinds of contributions which can be made by at least one type of discourse analysis to substantial theoretical issues in linguistics.

### 2.0 SOCIOLINGUISTICALLY BASED DISCOURSE

Perhaps the most familiar sort of discourse research is that which focuses primarily on the nature and organization of oral conversations, a tradition represented, for example, in the work of Blakemore (1988), Schiffrrin (1988), Stubbs (1983), Tannen (1984) and others. Conversations typically occupy center stage in this research, along with the strategies which participants employ as they initiate, advance, restructure and terminate their social and linguistic interactions. Given such a focus, it is not surprising that the factors investigated in conversational analysis are those associated with the participants themselves, including their age, sex, social status, relative power, degree of familiarity, and social roles. These factors are found to play significant roles in

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controlling conversations including, for example, the ways in which participants take turns, control conversational topics, guide the thread of developing discourse, change the subject, and even terminate conversations.

A classic example of the sociolinguistics approach to discourse analysis is found in Tannen (1984), in which a Thanksgiving Day dinner is analyzed in considerable detail. Here Tannen taped an extended afternoon dinner party, and while the participants knew that they were being recorded, they were by and large close enough friends that the taping appeared not to bother them. She later transcribed and analyzed the entire session, focussing her attention on such factors as the relative contributions of the participants, the types of turn-taking exhibited, and the ways topics were introduced and controlled. Many aspects of the description depended crucially upon the nature of the individuals' mutual interrelations, which Tannen characterized in her analysis.

The significance of the relative power held by conversational participants is often construed along gender lines (Lakoff, 1975; Kramer, 1978). Fishman (1980), for example, found that women used certain types of hedges five times more often than men, although these were concentrated at points where women unsuccessfully attempted to change the topic. Spender (1980) opines that we have different expectations of male and female language, namely that women speak longer than men, with more instances of tag questions and interruptions. The accuracy of this view has of course often been challenged. For example, Swacker (1975) found that men took on the average five times longer than women to provide oral descriptions of pictures, while O'Barr and Atkins (1980) found, *contra* Lakoff (1975), that the use of hedges, tags, emphatic forms, and "empty" adjectives was governed more by social status and experience than by sex (see also, e.g., Cutler & Scott, 1990; Dubois & Brouch, 1975). Given such results and expectations, it is not surprising to find Leet-Pellegrini (1980), for example, arguing that males tend to dominate and exhibit expertise in conversations, while women gravitate toward supportive roles. Power is revealed within discourse in terms of such factors as who controls topics of conversation and changes in topics, who expresses expertise, who is relatively more assertive, and who assumes a socially dominant role. Such power is manifested strategically by controlling turn-taking, interrupting, and using more assertive language as, for example, the use of direct versus indirect commands.

The discussion above might seem to suggest that conversational analysis has focused primarily on gender studies, but this is emphatically not the case. More recent research dealing with conversational analysis from a sociolinguistic perspective can be found in Duranti and Goodwin (1992) where elaborate attention is paid to the influence of "context" of a linguistic, social, and even cultural nature on the structure and organization of conversations within a variety

of societies. Duranti and Goodwin argue that when the notion of context is extended to encompass linguistic, social, cultural and ritualistic facts, greater attention can be paid to the roles of sociological and cultural factors in the structure of conversation (see, for example, Duranti, 1992; Lindstrom, 1992). Within the same sociological tradition, but differing somewhat from conversational analysis, is the complementary field of the ethnography of speaking (viz. Duranti, 1988), with its emphasis on "communicative competence", that knowledge which conversational participants both need and actually display as they engage in successful communication (Hymes, 1972). Such research places importance on the ways in which speaking and communicating actually contribute to the structuring of society and of people's lives.

In short, sociolinguistically based discourse analysis focuses on the sociologically relevant factors and seeks to determine how they contribute to the structure and control of language use. The theories informing such studies are typically those borrowed from sociology and sociolinguistics, and while ethnomethodology plays an important role here (e.g., Schegloff, 1992), other methodologies are also employed, including controlled experiments and studies of formulaic language (viz. Swacker, 1975; Bauman, 1992). Some ethnomethodologists, maintaining that their orientation is different from both conversational analysis and sociolinguistics, insist on a strong cultural focus in their work (e.g., Duranti, 1988). Nevertheless, the sociological variables and their influences on the form of human linguistic interaction remain primary.

### 3.0 COGNITIVELY BASED DISCOURSE ANALYSIS RESEARCH

In contrast to the sociolinguistically oriented studies of discourse, those deriving from a more "purely linguistic" and psycholinguistic tradition tend to focus on the contributions made to the organization and structure of discourse by cognitive and processing factors, with emphasis also placed on the ways in which functional distinctions are morphosyntactically coded. Within this approach, the *forms* of language are examined to determine the effects of both processing factors and syntactic considerations. Consequently, the psycholinguistic approach to discourse analysis tends to be more familiar to linguists, addressing such matters as the choice of syntactic variants, the use of anaphora, the distribution of given and new information, the effects of markedness and iconicity, and similar issues. Pioneering research in this area can be found in the contributions of Grimes (1975), Chafe (1980; 1982), Clark & Clark (1977), and Longacre (1983), while much of the earlier work is integrated in Brown and Yule (1983).

Within this framework, cognitive psychology, experimental psycholinguistics, formal syntax, and even typological research provide theoretical input. An illustration of such eclecticism is found in the contributions of Hopper (1979) and Hopper and Thompson (1980), who addressed from a typological perspective the issue of how transitivity is used to code foreground information. The role of foregrounding in discourse was further explored by Tomlin (1985) from a psycholinguistic perspective and by Givón (1987) from a typological/descriptive point of view.

Similarly, Prince (1981) offered a detailed analysis of the distribution of given and new information, a theme elaborated in Ariel (1985) and further discussed by Prideaux (in press).

The psycholinguistic approach to discourse analysis also attempts to relate processing factors to the organization of discourse. For example, Smyth (1988) examined the constraints imposed by working memory in discourse, while Prideaux (1991) attempted to relate various aspects of Leech's (1983) "textual rhetoric" to processing considerations. With a similar orientation, Chafe (1987) examined cognitive constraints on the flow of information in discourse. Researchers in this area typically exploit a variety of types of data, including experimentally collected oral and written conversations and narratives, as well as written texts, naturalistic observations, and typological data. Within the cognitive approach to discourse analysis, the data sources and methods are somewhat more controlled than is typically the case in ethnomethodological studies.

In one important sense, then, the sociolinguistic approach to discourse tends to focus on those variables which appear to be largely culturally based and, accordingly, under some degree of deliberate control by speakers and hearers. In contrast, the psycholinguistically oriented studies of discourse tend to seek out those factors which are common for biological reasons and, accordingly, are under less deliberate and conscious control of speakers and hearers.

Clearly, however, the two distinct approaches and research methodologies to discourse analysis sketched above are not necessarily mutually exclusive. It is not uncommon to find some studies addressing both social and cognitive variables. Syntheses are possible, and in fact it might even be argued that such collaborative efforts are crucial in sorting out the relative differences played by social and cognitive variables in the structure and organization of discourse. A short example will illustrate the value of such a "mixed" study.

#### 4.0 THE ROLE OF MARKEDNESS IN DISCOURSE

The distinction between marked and unmarked structures dates back at least to the theoretical insights of the Prague School. When two alternative forms are available, the *unmarked* member tends to be prototypical in structure, to exhibit a wider distribution, and to be more frequent than its *marked* counterpart. Psycholinguistically, the unmarked forms are generally easier to process and they also tend to be acquired earlier (Slobin, 1973).

From a functional perspective, Fox (1987) has suggested that marked structures might serve to delimit thematic boundaries in discourse, a view also supported by Givón (1987). One reason marked structures might be used to signal shifts in thematic structure is that since marked structures are unexpected and violate canonical form, they might inhibit processing, thereby providing a natural place for the hearer to integrate accumulated information into the currently



evolving "mental model" (Johnson-Laird, 1983). It is plausible that marked structures could serve the crucial role of flagging a change in topic, episode, or other thematic shift.

If this hypothesis is correct, the relative frequencies of marked and unmarked structures should differ, depending on their location within an extended discourse. In particular, marked structures should be more frequent at thematic boundaries, while their unmarked analogues should be more frequent within the thematic units.

To test this hypothesis against actual discourse data, Prideaux and Hogan (in press) examined pairs of English structures containing main and subordinate adverbial clauses, such as those in (1) below:

- 1a. Classes were cancelled when it snowed.
- 1b. When it snowed, classes were cancelled.

Instances of the first sort, in which the main clause precedes the subordinate clause (MC+SC), are unmarked structures, while those of the second sort (SC+MC) are marked (Clark & Clark, 1977; Givón, 1987). In an experiment, 40 subjects (20 males and 20 females) watched a short film clip and then narrated the events in the film orally to a friend. The narratives were taped for later transcription and analysis. Several independent judges also segmented the film clip into its constituent episodes. All instances in the oral narratives of sentences containing subordinate adverbial clauses were tabulated in terms of their structure (marked or unmarked) and their locations (episode initial or medial). In a second study using the same stimulus materials, 32 participants (16 males and 16 females) were asked to provide written narratives of the same film, and the same structures were examined in the resulting narratives.

When the data were examined to see whether males and females differed in their use of the targeted structures, either in oral or written forms, no significant differences based on the sex of the narrator were found. Moreover, the oral data revealed no significant differences in the numbers of marked and unmarked structures, although a very strong association was found between the position of the structure in an episode and its markedness. In particular, the unmarked structures tended to occur medially and the marked structures initially, just as the hypothesis predicted.

The written data exhibited the same pattern: no gender differences in the use of the targeted structures, and no significant difference in the overall use of marked versus unmarked structures. Again, however, a significant association emerged between the markedness of a structure and its location, with the marked structures strongly gravitating to episode boundaries and unmarked structures to medial positions.

These results illustrate several related issues in the analysis of discourse data. First, while the issue of markedness derives its importance from linguistic theory, a functional role for the distinction is explored, one which calls upon our (admittedly meagre) knowledge of language processing to provide some insight into how markedness might be employed in extended language use. Second, a methodology familiar from experimental psycholinguistics, a controlled experiment, provides real language data collected under controlled conditions. Such controls permitted an examination of the role of the sex of the speaker in the narratives, and in terms of the factor of markedness, no sex differences emerged.

A third aspect of the study involved an investigation to determine what role, if any, was played by the sex of the speakers in governing such production factors as relative wordiness, and numbers of clauses, pauses, and hedges uttered. While wide individual differences were found across speakers, no significant sex effect emerged in any of these variables. The only sociolinguistic factor which did exhibit significant results was that of same-sex dyads: when a speaker was talking to a friend of the same sex, that speaker tended to be somewhat more verbose than when speaking to a member of the opposite sex or to a stranger.

This brief example illustrates that controlled studies can investigate both sociologically significant variables and those cognitive factors of more immediate interest to linguists and psycholinguists. That is, while the two research paradigms sketched above clearly are distinct, there is nevertheless the possibility that with appropriate planning and organization, data on both types of factors can be extracted and examined independently. And such studies are crucial if the contributions of social, cultural, and cognitive variables to the organization of discourse are to be separated and understood.

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# PROPERTIES OF STUTTERED SPEECH

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## 1.0 INTRODUCTION

The purpose of this study was to compare the fluent and nonfluent speech of stutterers with the fluent speech of nonstutterers in terms of their prosodic features and spectrogram analyses. Three adult stutterers were matched with three nonstutterers and were asked to perform three tasks: passage reading, wordlist reading, and free speech. Results demonstrated that polysyllabic words and stressed syllables were more often stuttered, and speaking rate was slower in stutterers fluent and nonfluent speech. No differences were found between content and function words or in consecutive readings of the same passage. Spectrogram analysis showed increased glottal tension, more abrupt onsets and greater intensity of vowels within a stuttered segment. These findings suggest that glottal tension plays a role in the prolongation of phonemes and repetition of segments.

This paper is organized as follows: Section 2.0 presents a historical perspective on stuttering; Section 3.0 outlines the typical characteristics of stutterers' speech; Section 4.0 presents the methodology and results of the experiment and finally Section 5.0 presents the overall conclusions.

## 2.0 HISTORICAL PERSPECTIVES ON STUTTERING

From Ancient Greek philosophy to modern medicine, stuttering has been examined, discussed, and studied, and yet no firm causes nor cures have been discovered. In 384 B.C., Aristotle theorized that the tongue of the stutterer was too sluggish to keep up with his/her thoughts (Fiedler, 1983). In the Roman Era (4 B.C.) Celsus devised a treatment for these weak tongues. Gargling and massaging the tongue, he thought, would revitalize it, causing the stutterer to speak normally. Moving into the Middle Ages, Mercurialis believed the tongue to be either too wet or too dry, thus the stutterer should be treated accordingly with either blistering substances or gargles. In 1627, the stiff tongue theory was supported when it was noted that during periods of drunkenness, stuttering decreased. It was concluded that alcohol relaxed and loosened the tongue and therefore alleviated stuttering. One of the more horrific treatments for stuttering was developed in 1841. An Austrian surgeon believed the cause to be neurological spasms of the tongue. In order to relieve the stutterer of those spasms, and consequently of stuttering, the nerves

of the tongue had to be severed. This technique lasted for approximately ten years before it was finally realized that this procedure was creating worse speech disturbances than the one it was supposed to cure (Hulit, 1985).

Once into the 20th century, the focus of study moved to the central nervous system. Theories revolved around motor centres in the brain sending incorrect messages to the muscles of the tongue or to the respiratory system which controls breathing. In the 20's and 30's, the question of insufficient cerebral dominance arose (Hulit, 1985). Without dominance, neither hemisphere would impose its timing on language production. Thus, the signals from the two hemispheres would not be properly coordinated, causing stuttering. Other theories suggest that stuttering is some other kind of central nervous system disorder such as aphasia or epilepsy. This is no doubt due to the seeming lack of muscular control, erratic breathing, and facial tension that often accompany a stuttered event. During the 1950's, it was suggested that the auditory feedback system of the stutterer was delayed (Fiedler, 1983). Experiments had shown that nonstutterers' speech was impaired when their auditory feedback was artificially delayed, and thus the same feedback delay must happen in stutterers. Other studies suggested the problem originated in the larynx: inappropriate vocal fold vibration and difficulty in controlling air flow. In addition to the physiological theories of stuttering, others have proposed that stuttering is a symptom of a psychological problem: a response to a traumatic experience, or a learned behavior associated with fear of a specific event (Hulit, 1985).

Most recently, researchers have combined physiological, psychological, and neurological approaches, attributing stuttering to "the dynamic interaction and co-ordination of four neural systems" (Nudelman, 1992). Three of those systems involve the muscle groups for respiration, laryngeal activity, and articulation, all of which must be in sync with one another. The fourth system is a complex and intricate "cognitive" system that is comprised of many subsystems. These subsystems are responsible for such things as cognition and other psychological functions which select words from the lexicon and monitor the speech that is produced. A stuttered event occurs when there is a breakdown in the dynamic coordination of these systems (Nudelman, 1992).

Regardless of the multiple domains suggested as the origin of stuttering, there is no one position which can clearly explain the differences among stutterers. Stuttered events vary in the ways and situations in which they are expressed. Obviously, a complex interaction exists between physiological, neurological, and psychological factors. Treatment of stuttering has come a long way since the severing of tongues, and yet the most we can ask for now is that it be controlled by the many therapeutic methods available. And stuttering can only be overcome if the processes and properties involved in stuttered speech are understood.

### 3.0 CHARACTERISTICS OF STUTTERED SPEECH

One problem that is encountered in researching stuttering is how to qualify a stuttered event, as it is a complex and variable disorder that is hard to delimit. "Most speech-language pathologists regard part-word repetitions and silent or audible prolongations as the essential characteristics of stuttering" (Wingate, 1976). The occurrence of these speech dysfluencies are highly correlated with linguistic stress. Stressed syllables and words demand greater effort from the vocal system than those that are unstressed, and therefore are more likely to be stuttered (Bergmann, 1986). Prins, Hubbard, and Krause (1991) analyzed the various aspects of stress associated with dysfluency by having subjects read a short passage. The properties of the stuttered events were then analyzed and it was found that stuttered events occurred on syllabic stress peaks twice as frequently as on unstressed peaks, more often on polysyllabic words than on monosyllabic words, and more on content words than on function words. Although linguistic stress is highly correlated with occurrences of stuttered events, the segmental speech errors of normal speakers tend to occur at the same locations as stuttered events (Prins, Hubbard, and Krause, 1991). Thus, we have not distinguished stuttering entirely from occurrences of natural dysfluency. Van Riper (1971) provides an analogy to understand the difference between natural dysfluency and stuttering: we all stumble occasionally when we walk, but when it occurs frequently to the point where it inhibits our forward motion, then we would classify it as a "problem" or disorder.

Two features that have been found to differ between stutterers and nonstutterers are rate of speech and frequency of dysfluent utterances. Bakker and Bruten (1990) found that "stutterers exceeded the nonstutterers in the number of stuttering-type dysfluencies, frequency of normal dysfluencies, and the time needed to complete the oral reading". Other physical/acoustical differences exist as well, such as voice onset time (VOT), vowel length, and stress, which will be discussed in turn.

Some prosodic differences do not actually involve a stuttered event, but are found in the fluent speech of stutterers. A study by Prosek and Runyan (1982) involved presenting an audiotape recording of speech samples from stutterers and nonstutterers to a panel of judges whose task was to indicate which utterance was that of a stutterer. The speech samples "contained no instances of overt stuttering, audible respirations, or inappropriate voicing" (Prosek and Runyan, 1982). The judges were accurate in determining which was the stutterers' speech 85% of the time. Measurements of the speech samples were then made. These samples revealed significant differences between the groups for speaking rate (4.3 syllables per second in stutterers' speech compared to 5.2 syllables in nonstutterers speech) and average vowel duration (170.6 ms versus 144.1 ms), but not for average pause duration or number of pauses. Other studies have found stutterers' vowels exceed nonstutterer's vowels by 120-200 ms in duration (Disimoni, 1974).

The quality of the vowel in stuttered speech has also been questioned. The stuttered syllable appears to contain a neutralized vowel. "Almost universally the schwa vowel can be heard in the stutterers' abortive speech attempts" (van Riper, 1971). Two different hypotheses have emerged to explain the occurrence of the schwa-like vowel. Van Riper claims that stutterers produce an "incorrect" vowel in their speech and so discontinue their attempt at finishing the word. They then restart the word and repeat the process until they achieve the correct vowel. On the other hand, Howell and Vause (1985) report that the spectral properties of the stuttered vowel are similar to the following fluent vowel, thus the vowel is being articulated appropriately. It is the short duration of the stuttered vowel which tends to make it sound like schwa (Freeman et al., 1976). Unfortunately, analyses of the vowel in a stuttered event only applies to the speech of a syllable repetition stutterer, and does not account for other types of stuttering such as phoneme blocking: an inaudible prolongation of the onset of a phoneme, or whole word repetitions.

The present study was undertaken to determine if any generalizations could be made about stuttered speech, regardless of the type of stutter produced. Specifically, we wanted to see if spectrogram analyses of stuttered events would provide any insight into the physical and acoustical properties of stuttering. Additionally, we wanted to see if the results found in previous studies of prosodic features of stuttered speech could be duplicated.

## 4.0 THE EXPERIMENT

### 4.1 Subjects

Three adult stutterers: two males and one female; and three nonstutterers, matched in age and gender, were tested in this study. The average age of the stutterers was 25.6 years, and 24 years for the nonstutterers. Another male stutterer had been tested but his data had to be discounted. Since English was not his native language, this subject could not perform the required tasks in the same manner as the other subjects. All the stutterers had been diagnosed at some point by a Speech Therapist but only one subject, HP, had been through extensive therapy. Both experimenters were present during the testing. Each subject was recorded on a reel to reel recording device and then portions of their speech were transferred to the KAY Digital Sona-Graph 7800 for spectrogram analysis.

### 4.2 Procedure

The experiment was divided into three sections so that various aspects of stuttered speech could be analyzed. The first task involved reading a short nonsense passage, adapted from



Shakespeare's "A Midsummer Night's Dream" (Appendix A). The passage contained twenty-nine words and forty-one syllables. The subjects were asked to read it three times to test the hypothesis that as familiarity increased, rate of stuttering would decrease. Between each reading, the subjects were asked to read aloud one word list. The first list consisted of forty monosyllabic words (Appendix B). The words were carefully selected so that the voiced and voiceless counterparts of each place of articulation were represented. The consonants used were [p,b,f,v,t,d,t,č,j,k,g] and each was positioned in front of four different vowels [i,e,u,o]. The second list (Appendix C), contained twenty polysyllabic words, consisting of either two or three syllables. The initial consonants of these words included the ones mentioned above in addition to nasals, vowels, and the remaining fricatives. The vowel following the consonant was not controlled. The important aspect of this list was the position of stress in the word. Thirteen of the twenty words were stressed on the first syllable. The final portion of the experiment involved a free speech component. The subjects were asked to talk spontaneously for two or three minutes. The first 150 words for each subject were used in the analysis.

Once the testing had been completed, the tapes were analyzed. The stuttered events from each section were recorded by each experimenter. All phoneme prolongations, silent or audible, and repetitions, whole words or syllables, were considered stuttered events. Whole words were considered stuttered, as opposed to natural thinking repetitions, when an obvious struggle occurred with the word. There were audible features associated with these struggles which will be discussed later in this paper. The results from the two experimenters were then compared. If there were any disagreements, the tape was reanalyzed until a consensus was reached.

### 4.3 Results

#### 4.3.1 Passage Reading, Word Lists, and Free Speech Tasks

The results obtained from the analysis of the passage and word reading tasks for the stuttered events did not concur with all of the previous research. The passage reading task did not show conclusively that consecutive readings result in adaptation: a "decline in stuttering frequency that accompanies consecutive oral readings of the same material" (Prins and Hubbard, 1990). As shown in Table 1, only subject SC improved over the three trials. DM and HP remained constant, with HP recording no episodes of stuttering in any of the trials.

<u>PASSAGE REPETITIONS</u>	<u>SUBJECT</u>		
	<u>SC</u>	<u>DM</u>	<u>HP</u>
1	4	2	0
2	1	2	0
3	1	1	0

TABLE 1: The number of words stuttered (out of a total of twenty-nine words) by each stutterer on the three consecutive readings of the same passage.

This data was inconsistent with previous research that found stuttering declined with consecutive readings. Prins and Hubbard (1990) had subjects read a passage five times. There was a reduction of about 50% in stuttering frequency from the first to the fifth reading. Though our subjects show a 50% improvement, the numbers are not significant enough to show that adaptation occurs.

The passage readings do show that the stutterer's speech is longer in duration than nonstutterers, which is what we expect to find. The average duration of the passage was 12.4 seconds for the stutter group and 10.3 seconds for the controls: a difference of 2.1 seconds. SC's first reading was not included in the averaging as her four episodes of stuttering would skew the results. The rest of the stuttered events would not independently cause such a difference in duration. It should be noted that although the controls did not register any stutters, normal periods of dysfluency, such as pauses, did occur.

The word lists were used to see if stuttering rates corresponded with the initial segment of the word or the following vowel, or if it was affected by the number of syllables in the word. The results are shown in Table 2.

	<u>SC</u>	<u>DM</u>	<u>HP</u>
WORDLIST 1: /40	13, 33%	0	\
WORDLIST 2: /20	6, 30%	2, 10%	\

TABLE 2: The number and percentage of words stuttered on the monosyllabic and polysyllabic wordlists, respectively.

Again no generalizations can be made about rate of dysfluency or types of segments stuttered from the data obtained in this exercise. Subject SC stuttered on one third of the words in both lists whereas DM stuttered only twice on the polysyllabic words. Perhaps the idiosyncra-

sies of our subjects, i.e. level of nervousness and the types of activities they are comfortable doing, can account for these differences. Both this test and the previous one show that SC's and DM's stutters occur on a variety of segments, and thus no conclusions can be made about the type of segments more frequently stuttered. All the stutters that occurred on the polysyllabic words occurred on the first syllable, which was also stressed. This supports previous research which shows an increased rate of stuttering on stress peaks. The data obtained from HP was not used as he has been through extensive therapy and can apply techniques to control his stuttering, especially on a reading task. On his first repetition of the lists he did not stutter at all. But when he indicated that he could "turn off" the techniques he had learned, and then repeated the lists, he stuttered on all the words. Thus it is not felt that analysis of the prosodic features of his stuttered speech would adequately reflect the patterns of a "normal" stutterer.

The free speech component of this experiment provided interesting data with which to compare previous research. The data collected from the stutterers' free speech is shown in Table

<u>FREE SPEECH</u>	<u>SC</u>	<u>DM</u>	<u>HP</u>
# OF WORDS STRD	14	12	14
TOTAL # OF WORDS	150, 9.3%	150, 8%	150, 9.3%
# OF POLY WORDS STRD	3	5	5
TOTAL # OF POLY WORDS	20, 15%	23, 22%	33, 15.2%
# OF MONO WORDS STRD	11	7	9
TOTAL # OF MONO WORDS	130, 8.5%	127, 5.5%	117, 7.7%
# OF CONTENT WORDS STRD	8	6	7
TOTAL # OF WORDS STRD	14, 57%	12, 50%	14, 50%
# OF FUNCTION WORDS STRD	6	6	7
TOTAL # OF WORDS STRD	14, 43%	12, 50%	14, 50%

TABLE 3: The number of words stuttered on different types of words, and the percentage associated with these numbers.

(STRD=stuttered,POLY=polysyllabic,MONO=monosyllabic)

The percentage of words stuttered supports previous research which found the frequency of stuttering ranged "from 2% to 23% of words spoken, with a mean of 8.3% stuttering" (Healy and Ramig, 1986). The data found for polysyllabic and monosyllabic words reinforces studies that show polysyllabic words are more likely to be stuttered than are monosyllabic words. An average

of 17.4% of the total number of polysyllabic words in the subjects' free speech were stuttered, whereas only 7.2% of stutters occurred on monosyllabic words. A major difference found in this experiment and others is in the occurrence of stuttering on content and function words. These subjects demonstrated an even distribution of stuttering on content and function words, whereas Prins, Hubbard, and Krause, 1991, found that content words were twice as likely as function to be stuttered.

#### 4.3.2 *Spectrogram Analysis*

Stuttering has been described as speech that is "interrupted by a motorically disrupted sound" (Perkins, 1990). Therefore, stuttering must be looked at from a perspective which can exemplify the motor and acoustic properties of dysfluent speech. By analyzing a spectrogram, one can compare a fluent and non-fluent segment, and easily decipher one from the other. Stuttered speech is characterized by certain acoustic properties. Some of these features are: a sharp increase in articulatory power, constriction of the glottis, and an increase in length of closure before the release of the segment. Each of these will be discussed in the spectrogram analysis.

We propose to distinguish the onset of a stuttered segment from a nonstuttered segment by introducing the terms 'gentle onset' and 'abrupt onset'. Characteristics of an 'abrupt onset' are: a long closure before the onset of the segment that acts as a block, increased intensity of the vowel formants following the stuttered event, and increased aspiration following a voiceless stop and short bursts of scattered energy following the release of voiced stops. These three characteristics are dynamically dependent on one another. The release of the segment exerts a powerful energy burst which causes an increase in intensity throughout the syllable in which the stuttered event occurs. The power is exemplified by the amplitude marker on the spectrogram and the dark vowel formants following the stuttered event. The formant which seems to be most affected is the second formant in the spectrogram. This second formant is generally believed to be associated with the natural mode of vibration of the air in the mouth in front of the highest point of the tongue (Rogers 1991). It is speculated that when the air is released after a long glottal closure, the pressure of the air is forced through the oral cavity at a rapid pace, illustrated by dark second formants on our spectrograms. Unfortunately, recent theories believe that the vocal tract vibrates as a whole system and it is impossible to treat the throat and mouth cavities as independent (Ladefoged, 1962). Thus, further conclusions regarding F<sub>2</sub> require more intense study that are beyond the scope of this paper.

'Gentle onsets' are what we find in most fluent speech. Gentle onsets are characterized by a constant intensity in the vowel formants and a constant airflow through the production of a sound.

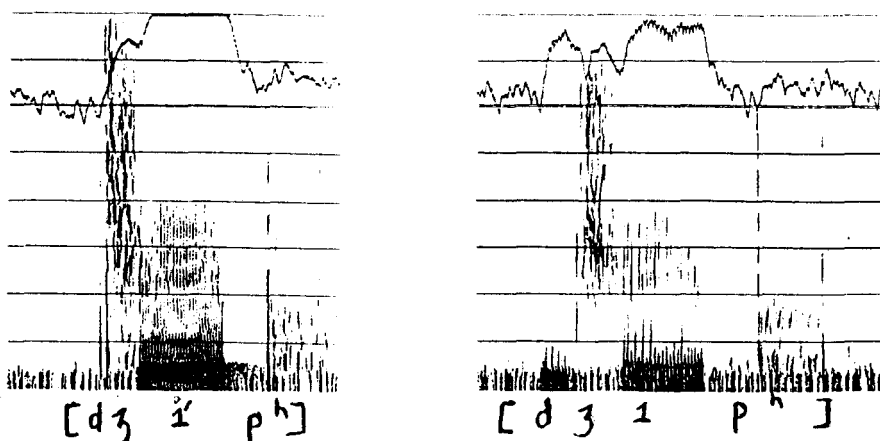


FIGURE 1: a) "abrupt onset" of dysfluent affricate in first segment of the word "jeep"  
 b) "gentle onset" of fluent affricate in the first segment of the word "jeep"  
 spoken by subject HP.

Figure 1 demonstrates the distinction between 'gentle onset' and 'abrupt onset'. The dysfluent onset of 1a) is marked by an abrupt release, while the fluent release of the segment in 1b) shows a 'gentle onset'.

Glottal tension and constriction are characteristic of stuttered speech. The tension and constriction of the glottis is manifested in the spectrograms by the increased distance between the glottal pulses. Glottal tension can be demonstrated by the alteration in the glottal pulses of fluent and dysfluent speech. In fluent speech the glottal pulses are evenly distributed, whereas in dysfluent speech the glottal pulses are irregular and the distance between them is longer than in fluent speech. Each individual glottal pulse is also wider. Furthermore, we hypothesize that this constriction prevents the production of a sound or word from being completed and therefore, the word or syllable is attempted again. To summarize, the two properties of stuttered speech that are apparent in the analysis of the spectrograms are glottal constriction and the differing types of onsets: 'gentle' and 'abrupt'.

Two different types of stuttering occur in human speech: repetitions, which are either repetition of a syllable or of a word, and prolongations which are either audible or silent (Wingate 1976). The silent prolongations are also referred to as "blocks". These "blocks coincide to the momentary occlusion of the airway" (van Riper, 1971). This closure may occur at the level of the vocal cords or within the mouth. In order to emit the air, the stutterer compresses his or her abdomen and forces the air to overcome the blockade (van Riper 1971). This explains the abrupt onset demonstrated in the release of stuttered events, described as "blocks".

The following analysis of the spectrograms will be concerned with the two subtypes and the four manifestations of stuttering.

**SONORANTS** Our data concluded that dysfluent vowels are found only in word initial position. Moreover, they also shared another feature with one another: all stuttered vowels were silent prolongations. These silent prolongations were manifested in long pauses preceding the abrupt onset of the vowel. The sudden burst of energy released by the onset of the vowel caused the formants of the vowel to be more intense when compared to the other vowels in the word or phrase that followed the stuttered event. The sonorants were also characterized by glottal tension in some instances. In Figure 2, glottal tension was found after the first attempt on the initial syllable. The syllable was then re-attempted. This is consistent with our glottal tension hypothesis which states that glottal constriction enables the speaker to complete the word. Therefore, the articulation must be attempted again.

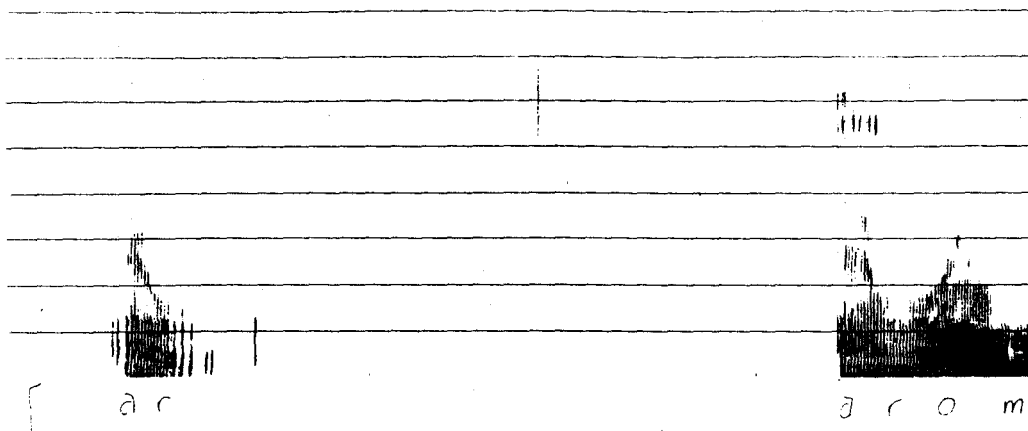


FIGURE 2.: "Aroma". Syllable repetition of two sonorants.

As stated above, the repetition in Figure 2 is characterized by glottal tension manifested in increased distance between the individual glottal pulses. Glottal tension forces the syllable to be re-attempted.

A marked difference was found in the length of dysfluent nasals. Release of dysfluent nasals are preceded by a long voicing bar. The dysfluent nasal stop is comparatively longer than the fluent nasal stop of both the person who stutters and the person who does not stutter.

**OBSTRUENTS** The class of obstruents includes fricatives, affricates, and oral stops. Each of these subclasses were represented in our dysfluent data. Voiced and voiceless counterparts of each subclass were also represented with one exception: voiced fricatives were not stuttered on.

Stuttered events concerning fricatives would be generally classified as audible prolongations. This infers that the fricative is held for a longer than normal duration. Figure 3. shows an audible prolongation that is attempted twice, therefore, it has both the features of the two types of stuttering. The two attempts at the phoneme are different; the first attempt has scattered activity concentrated at approximately 4000 to 7000 Hz, and the second attempt has activity throughout the spectrogram (1000 to 8000 Hz). Van Riper (1971) stated that a person who stutters formulates the lip posture of a schwa when a stuttered event occurs. This produces a neutral, isolated /s/ instead of the allophonic /s/ dependent on the following vowel in the word. This appears to account for the first prolongation on the spectrogram. The second attempt has a concentration of activity where the formants of the following vowel occur.

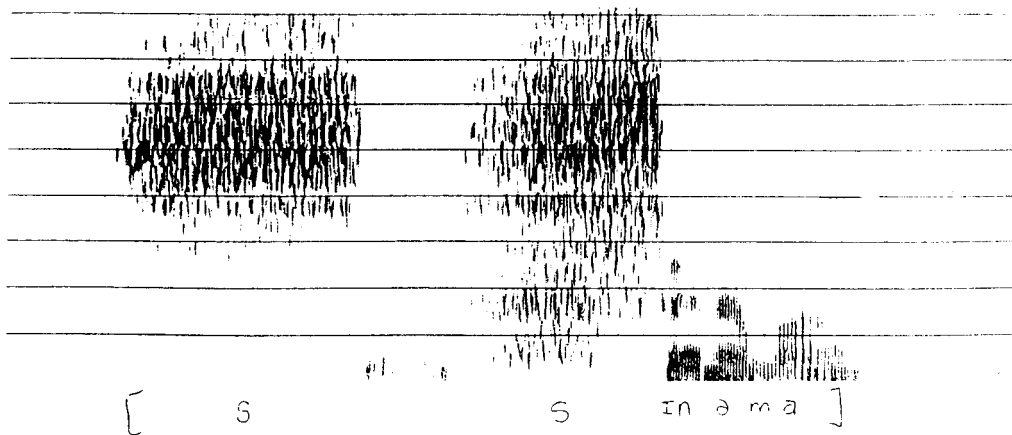


FIGURE 3:. Audible prolongation and repetition of the phoneme /s/ in the initial segment of the word "cinema"

Duration is the most prevalent aspect of stuttered fricatives. Dysfluent fricatives are approximately two to three times longer than fluent fricatives. Duration is an accepted indicator of the severity of this speech disorder. Thus, the longer the duration, the more severe the disorder.

Our data concerning voiced oral stops is congruent with our hypothesis stated earlier that there is increased glottal tension at stuttered events. Oral stops can be characterized within the two subtypes of stuttered events: they are either manifested as audible or inaudible prolongations, or phoneme repetitions. Inaudible prolongations are shown to have a long closure period before the onset of the oral stop. Moreover, when this long closure is released, it produces an abrupt onset of the consonant, causing increased intensity in the vowels of the syllable containing the stuttered event. Audible prolongations are exhibited in the spectrogram by a prolonged voicing bar before the release of the oral stop. This indicates a negative voice onset time (VOT). The long voicing bar also displays glottal tension. Glottal tension, a short period of voicing characterized by extreme vocal fold tension, is demonstrated by the individual glottal pulses above the voicing bar. Figure 4 shows a prolonged voicing bar before the release of the phoneme /g/. contrasted with a fluent reading of the same word.

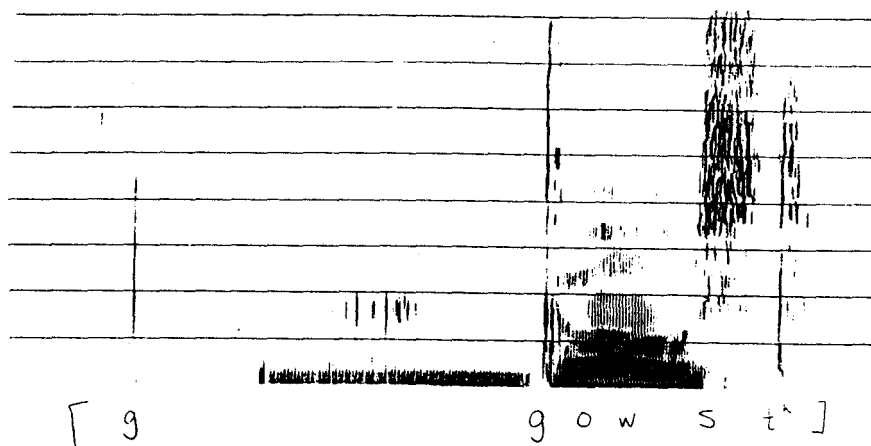


FIGURE 4: a) Audible prolongation demonstrated by long voicing bar before onset of oral stop /g/ in the word "ghost".

Glottal constriction is manifested above the voicing bar.  
b) Fluent reading of the word "ghost".



## 5.0 CONCLUSION

This study was undertaken to look at the linguistic realization of stuttered events (i.e. stress placement, within a sentence and within a word, monosyllabic versus polysyllabic words, and individual phonemes), and the physical properties involved, as determined by spectrogram analysis. Our results did not show any correlation between the occurrence of stuttering and linguistic stress. Analysis of phonemes within the spectrograms shows that glottal tension is involved in stuttered speech. The differences found between nonfluent and fluent speech are: 'abrupt onset', dark second formants (in vowels), and prolongation of the sound (in fricatives). Due to equipment limitations, a small sample, and time restrictions, we were not able to investigate these findings in depth, nor can we draw any firm conclusions. But further research in time may prove our data significant.

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## APPENDIX A

### PASSAGE

Be kind and courteous to this gentleman  
hop in his walks and gamble in his eyes,  
feed him with apricots and dewberries,  
with purple grapes, sour figs, and mulberries

## APPENDIX B

### WORDLIST 1

- |            |            |
|------------|------------|
| 1. feed    | 21. cheap  |
| 2. bait    | 22. gate   |
| 3. pooch   | 23. ghost  |
| 4. June    | 24. coop   |
| 5. phone   | 25. veep   |
| 6. tail    | 26. vote   |
| 7. poke    | 27. douche |
| 8. toot    | 28. peace  |
| 9. jeep    | 29. case   |
| 10. boat   | 30. boot   |
| 11. choose | 31. choose |
| 12. keep   | 32. taupe  |
| 13. pace   | 33. deep   |
| 14. fame   | 34. Jake   |
| 15. goop   | 35. chain  |
| 16. beat   | 36. joke   |
| 17. tease  | 37. food   |
| 18. dopt   | 38. geek   |
| 19. voom   | 39. coast  |
| 20. vain   | 40. date   |

## APPENDIX C

### WORDLIST 2

1. pizza
2. pizzazz
3. suppose
4. cabinet
5. cinema
6. aroma
7. maintain
8. charity
9. super
10. velocity
11. fundamental
12. xanadu
13. difficult
14. negative
15. superior
16. judgment
17. testament
18. zombie
19. beaker
20. bountiful

# Acoustic correlates of the fortis/lenis contrast in Swiss German plosives

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## 1.0 INTRODUCTION

Several of the High Allemanic dialects of German, collectively known as Swiss German, exhibit consonantal contrasts which differ in nature from those in other German dialects. Through spectral analysis, the nature of the two Swiss German plosive series (/p, t, k/ contrasting with /b, d, g/) will be investigated. The manner in which these series contrast is not one of voicing or aspiration, and can best be characterized as fortis versus lenis. The acoustic character of the fortis/lenis contrast in Swiss German plosives will be explored by examining the main spectral features of each plosive in three phonetic environments: word-initially, word-medially, and word-finally.

The problem to be addressed directly is one of speech production: what, if any, acoustic correlates can be found to distinguish fortis stops from lenis ones in (1) the closure phase, and (2) the release phase of the the plosives? Additionally, a consideration of possible articulatory gestures associated with fortis and lenis production will lead to a simple hypothesis predicting two spectral correlates of the vowels following fortis plosives, at least one of which has been clearly observed.

## 2.0 THE CONTRAST *fortis/lenis*

### 2.1 [ $\pm$ fortis] in a phonological system

A great deal of discussion has taken place in the past concerning the nature of consonantal contrasts. The features voiced/voiceless, aspirated/unaspirated, and tense/lax have variously been applied to the descriptive differentiation of consonant series. The opinion supported here is that put forth by Kohler (1979, 1982, 1984): the fortis/lenis opposition is most closely related to the tense/lax opposition. It will be shown that this explanation incorporates both the closure and release phases of stops as a single dynamic system, unlike the traditional account of such contrasts in terms of Voicing Onset Time alone.

The feature [ $\pm$ tense], phonologically speaking, has not traditionally met with unanimous reception. Catford (1977: 203) allows that tense/lax features may be productive for stops. H. Sweet (1877) was the first to suggest that a parameter of tension may be manifested by specific glottal gestures (such as voicing accompanying lax obstruents). This point of view was taken up by

Jakobson, Fant and Halle (1952), who suggested several articulatory gestures to accompany tenseness in stops, specifically the strength of the explosion, caused by muscular strain affecting the walls of the vocal tract and glottis. Catford (1977) has replied that it is doubtful if this exists, but reports that Malecot (1970) has singled out air pressure behind the point of articulation as being a reliable indicator of tense obstruents. The system of laryngeal features proposed by Halle and Stevens (1971) does not incorporate the tense/lax opposition as a separate phonatory dimension, placing voiced and plain (voiceless unaspirated) stops together under the rubric of a "neutral" glottal state ([spread glottis, -constricted glottis]). Clearly, the parameter (or perhaps several parameters) of tenseness in obstruents has never been definitively described in relation to articulation or acoustics.

Kohler (1984) has argued for the adoption of the feature [ $\pm$ fortis] in general phonological description. He singles out the atemporal nature of conventional descriptions of the /b, d, g/ versus /p, t, k/ opposition as a major weakness. Specifically, he characterizes much past work as a "translation theory" which maps static, discrete phonological elements like [ $\pm$ voiced] and [ $\pm$ aspirated] onto dynamic and continuous processes (i.e. the speech chain). He argues for the integration of the time dimension into the phonology via a specific agenda for linking the phonological categorization of obstruents with the terms *fortis* and *lenis*, stressing that [ $\pm$ fortis] is not proposed as an abstract feature, but rather one with a phonetic base provided by degrees of articulatory power.

## 2.2 The Temporal Nature of Fortis/Lenis Plosives

Through perceptual and acoustic studies (Kohler 1979, 1982, 1985) it has been demonstrated that the fortis/lenis plosive opposition in New High German is manifested as acoustic and articulatory features in two distinct temporal frames. These properties will now be reviewed, along with current results concerning the perceptual relevance of each such feature with respect to the maintenance of a productive contrast between the two plosive classes in the language.

### 2.2.1 The Closure Phase

The period of silence immediately preceding the release burst of the plosive (but following the preceding segment) will be referred to as the closure phase. It is here that the perceptually salient correlates of articulatory tension are manifested. It has been suggested (Jakobson, Fant and Halle 1952) that greater articulatory power is present in the articulation of fortis plosives, which manifests itself in more extensive movements as well as greater peak and average velocities of the articulators (Kohler 1984). This leads to the well-documented tendency of the length [preceding vowel plus closure phase] to remain constant over fortis and lenis

segments. It has been shown (Kohler 1977) that the duration ratio vowel/(vowel + closure) is important for the fortis/lenis distinction. Specifically, in NHG a ratio above 0.70 is a clear indication of a lenis stop (in the case of a phonologically stressed /a:/ before a plosive), while a ratio below 0.60 is unequivocally related to a fortis plosive (Kohler 1979).

In addition to the durational features of the closure phase, it has been found that, in V<sub>1</sub>CV<sub>2</sub> contexts, V<sub>2</sub> is prepared through coarticulation at an early stage in the speech chain (Öhman 1966). If C is a fortis plosive, and thus quite long, the articulation for V<sub>2</sub> will be mainly shaped during the closure, whereas in the case of the much shorter lenis plosive the coarticulatory effect will extend into V<sub>1</sub> and be apparent in different formant transitions (Kohler 1984). Kohler identified the two dimensions of duration ratio and formant transitions as perceptually salient for the contrast, but placed duration ratio above formant transitions in a saliency hierarchy.

The effect of the manner of plosive articulation on the local F<sub>0</sub> (fundamental frequency) contour has also been investigated. The experimental results of Kohler (1985) show that a higher pre-closure F<sub>0</sub> within the final 100 ms period of the preceding vowel is associated with a fortis plosive, as against a lenis one, providing the global intonation pattern can incorporate such a modification without losing its characteristic F<sub>0</sub> macrostructure. This F<sub>0</sub> feature can be a cue to stop manner in perception. Kohler suggests that the local F<sub>0</sub> effect is connected with different degrees of vocal fold tension in the production of the two stop manner classes of German.

### 2.2.2 The Release Phase

The release phase of the plosive begins at the end of the plosion itself, as soon as the articulators have been separated. It is here that any glottal activity occurs which may contribute to the category separation in perception of fortis from lenis. In stops, aspiration and voicing can be seen to be glottal reinforcements of the fortis and lenis actions at the oral valve to produce the necessary intensity differences (Kohler 1984). In standard High German, fortis stops are accompanied by aspiration during their release. In other languages the fortis/lenis contrast may be manifested by voicing accompaniment in the lenis stops. The importance of these release features must not be underestimated, for in utterance-initial stops the closing movement can add very little to the differentiation. In this case, the distinction is carried by the release phase, meaning the accompanying glottal activity.

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<sup>1</sup> The effect is observed in English in CVC syllables (*bat, bad; back, bag*), where there appears to be a certain fixed 'quantum' of duration available for the monosyllable, the vowel being lengthened when the consonant is shortened (Catford 1977; 200).

For plosives in initial position, then, it is clear that the fortis/lenis distinction in perception must shift its salient features from the closure to the release. What remains unclear is precisely what release features can be perceptually salient. It is the opinion of Kohler (1984) that only strongly recognizable glottal features such as voicing and aspiration may be of importance. While a perceptual study has not been undertaken here, it does seem reasonable to extrapolate from Kohler's findings regarding the perceptual salience of pre-closure formant transitions to the suggestion that post-release formant transitions may also be perceptually salient.

### 2.2.3 Global Tension in Fortis Manner

It has been stated above that an increase in power expended by the articulators at the oral valve accompanies the stronger closure of a fortis plosive. Evidence has been found which supports the notion that the larynx also experiences a certain increase in tension during the production of a fortis plosive. Investigations by Catford (1977) have indicated that an increase in "phonatory tension" accompanies fortis obstruents in Javanese, while Ohde (1983) has made reference to vocal cord tension to explain  $F_0$  increases associated with voiceless (fortis) stops in General American English. Ohde cites Honda (1981) for physiological data showing the relationship between increased longitudinal vocal cord tension and higher fundamental frequency. Kohler (1982) has summarized the fortis—tension relationship in global articulatory terms: "Different levels of muscular activity (articulatory effort) seem to be set for the vocal tract in its entire extension from the larynx upwards."

The effects of this global tension in each temporal phase of the fortis plosive are distinct. The closure phase is affected primarily by articulatory tension at the oral valve (point of articulation), causing increased closure speed and duration. These tension features of the closure provide perceptual indications of the plosive contrast in word-medial and word-final position. The release phase, where glottal features are found, is primarily affected by tension in the vocal folds. The precise acoustic correlates of this phonatory tension in fortis obstruents are not clearly known.

Kohler and van Dommelen (1987), however, explored the possibility that global voice quality may influence fortis/lenis judgements in obstruent perception, and found that "tense voice" shows a fortis bias. They created stimuli in which fortis and lenis stops were placed within one of three voice quality frames: tense voice, neutral voice, and breathy voice.

Across all the utterances, the spectra of "tense voice" differ from those of the other two voice qualities by having a less prominent first spectral peak in relation to the higher-order peaks and/or a less steep spectral tilt. This reduces the lower-frequency energy concentration and, therefore, the ratio between the lower portion of the spectrum [below 1000 Hz]



and the upper one; it also defines the peaks more sharply. This corresponds to the narrow formant bandwidths in the laryngealized as against the plain vowels described by Ladefoged (1982) for !Xóó, a Khoisan language of southern Africa. [pp. 367-369]

Subjects showed an increased tendency to judge plosives as fortis when they were presented within a tense voice quality frame. They found the effect to exhibit both a pre-closure and a post-closure component, with the differing spectral energy distribution of tense voice manner cueing the opposition.

### 3.0 SWISS GERMAN PLOSIVES

At this point, given the literature that has been reviewed above, it is possible to formulate a hypothesis predicting certain acoustic (spectral) features of the release phase of fortis plosives, aside from those associated with voicing and aspiration.

1. increased articulatory power (and speed) will result in faster formant transitions from plosive release to vowel stasis;
2. increased glottal tension will result in increased intensity of high frequency formants in the post-release sonorant region, as a source phenomenon.

#### 3.1 General Phonetic Properties

While Swiss German dialects have never been widely studied in a phonetic context, their general phonetic properties, in comparison with those of standard High German, have been recognized. Specifically, it is known that all obstruents are in general voiceless and all stops (there are six: /p, t, k, b, d, g/) are unaspirated (Moulton 1983; Dieth 1950). There is no syllable position in which the commonly recognized release features (voicing and aspiration) differentiate between fortis and lenis. In all word-medial and word-final cases the distinction is carried by articulatory timing, the fortis consonant being substantially longer (Kohler 1984). The question then remains as to what fortis plosive features could possibly carry the distinction in utterance-initial position. It is the opinion of Kohler (1984) that the fortis/lenis contrast in such positions is in fact neutralized. However, the experimental results presented below indicate quite clearly that other acoustic properties of the release and post-release phase can be directly associated with the [+fortis] feature.

## 3.2 Analysis of Swiss German Speech

### 3.2.1 Methods

A native speaker of Zürichtötsch, the Zurich dialect of Swiss German, was provided with a small corpus of words in list form. The words were carefully read aloud by the speaker, and recorded on tape.<sup>1</sup> The corpus included several examples of each type of Swiss German plosive (/p, t, k, b, d, g/) in each of three phonetic environments (word-initial, word-medial, word-final). From the tape recording, a wide-band spectrogram of each word in the corpus was produced, using the Kay Digital Sonograph 7800. Through examination of the spectrograms, the spectral features common to each of the six plosive varieties were deduced. Statistical analysis was applied where necessary to show the significance of the results; however, any noted features associated with a particular type of segment were invariably manifested in all relevant examples in the corpus.

### 3.2.2 Results

The following features of Swiss German plosives were evident from the spectrograms:

Voicing Onset Time (VOT) was virtually identical for fortis as against lenis stops: mean VOT for both /t/ and /d/ was approximately 14 ms and showed no significant variance from fortis to lenis.

Voicing of the stops was not evident, with the exception of [p] in a lateral plosion ([pɫV]). In such cases, VOT diminished to zero.

Aspiration was clearly evident only in final position, where it occurred to the same degree in fortis and lenis stops, and was accompanied by low-level vocalization.

Closure duration of fortis stops was clearly longer than that of lenis stops (evident in medial and final positions only); the fortis closure was on the order of 4 times the length of the lenis closure.

Occurrences of [t] and [d] in initial position show that an F<sub>2</sub> locus at 2 kHz is generally associated with both classes of alveolar stop.<sup>2</sup>

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<sup>1</sup> The speaker was female, in her thirties, and was not entirely phonologically fluent in English.

<sup>2</sup> A clip-on condenser microphone was used, with a Technics 2-track reel-to-reel (quartz-locked, isolated loop design).

Post-release sonorant formants above  $F_2$  show increased intensity, movement, and clarity following all types of fortis stop, as against lenis stops.

With respect to the closure phase, these results demonstrate the validity of closure duration as a distinguishing feature of fortis as against lenis stops. The commonly recognized release features of aspiration and voicing, however, clearly do not carry the fortis/lenis distinction. Formant loci as well have been shown to be non-distinctive. The release and post-release phase of the observed stops do differ in the "activity" level (includes intensity and movement in transition) of high formant frequencies. The observed effect is strikingly similar to the features of global tense voice used in the perceptual study by Kohler and van Dommelen (1987). It remains to be determined whether fortis plosive production in Swiss German in fact exploits the perceptual salience of tense voice demonstrated by these authors.

#### 4.0 GENERAL DISCUSSION

Kohler (1984) states:

Word-initial stops must be characterized generally by the presence or absence of voice or aspiration, if [ $\pm$ fortis] distinctions are to be maintained in the language.

In isolated words [of Swiss German] the opposition is only signalled intervocalically.

Kohler is essentially claiming that voicing and aspiration are the only viable plosive release features to carry a fortis/lenis distinction in the absence of the perceptually powerful closure duration cue. Beyond that, he claims that the fortis/lenis opposition in Swiss German word-initial plosives is only maintained where connected speech provides the necessary intervocalic environment for the perception of a closure duration cue. This is equivalent to the aforementioned claim that, in isolated word production, word-initial plosive classes are neutralized.

The spectral features of Swiss German plosives described above clearly exhibit the increased formant activity predicted earlier. Whether this is actually the result of increased glottal tension, as proposed, remains to be determined. The likelihood of this seems high, however, in light of the spectral features of tense voice employed by Kohler and van Dommelen (1987). No evidence has been found to support the first hypothesis, concerning the speed of formant

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<sup>3</sup> Kewley-Port (1982) has shown that  $F_2$  and  $F_3$  loci are an invariant correlate of place of articulation only for alveolar plosives, and not for bilabials or velars.

transitions into post-release sonorants. In any case, the observed formant features can be directly associated with the fortis stops, and could be interpreted as a secondary perceptual cue which would become the primary cue in utterance-initial position. Such an analysis prevents the otherwise inevitable neutralization word-initially of the fortis and lenis plosives.

If vocal fold tension is indeed the articulatory factor responsible for the observed spectral correlates of the feature [+fortis], it becomes necessary to include tension as a variable in the laryngeal feature framework to be applied in phonological description. Thus far, no such feature system has been put forth which permits the treatment of glottal tension as an autonomous dimension in the framework, separate from other glottal states responsible for voicing and aspiration.

## 5.0 CONCLUSION

The spectral features of the various plosive segments in Swiss German have been examined, and the results indicate the presence of distinctive [+fortis] correlates in both the closure and release phases. While the durational cue in the closure has been shown (Kohler 1979) to be perceptually salient for the fortis/lenis contrast, such a cue cannot operate in word-initial position. Abramson (1986) suggests that, in such cases, a secondary feature present in the plosive release operates as a perceptual primary to facilitate the distinction. It is suggested here that the observed formant features associated with fortis stops may be sufficient to adopt the role of perceptual primary cue in initial position. The existence of such spectral features is not easily reconciled with Kohler's statement that the fortis/lenis opposition in Swiss German is entirely dependent on the features of the closure, and therefore will only manifest in word-medial or word-final position. A stronger statement in this regard will require positive evidence from a perceptual study investigating the possible perceptual salience of such contrasting features of the post-release formants. Given Kohler and van Dommelen's findings regarding the salience of global tense voice manner in fortis/lenis perception, such evidence does not seem unlikely.

While Swiss German is rare in that its fortis/lenis contrast is unsupported by strong release features (voicing and aspiration), it is unsubstantiated that such features are necessary to the maintenance of the contrast. Clearly, phonological systems in general are not so strictly constrained. This may relate to the notion that the laryngeal dimension of tension exists as an autonomous factor. Were this so, it would then clearly be possible to define a system in which fortis/lenis contrasts remained productive when tension alone provided a distinction.

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A FUNCTIONAL CATEGORY ANALYSIS OF THE  
GERMAN ACQUISITION DATA :  
A REPLY TO CLAHSSEN'S PARAMETER CONSTRAINTS

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## 1.0 INTRODUCTION

Clahsen (1991) proposes three learnability constraints on the Principles and Parameters approach to language acquisition: the Parameterization Constraint, the Parameter Setting Constraint, and the Continuity Constraint. The Parameterization Constraint restricts triggering domains, and the Parameter Setting Constraint states that once parameters are set, they can never be re-set. Clahsen uses the Continuity Constraint to propose that while parameters are available from the earliest stages of language acquisition, syntactic structures must develop. Clahsen therefore claims that while German children's initial grammars lack both a CP and an AGRP, these early grammars have an FP (finite projection) which allows some verb movement in the early acquisition stages. Acquisition of the agreement paradigm triggers the development of AGRP, and the acquisition of lexical complementizers triggers the replacement of FP with CP, and once CP is in place so is V2 word order. As well Clahsen claims that once children have the agreement paradigm, AGR can no longer identify the null subject (pro), and therefore overt subjects appear consistently<sup>1</sup>. In this paper, I argue that Clahsen's proposals regarding syntactic development violate Universal Grammar (UG), and that an analysis based on Guilfoyle and Noonan's (1992) Structure Building Hypothesis more satisfactorily accounts for the German acquisition data. I show that evidence from Stage I of German child language, which Clahsen ignores, suggests that early child grammars are lexical and therefore lack both an FP and an AGRP. Following Weissenborn (1990), I argue that verb movement is linked to finiteness and not to subject-verb agreement as Clahsen claims, and that the acquisition of the finite/non-finite distinction triggers the emergence of AGRP. I argue that children initially place the finite operator in AGRP and not FP as Clahsen proposes, and that placing this operator in AGRP allows the limited verb movement seen in Stages II and III. I then argue that the acquisition of lexical complementizers triggers both the development of CP and the movement of the finite operator from AGRP to CP, and that once this has occurred, V2 acquisition is complete. Finally, I argue that the appearance of overt subjects

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<sup>1</sup> Clahsen also accounts for the acquisition of negation which this paper does not discuss.

is also linked to the development of CP and the subsequent movement of the finite operator from AGRP to CP. In conclusion, I show that Clahsen's three learnability constraints are also compatible with a structure building analysis of the German acquisition data.

This paper is organized as follows: Section 2.0 summarizes Clahsen's analysis of German word order and overt subject acquisition data; Section 3.0 examines some potential problems with Clahsen's analysis of the German acquisition data, and Section 4.0 presents an alternate analysis of the data based on Guilfoyle and Noonan's (1992) Structure Building Hypothesis. Finally, Section 5.0 gives the overall conclusions and discusses some implications for triggering data.

## 2.0 CLAHSEN'S (1991) PROPOSALS AND ANALYSES

This section first discusses Clahsen's (1991) proposals for restricting the Principles and Parameter model of language acquisition, and then outlines Clahsen's analysis of German word order and overt subject acquisition.

### 2.1 Learnability Constraints

Clahsen (1991) claims that the current Principles and Parameters approach to language acquisition is too unrestrained and therefore he proposes three learnability constraints to restrict the theory. These constraints are given in (1).

- (1) a. Parameterization Constraint  
Parameters can only refer to heads or properties of heads.
- b. Parameter-Setting Constraint  
Fixed Parameters cannot be re-set.
- c. Continuity Constraint  
UG parameters do not mature.

The Parameterization Constraint is intended to solve the triggering problem by restricting the child's triggering experience to government domains. That is, all positive evidence required by the language learner to set a particular parameter must be available within the government domain of the head referred to in that parameter.<sup>2</sup> The Parameterization Constraint, however, does not

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<sup>2</sup>One problem with this claim will become evident in Section 5.0 when the implications for triggering data are discussed.



explain why the language learner is suddenly able to make use of a salient aspect of the linguistic input as a trigger for setting a parameter, when that evidence has always been available to the learner, but has not previously functioned as a trigger. The triggering problem therefore remains.

The Parameter-setting Constraint is intended to prevent the child from switching back and forth between parameter values when presented with contradictory evidence in the linguistic data.<sup>3</sup> Clahsen uses the acquisition of overt subjects as an example of this possibility: children learning non pro-drop languages such as English and German hear sentences both with and without overt subjects, and therefore could conceivably switch between the correct and incorrect parameter settings, before finally choosing the appropriate setting for the language being learned. This constraint also implies that learners require a certain amount of positive evidence before a parameter can be set. Since the acquisition data suggests that children do not switch parameter values when acquiring a language, but move in a steady progression toward the target grammar, this constraint is simply an explicit statement of how the theory functions.

Finally, the Continuity Constraint claims that the learning device does not change over time. This constraint implies that since UG parameters do not mature, they are available from the earliest stages of language acquisition. However, if parameters are always available, the Continuity Constraint alone cannot explain why learners do not immediately set them. Clahsen claims that children's syntactic structures differ from adult structures, and therefore although parameters are available, they can not be set until some aspect of the syntax has developed. This proposal is similar to Guilfoyle and Noonan's (1992) Structure Building Hypothesis, although as will become clear, the nature of syntactic development differs between the two accounts. Despite the difficulties with Clahsen's proposals, learnability constraints do have some merit, since the problems within current acquisition theory they are intended to solve are valid.

## 2.2 Clahsen's Analysis of the German Acquisition Data

Clahsen uses the three parameters given in (2) in his analysis of German language development.

- (2) a. Verb-Second Parameter<sup>4</sup>  
      [+F] is in (a) AGR  
              (b) C(omp)

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<sup>3</sup>This is similar to the deterministic learner Dresher and Kaye (1990) propose for the acquisition of stress.

<sup>4</sup>Clahsen uses INFL not AGR as one of the possible settings for this parameter. However, since I intend to follow Pollock's (1989) split-INFL hypotheses, I use AGR instead of INFL.

**b. AGR Parameter**

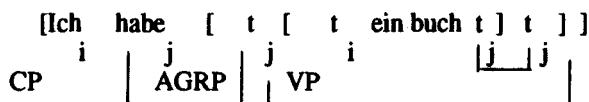
- AGR does (a) exist in the syntax  
(b) not exist in the syntax

**c. Recovery Parameter**

- AGR is (a) [+pronominal]  
(b) [-pronominal]

Clahsen analyzes [+F] as an operator which both governs nominative case and is bound by a variable (i.e. a trace). To govern nominative case, [+F] must be lexicalized, and to be lexicalized [+F] can be realized either as a subordinate complementizer, or a finite verb. For [+F] to bind an empty category, the element lexicalizing [+F] must move into the operator position (Pollock, 1989). When [+F] occupies COMP, as it does in German, the lexicalization and binding requirement together account for V2 word orders, as the examples in (3a) and (3b) illustrate.<sup>3</sup>

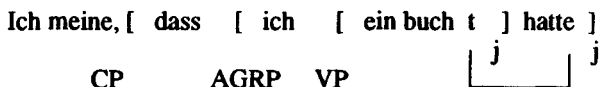
**(3a) Verb Movement in Matrix Clauses**



I have a book

I have a book.

**(3b) Lack of Verb Movement in Embedded Clauses**



I think that I a book had

I thought that I had a book.

<sup>3</sup>Clahsen ignores the movement of either the subject of non-subject into (spec,CP) required to derive V2 order. I assume that this movement also applies.

In (3a), the verb [habe] moves from the VP to AGRP, and then to COMP, while in (3b), [habe] only moves to AGRP, since [dass] occupies COMP. Since COMP is filled in both (3a) and (3b), the lexical requirement on [+F] is also satisfied. However, only in (3a) is the operator interpretation possible, since according to the standard definition of binding in (4), only in (3a) does the trace in AGR bind [+F].

- (4) A is bound by B iff  
 a. A is c-commanded by B  
 b. A and B are co-indexed.

Therefore finite verbs, and not complementizers exhibit subject-verb agreement.

The AGR parameter determines whether or not the syntactic structure includes an AGR phrase. The nature of agreement within the language determines the appropriate setting for this parameter. Languages having no subject agreement, or having subject and number agreement, select (b) AGR does not exist in the syntax, while languages having subject-person agreement select (a) AGR does exist in the syntax (Clahsen, 1991). Therefore only languages including German, which select (a) construct an AGR phrase.

Finally, Clahsen's third parameter, the Recovery Parameter, determines whether or not AGR can identify or recover the features of a null subject (pro). Clahsen assumes that null subjects must both be licensed and identified. Government by INFL (AGR) licenses the empty subject, and person and number features in AGR identify the missing subject. Government relations between AGR and the subject position determines whether AGR can or cannot recover person and number features. When AGR can recover these features, [+pronominal] is selected, and when AGR cannot, [-pronominal] is chosen. Since German only permits expletive null subjects which do not require person and number features, German licenses null subjects, but cannot recover these features from pro. German therefore selects the [-pronominal] value for the Recovery Parameter.

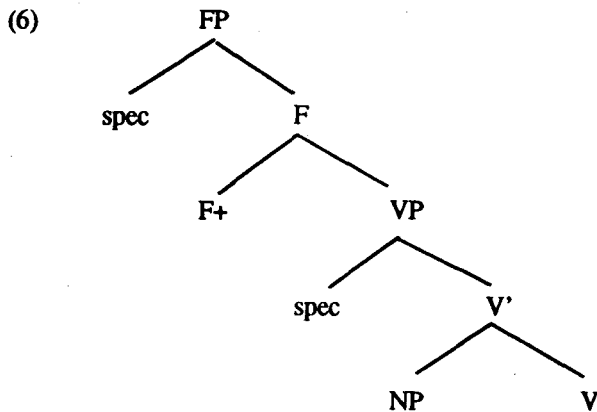
Clahsen's uses the parameters in (2) to account for the developmental shift seen between Stages II/III and Stage IV in German child language development. (5) summarizes the characteristics of this developmental shift.

- (5) Stage II/III  
 no subject verb agreement  
 no agreement markings  
 - t functions as a transitivity marker  
 modals in second position  
 verb final and verb second patterns  
 - uninflected verbs in final position  
 - inflected verbs in second position  
 grammatical subjects often absent

#### Stage IV

subject-verb agreement  
agreement paradigm acquired  
appearance of embedded clauses  
verb placement correct in matrix and embedded clauses  
grammatical subjects realized

Since syntactic structures and not parameters mature, Clahsen proposes that in Stages II/III children have the tree in (6) making the parameters in (2) inapplicable.

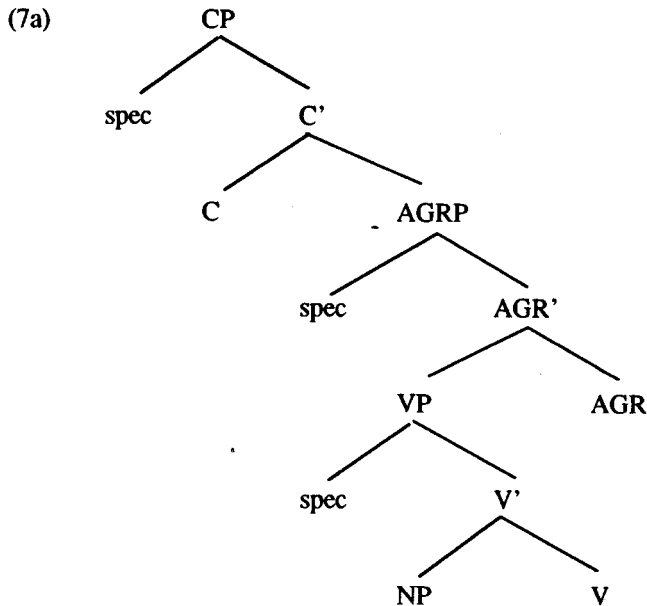


First, without either AGR or COMP, the V2 parameter cannot be set. Second, the AGR parameter cannot yet be set, since setting it now would require re-setting it later when agreement is acquired thereby violating the Parameterization Constraint. Third, without AGR, there are no person and number features the child could use to identify subjects. Instead [+F] licenses empty subjects (Clahsen, 1991). Therefore, the Recovery parameter is also inaccessible making null subjects permissible.

Without the three parameters in (2), Clahsen accounts for the Stage II/III data as follows:

1. Without AGRP, there can be no movement from AGR to [+F], and without this movement, the interpretation of [+F] cannot involve subject agreement, and therefore /-t/ marks intransitivity, not agreement. 2. Modals and the /-t/ affix are lexically specified as [+F] and base generated in F. /-t/ triggers movement from V to F explaining the occurrence of inflected verbs in second position.

3. (spec,FP) functions as a topicalized position allowing both subjects and objects.<sup>6</sup> Therefore when either modals or inflected verbs occupy [+F], subjects must move from their D-structure position in (spec,VP) to FP to derive the V2 word order seen with these elements, and 4. null subjects are allowed simply due to the unavailability of the Recovery Parameter, although Clahsen's does not discuss how null subjects are identified without this parameter. In Stage IV, learners have the syntactic structure in (7a) and the parameter settings in (7b).



- (7b)
- a. V2 Parameter: [+F] is in COMP
  - b. AGR Parameter: AGR exists in the syntax
  - c. Recovery Parameter: [-pronominal]

According to Clahsen, once learners have acquired the /-st/ agreement ending, they have the full subject verb agreement paradigm making it possible to set the parameters in (2). Since learners now have agreement, AGRP exists in the syntax and agreement inflections are base-generated as

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<sup>6</sup> Following Diesing (1990), (spec,F?) and (spec,CP) function both as an A and A' position.

AGR. Accordingly, finite verbs must move to AGR to pick up inflection. The acquisition of the agreement paradigm also triggers the re-analysis of modals as main verbs, since modals like finite verbs bear inflection in German. Therefore, modals are now generated under V and also move to AGR to pick up inflection. Once complementizers emerge, CP replaces FP, and the V2 parameter is set, placing [+F] in COMP. Since the syntax now has an AGRP containing inflected finite verbs, and since [+F] must be lexicalized, the development of CP also triggers movement from AGR to C when there is no complementizer. Finally, now that AGR is available to children, person and number features are also available and the Recovery parameter is set to [-pronominal] causing the observed increase in overt subjects. Having set the parameters as indicated in (7b), the learner has now acquired the adult grammar.

### 3.0 PROBLEMS WITH CLAHSSEN'S ANALYSIS

Although Clahsen's analysis does seem to account for the German acquisition data, it has some serious theoretical problems. First, the child grammar presented in (5) violates UG. While child grammars may differ from adult grammars, child grammars are external to UG only if these grammars are found exclusively in child grammars. Guilfoyle and Noonan's (1992) lexical grammar is within UG, since lexical grammars have been proposed for languages such as Japanese which lacks functional categories. However, Clahsen's grammar is outside UG, since it contains a functional category (FP) not found in any adult grammar (Hoekstra, 1992).

Second, Clahsen ignores evidence from Stage I which indicates that if there is a verb it is uninflected and generally in final position.<sup>7</sup> This evidence suggests that at this developmental stage, child grammars lack both an FP and an AGRP making them lexical. As well, Clahsen's proposal that AGR is inserted between the VP and CP, increases the computational demands on the learner: it seems computationally less difficult to build syntactic trees from the bottom up as Guilfoyle and Noonan (1992) propose.<sup>8</sup>

Third, Clahsen does not specify why I+[F] must govern nominative case. It may be that this government requirement motivates the movement of a subject to a topicalized position. However, when subjects are not topicalized as in embedded clauses, nominative case would not be governed, incorrectly predicting such sentences to be ungrammatical. This is only a problem in Stage IV, since without AGRP, it seems unlikely that case has appeared in Stage II/III. Fourth, Clahsen (1986) presents evidence showing that although learners in Stages II/III make errors and

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<sup>7</sup>This Stage I data is being debated.

<sup>8</sup>While this makes good intuitive sense, it requires supporting evidence.

overgeneralizations, children do use inflectional affixes in Stage II and this usage gradually increases throughout Stage III. However Clahsen (1986) argues that because learners do make errors, in Stages II/III, /-t/ does not encode subject-verb agreement, but indicates intransitivity: /-t/ is found only with intransitive verbs. Weissenborn (1990) argues that finiteness not agreement triggers the acquisition of V2 word orders. Weissenborn shows that the suffix /-t/ is not limited to intransitive verbs, and that inflectional morphemes other than /-t/ trigger verb movement in Stages II/III. Weissenborn also presents sentences exhibiting postverbal negation suggesting that verbs move from their initial preverbal position.<sup>9</sup> Some of examples of each evidence type are given in (8a), (8b) and (8c).<sup>10</sup>

(8a) Transitive Verbs

kauft Angela  
buys Angela

mal t eier  
paints eggs

vogelein singt was  
bird sings something

(8b) Other Inflectional Affixes

backe kuchen  
bake(1 pers.sg) cake

machen knodel  
make (1 pers.pl) dumpling

grunen passen nicht  
green fit (3 pers. pl) not

(8c) Postverbal Negation

brauche nich lala  
need not pacifier

glaub nich  
believe not

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<sup>9</sup> Jordens (1990) presents similar evidence supporting Weissenborn's claims.

<sup>10</sup> Weissenborn (1990) assumes phonologically null subjects empty sentence initial positions.

Weissenborn claims once children have acquired the finite/non-finite distinction, they move all finite verbs to second position regardless of how they are inflected. According to Weissenborn, children acquire and master the agreement paradigm independently of verb movement allowing learners to omit inflections, use incorrect inflections, and make overgeneralizations which Clahsen's account prohibits, but which is evident in the acquisition data.

Fifth, Clahsen claims that modals are initially base generated under [+F], and that once the agreement paradigm has been acquired, modals are re-analyzed as main verbs. If modals are base generated under [+F], then Clahsen's analysis violates Pollock's (1989) ban on vacuous quantification. (Weissenborn, 1990) That is, there is no empty category binding [+F], since both [+F] and modals are generated in the same position. As shown in (3a) and (3b) only when modals originate in the VP, can [+F] be bound by a variable, since binding requires movement to generate a c-commanding co-indexed trace. However, this lack of movement might not be a truly valid problem, since Clahsen links agreement with movement, and in Stage II/III, both movement and agreement are absent.

Sixth, and relatedly, Weissenborn argues that learners initially analyze modals as main verbs, and not as a [+F] elements. First, German modals function as finite verbs in that they are also inflected for subject-verb agreement. Second, German modals like main verbs select an NP complement. Therefore, if modals are lexically specified as [+F], they should not be found with NP complements. Weissenborn presents evidence against this prediction. Some examples are given in (9).

(9) Modal Verb + NP-Complement

will auch ein ball  
want also a ball

Mone mag des  
Mone likes this

muss das, Mama  
must this, Mama

Since (9) shows that modals select both a VP and an NP, modals cannot be base-generated as [+F] which only selects a VP.

Seventh, and finally, as Clahsen himself acknowledges, it is unclear what triggering data is required to set the Recovery Parameter to [-pronominal] for German. Following Hyams (1986), Clahsen tentatively suggests that overt expletive pronouns may function as the trigger for setting



the Recovery parameter. This suggestion may have some merit, since expletive pronouns occur in subject position and therefore within the government domain of AGR maintaining Clahsen's restriction on triggering data. However, more data is needed to determine if a correlation between the use of expletives and overt subject exists (Clahsen, 1991). As well, since German does allow null expletives, this would not be a particularly salient cue. In the next section, I show that there may be a more readily available trigger for the Recovery parameter which still obeys Clahsen's Parameterization Constraint.

#### 4.0 AN ALTERNATE ANALYSIS OF THE GERMAN DATA

In this section, I present an alternate analysis of the German acquisition data, which accounts for Stages I through IV of German language development, and which avoids the problems found in Clahsen's analysis.

##### 4.1 Theoretical Assumptions

I adopt Guilfoyle and Noonan's (1992) Structure Building Hypothesis which claims that since functional categories emerge after lexical categories, children's early grammars lack both functional categories and the maximal projections containing these categories. I follow Guilfoyle and Noonan in assuming that syntactic development requires both continuity and maturation. That is, while triggering data is always available, learners can not use it until they have matured in some way. Following Hyams and Jaeggli (1990), I assume that null subjects must both be licensed and identified. A rich inflection system licenses null subjects, and a case-governing category containing person and number features identifies pro. In having a rich inflection system, German licenses null subjects. This analysis is not entirely satisfactory, since there is no way for children to determine whether or not their language has a rich inflection system without comparing it to the inflectional systems of other languages, and children are not "little linguists". Guilfoyle and Noonan (1992) propose an analysis relating the development of IP to the disappearance of null subjects. While this proposal avoids the triggering problem altogether, I adopt Hyams and Jaeggli's analysis to show that a functional analysis may also help solve their triggering problem. However, since AGR assigns case and since TENSE (+F) containing person and number features is in COMP, German does not identify pro. Following Pollock (1989), I assume that INFL is split into TP and AGRP. However, since there is insufficient data on the development of TP, I follow Clahsen (1991) in omitting this phrase from syntactic trees.<sup>11</sup> In the absence of TP, I assume that AGR assigns nominative case. Following current work in GB theory, I assume that subjects are generated VP internally and move to (spec, AGRP) to satisfy the Case Filter. Following Pollock (1989), I assume that [+F] is an operator which must be bound by a variable, but which does not govern nominative case as in Clahsen's analysis. I also assume that modals are main verbs which select a V' complement. Finally, I adopt Clahsen's three parameters: Verb-second, AGR, and

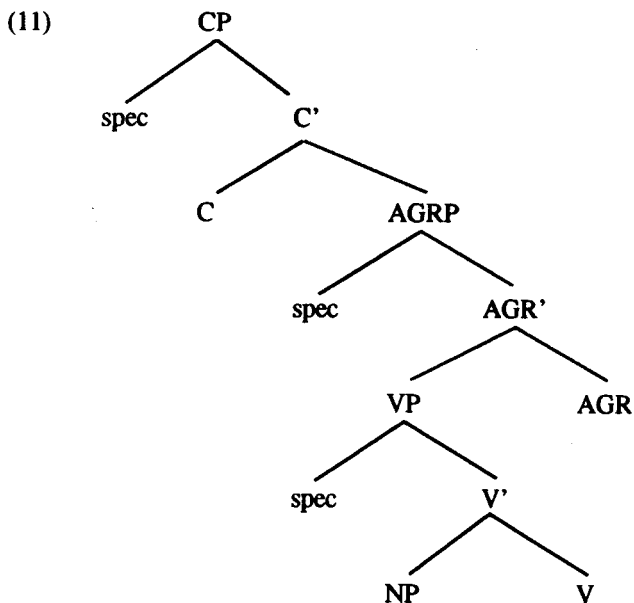
Recovery parameter with the default settings in (10)

- (10)    Verb Second:    [+F] is in AGRP  
           AGR:            does not exist in the syntax  
           Recovery:       AGR is [+pronominal]

I assume the same adult settings for the three parameters as Clahsen: [+F] is in C, AGR exists in the syntax, and AGR is [-pronominal] and therefore cannot recover person and number features from pro.

## 4.2 The Adult Grammar

The assumptions given in 4.1 generate the same adult syntactic tree Clahsen proposes in (7a) repeated here as (11)

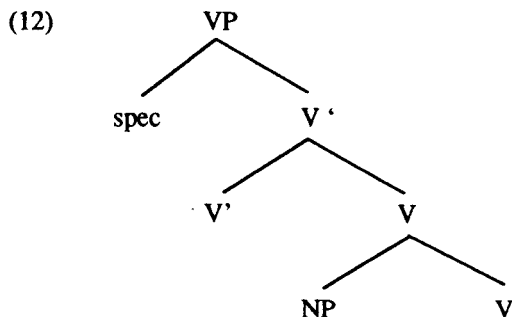



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11        While Meisel and Muller (1992) discuss both the development of TP and AGRP, this development pertains to German children learning both German and French, and therefore may not be altogether similar to the development of syntax in German monolinguals.

At D-structure, the verb occupies V and the subject (spec,VP). To derive the correct S-structure, the subject moves from (spec,VP) to (spec,AGR) to receive nominative case, and the verb moves from V to AGR to receive inflection and then to C to fulfil the lexical requirement on [+F]. Finally, either the subject, or another constituent is topicalized to (spec,CP). These movements generate V2 word orders in matrix clauses. If an overt complementizer occupies C, the finite verb remains in AGR. Eliminating movement from AGR to C generates the verb final pattern seen in embedded clauses.

Verb phrases containing modal verbs have the D-structure shown in (12).



Modal verbs are generated as the highest verb in the tree, and therefore modals and not the main verb move up to AGR to pick up inflection, since only if the highest V moves is the c-command requirement on traces satisfied (Diesing, 1990). Once inflected, the modal then moves to C to satisfy the lexical requirement on [+F]. Again, if C is filled with a complementizer, the modal remains in AGR. This generates the surface structures shown in (13a) and (13b).

- (13a) Ich will das Buch lesen.  
 I want the book to read  
 I want to read the book.

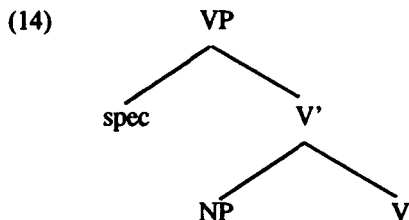
- (13b) Die Frau hat gesagt, [dass sie das Buch lesen muss]  
 the woman had said that she the book to read must  
 The woman said that she must read the book.

In (13a), the modal verb [will] moves to second position in the sentence ( $V \rightarrow \text{AGR} \rightarrow C$ ), with the main verb [lesen] remaining in final position. In (13b), only the modal [muss] moves to AGR, since COMP is filled, leaving it, and not the main verb [lesen] in sentence final position. Finally, null subjects are impermissible, since AGR assigns case, but [+F] is in COMP thereby failing to satisfy Hyams and Jaeggli's identification of a case governing category containing person and

number features.

### 4.3 Children's Grammar

In Stage I of German language acquisition, modals and subjects are absent, and verbs are uninflected and in final position. This data suggests that children's initial syntactic structures are purely lexical as shown in (14).



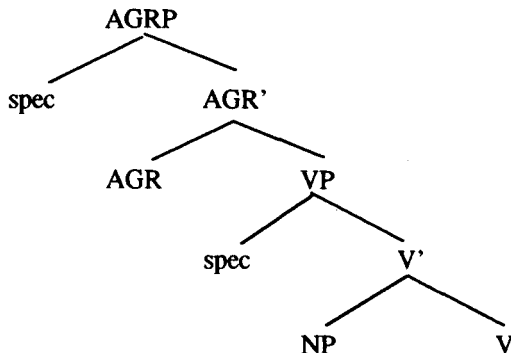
Since there are no agreement morphemes, AGR and therefore AGRP does not exist in the syntax. Without either AGRP, or CP, the V2 parameter is inapplicable and the verb remains in final position. Without AGRP, the Recovery Parameter is irrelevant. Null subjects therefore are also permitted, however it is unclear how these subjects are identified without AGR.<sup>12</sup> As well without AGRP, the case filter is not violated, since case does not exist, and therefore cannot be assigned.

In Stage II/III inflection begins to emerge, modals occur in second position, and verbs begin to move to second position. Weissenborn's data in (8) suggests that the finite/non-finite distinction has emerged. This evidence all suggests that learners have now set the AGR parameter to include AGRP in the syntax. Children have the syntactic structure in (15).

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<sup>12</sup> Hyams (1986) suggests that children use a discourse topic to identify subjects in this stage.

(15)



Since AGRP has emerged, agreement morphemes are generated under AGR, and finite verbs move to AGR to receive inflection. Notice that AGRP must be head-initial at this stage to derive the correct word order. This hypothesis that AGR is first head-initial might follow from the default setting of White's (1991) headedness parameter. With the development of AGRP, the default setting of the V2 parameter is also available. Since [+F] occupies AGR, finite verbs move from V to AGR, even in the absence of inflection, as Weissenborn's data suggests. However, since verbs do not always move, learners would appear to be occasionally violating universal grammar. While I do not have an explanation for this apparent violation of UG, it may be that learners have two competing analyses: one with AGR head-initial and one with AGR head-final. However this proposal requires more research to determine whether it violates Clahsen's Parameter Re-setting Constraint. Modals also emerge in this stage, correctly analyzed as main verbs, and like finite verbs move to AGR for inflection, and to derive the verb second orders observed with these grammatical elements. As well, now that AGRP is available, subjects move from (spec,VP) to (spec,AGRP) to receive case from AGR. Null subjects, however are still permitted, since AGRP both assigns case to the subject position and contains [+F]. AGR therefore still has the default setting: [+pronominal].

In Stage IV, complementizers develop suggesting that CP has now emerged and that the learners have the adult structure shown in (11). Children now also notice that when clauses contain a complementizer, the finite verb does not move triggering the adult setting of the V2 parameter. With [+F] in COMP, word order is also correct in embedded clauses. Notice that AGRP is now head-final. According to Miesel and Muller (1992), AGRP was only head-initial to derive the V2 word orders seen in Stage II/III, and since [+F] is now in COMP and derives V2 orders, AGRP is head-final.<sup>13</sup> With the emergence of CP, null subjects can no longer be identified, since AGRP

<sup>13</sup> Miesel and Muller use this argument for the restructuring of TP, however, in the absence of TP, it also applies to AGRP.

assigns case, but [+F] is in COMP. The Recovery parameter therefore is set to [-pronominal] and overt subjects consistently appear.

Notice that this analysis of the German acquisition data solves most of the problems outlined in Section 3.0. First, following Guilfoyle and Noonan (1992), lexical grammars do not violate UG. Second, since syntactic structure is built from the bottom up, computational difficulties are eased. Third, since [+F] does not govern nominative case, sentences with embedded clauses are correctly predicted to be grammatical. Fourth, since verb movement is now linked to the acquisition of the finite/non-finite distinction instead of agreement, Weissenborn's data in (8) is accounted for. Fifth, since modals are initially analyzed as verbs, the data in (9) is also accounted for. Sixth, since modals originate in the VP and move to AGR, and then to C, the operator analysis is satisfied in all acquisition stages. Finally, the null subject analysis has provides a salient trigger for the appearance of overt subjects: the identification of COMP.

## 5.0 CONCLUSIONS

This paper set out to show that a functional category analysis of the acquisition of German word order and overt subjects avoids the problems found in Clahsen's (1991) analysis of the German language developmental stages while maintaining Clahsen's constraints on parameter theory. While the previous sections presented arguments in favour of the former, this discussion focuses on the latter. First, all three parameters refer to heads: [+F] is placed in either AGR or COMP, AGRP is only built if its head AGR exists in the language, and finally the heads AGR and C both determine whether *pro* can be identified making null subjects permissible. Second, parameters are never re-set: parameters are either inapplicable at a certain stage in the language development, or the default setting applies, and since once a parameter setting is chosen, it never changes, acquisition steadily progresses towards the adult grammar. Third, all three parameters are available throughout the acquisition process, although without the required syntactic structure, they are not always applicable. This lack of syntactic structure accounts for why parameters are set in different developmental stages.

Clahsen's learnability constraints have at least two implications for the triggering data needed to set parameters. First, it is unclear how Clahsen's Parameterization Constraint restricts triggering data to government domains of parameter heads. For example, if the child's grammar does not contain AGR, the government domain of AGR can never be used to set Clahsen's three parameters (i.e. Verb Second, AGR, and Recovery) as he proposes. Second, as discussed in Section 2.0, Clahsen's analysis cannot account for why data that was always available suddenly functions as a trigger. Therefore, Clahsen's analysis fails to solve the triggering problem. In contrast, Guilfoyle and Noonan's Structure Building Hypothesis proposes a combination of

continuity and maturation. According to this proposal, AGRP cannot develop until learners have matured enough to pay attention to the differing behavior of finite and non-finite verbs in matrix clauses. Similarly, CP cannot develop until learners are able to acquire complementizers and to notice the relationship between complementizers and word order in embedded clauses, since according to Clahsen, children never make word order mistakes in embedded clauses.<sup>14</sup> A functional category analysis of the acquisition of German word order and overt subjects then not only avoids both the above triggering problem and the government domain restriction on triggering data, but also accounts for language development within a UG framework while still obeying Clahsen's proposals to constrain parameters.

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<sup>14</sup> This claim may also be debatable, since Miesel and Muller (1992) present evidence showing that some children may in fact make word order errors in embedded clauses.

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