

# **Variation, Selection, Development: Probing the Evolutionary Model of Language Change**

*Edited by  
Regine Eckhardt  
Gerhard Jäger  
Tonjes Veenstra*

**Mouton de Gruyter**

## Variation, Selection, Development



# Trends in Linguistics

## Studies and Monographs 197

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Berlin · New York

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Mouton de Gruyter (formerly Mouton, The Hague)  
is a Division of Walter de Gruyter GmbH & Co. KG, Berlin.

⊗ Printed on acid-free paper which falls within the guidelines  
of the ANSI to ensure permanence and durability.

*Library of Congress Cataloging-in-Publication Data*

Variation, selection, development : probing the evolutionary model of  
language change / edited by Regine Eckardt, Gerhard Jäger, Tonjes  
Veenstra.

p. cm. — (Trends in linguistics. studies and monographs ; 197)

Includes bibliographical references and index.

ISBN 978-3-11-019869-0 (hardcover : alk. paper)

1. Linguistic change. 2. Creole dialects. 3. Language and  
languages — Variation. 4. Language and languages — Origin.

I. Eckardt, Regine. II. Jäger, Gerhard. III. Veenstra, Tonjes,  
1962—

P142.V37 2008

417'.7—dc22

2007050434

ISBN 978-3-11-019869-0

ISSN 1861-4302

*Bibliographic information published by the Deutsche Nationalbibliothek*

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie;  
detailed bibliographic data are available in the Internet at <http://dnb.d-nb.de>.

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Cover design: Christopher Schneider, Berlin.

Printed in Germany.

The present volume emerged from the contributions to the 4. Blankensee-Colloquium which took place on July 14–16, 2005 in Berlin-Schmöckwitz. The Blankensee Colloquium series is financed by a cooperation funds in agreement with the senate of Berlin. The presidents and rectors of the Free University Berlin, the Humboldt University Berlin, the Technical University Berlin, of the Berlin-Brandenburg Academy of Sciences, the Berlin Research Centre of Social Sciences, and the Wissenschaftskolleg zu Berlin, are responsible for the program of the series. The annual topic is selected in a competitive call for proposals for an international conference in the humanities and social sciences on the issue of “Cultural and Social Change”.

It is the aim of Blankensee Colloquia to offer young scholars from the region Berlin-Brandenburg the opportunity to present and develop innovative own topics for research in the context of an international conference. The funding program was devised to strengthen the profile of the scientific landscape in the Berlin-Brandenburg area, and to support institutional cooperations in the region.



# Contents

|                       |   |
|-----------------------|---|
| Introduction.....     | 1 |
| <i>Regine Eckardt</i> |   |

## Survey

|                                                                                           |    |
|-------------------------------------------------------------------------------------------|----|
| Language change as cultural evolution:<br>Evolutionary approaches to language change..... | 23 |
| <i>Anette Rosenbach</i>                                                                   |    |

## Formal Approaches

|                                                                       |     |
|-----------------------------------------------------------------------|-----|
| Language change as a source of word order correlations.....           | 75  |
| <i>Brady Clark, Matthew Goldrick, Kenneth Konopka</i>                 |     |
| Evolutionary motivations for semantic universals.....                 | 103 |
| <i>Robert van Rooij</i>                                               |     |
| Back to nature or nurture: Using computer models in creole genesis... | 143 |
| <i>Teresa Satterfield</i>                                             |     |

## Forces in Language Change

|                                                                                                                                                            |     |
|------------------------------------------------------------------------------------------------------------------------------------------------------------|-----|
| Economy of Merge and grammaticalization: Two steps in the<br>evolution of language .....                                                                   | 179 |
| <i>Elly van Gelderen</i>                                                                                                                                   |     |
| Prehistoric and posthistoric language in oblivion .....                                                                                                    | 199 |
| <i>Wouter Kusters</i>                                                                                                                                      |     |
| Grammaticalization, constructions and the incremental development<br>of language: Suggestions from the development of Degree Modifiers<br>in English ..... | 219 |
| <i>Elizabeth Closs Traugott</i>                                                                                                                            |     |



**Cognitive Foundations**

The two faces of creole grammar and their implications for the  
origin of complex language..... 253  
*Alain Kihm*

Functional similarities between bimanual coordination and  
topic/comment structure ..... 307  
*Manfred Krifka*

Inflectional morphology and universal grammar:  
*post hoc* versus *propter hoc* ..... 337  
*John McWhorter*

Why don't apes point? ..... 375  
*Michael Tomasello*

List of contributors ..... 395

Index of subjects ..... 397

Index of languages ..... 400

Index of persons ..... 401

# Introduction

*Regine Eckardt*

The appeal of evolutionary models for change lies in the fact that they view changes as directed processes rather than arbitrary fluctuation. The vision that change in language, even though apparently arbitrary from a local perspective, might show general patterns on a larger scale has always fascinated scholars in historical linguistics. Georg von der Gabelentz (*Die Sprachwissenschaft*, 1891) explicated what was implicit in the work of many precursors and attributed most changes in language to two counteracting forces, the strive for brevity (*Bequemlichkeitstrieb*) and the need for clarity (*Deutlichkeitstrieb*). In evolutionary terms, these two could be conceptualized as opposed selective pressures, favouring different kinds of variant forms and operating with different strength at different times in different communities. Their conspiracy offers the conceptual basis for a range of models of non-random linguistic change that were proposed in the nineteenth century. Humboldt's (1822) four stage model led the way, proposing that languages without any grammatical forms or words (stage I) are enriched to languages where word order and specially recruited words express grammatical concepts (stage II), move on to languages where the functional elements tend to be agglutinated to the lexical elements (stage III, and one could hypothesize the strive for brevity in action here) and reach fully inflectional languages (stage IV). It is interesting to observe that the dynamics that lead from stage I to IV were conceptualized as continued improvements and maturation processes whereas all developments away from a stage IV language were perceived as *decay*. Humboldt and his followers hence compare "a language" to an organic body: from a simple infantile stage, "the language" reaches maturity, passes its best years and then moves toward the grave. This view is in stark mismatch with the evolutionary view that rests on the credo that all changes are always changes to the better, urged by changing external circumstances. Whatever the value of this salvation story, Humboldt's life-cycle metaphor was disqualified by at least two seriously wrong predictions that could be derived from it. Firstly, the main examples that fed Humboldt's views on stage IV languages were the languages of the Classical Antique – Greek and Latin – languages spoken in highly developed

cultures, and languages that seemed to change their structure in correspondence to the decay of the cultures in question. Broader typological investigations, as well as a debate that goes on till today, strongly suggest that there is no positive correlation between highly flectional grammars and highly developed cultures whatsoever. To the contrary, there appears to be a link between the secludedness of a society and the complexity of its language, as is discussed in much detail in the contribution by Kusters. This, of course, does not challenge Humboldt's four stages automatically, but at least the perception of language change as decay, paralleling the rotting of social and cultural structures, turns highly implausible.

Secondly, it was and is not clear whether the processes of decay that lead away from a stage IV language type are inevitable, or whether they can be stopped and reversed to a positive force again. This question is particularly vexing if we take into account that, surprisingly, no cultural *elite* at any time has ever judged *their own language* as being at its grammatical climax. In his recent popular introduction to language emergence, development and change, Guy Deutscher carefully and amusingly assembles quotes of esteemed thinkers throughout the centuries who assert that *their own* contemporaries showed frightening evidence of language decay whereas speakers of the language of some earlier time were still in full command of the grammar in all its subtleties. For which time, of course, Deutscher offers yet another guardian of language who observes that at this specific times, contemporaries showed frightening evidence of language decay whereas in earlier times ... (see Deutscher 2005: ch. 3). Deutscher clearly states the consequences of such individual statements: If these series of philologists had been right, many modern languages should be in a completely dysfunctional state after centuries of continued decay. Which, of course, does not correctly describe the state of any modern language, no matter how much its speakers may deplore its present form. So, whatever psychological motive may stand behind the very pervasive perception of "language decay", it can not have the status of a scientific insight about linguistic structure.

The fallacies of simple unidirectional trends were seen most clearly by linguists like Georg von der Gabelentz and Otto Jespersen. Both became known for their reformulations of Humboldt's unidirectional model in terms of a spiral model (von der Gabelentz) or a cycle, notably Jespersen's cycle of negation which offers a textbook example of the dynamic equilibrium between brevity and expressiveness. Such models come closer in spirit to evolutionary views because they cover the fact of never-ending changes in language. The last decade of the 20th century witnessed a renewed interest

in the emergence of grammatical forms, and Jespersen's cycle was complemented by a wide range of case studies of grammaticalization clines. The cline metaphor, once again, stresses the unidirectionality of the developments that affect a single construction or expression. The cline covers one turn in von der Gabelentz' spiral. The emergence of a new linguistic form for an old concept, once the older form has been reduced to a certain degree, is a "felicitous coincidence" rather than an inherent part of the cline metaphor. Bringing the evolutionary terminology to bear at this more microscopic level of investigation, we could propose that the apparently antagonistic selective forces towards brevity and expressiveness might apply to different sets of competing elements: While "brevity" within the cline leads to reduced variants and overuses of the same lexical form(s), "expressiveness" invites to move from one set of lexical forms to another.

In recent years, these theoretical considerations have been complemented by an entirely new way to approach language change in terms of evolutionary models. Mathematical models of processes of replication and selection, as well as related computer simulation software, allow to study the development of dynamical systems in many scientific domains. A growing community of researchers, inspired by the Edinburgh group around Jim Hurford, by Martin Nowak and his research group, and others (e.g. Briscoe 2003; Nowak et al. 1996, 2002, to name but a few) test the felicity of computer simulations of known instances of language development. The big question is, how well do these models fit their case, judging from the outcome of simulation experiments? Do simulations show the same trends as we know from the historical records? Which factors have to be taken into account to improve the match? What models of language acquisition turn out to be most suited? Does a notion of acquisition that amounts more or less to copying suffice, what is the influence of frequency effects in the input data, which other kinds of acquisition biases prove fruitful? How can an acquisition bias counteract the relative rareness of certain kinds of input structures; in what other ways can rare structures survive and spread? How frequently do dynamic systems reach stable mixed stages, equilibria with different coexisting grammars? The considerable number of factors that potentially influence the behaviour of evolutionary models suggests that the felicity of the approach can not be proved in one single study. The two papers in the present volume which operate in this framework, Brady Clark and colleagues, as well as Teresa Satterfield, offer results that contribute to this research program. A variant of the same kind of research is exemplified

in the article by Robert van Rooij. Investigations in the mathematical models in evolutionary game theory offer a theoretical counterpart to population software. Van Rooij studies stable signalling games and their development in dependence of the expected utility values of various types of situations that the signallers may face. Here, the utilities as well as the nature and range of competing messages and signals determine the outcome of the system; the success of the models hinges on the plausibility of the assumptions about utility values.

Both types of approach have in common that the modelled systems usually converge towards stable states. For some of the modelled developments, this prediction is acceptable without restrictions, for example van Rooij's analysis that human signalling games always yield convex concepts (in the sense of Gärdenfors 2000), or the prediction that compositional linguistic systems in the long run win over holistic signalling systems. The latter not only is another result in van Rooij's contribution but was also the focal point of Simon Kirby's presentation at the Blankensee Colloquium, "The mechanisms of adaptive linguistic evolution", which was not available in print for publication (see Kirby, 2003 for a presentation of the framework).

However, when simulations model more local historical developments, like for instance the English move from SOV/V2 to an SVO language in the contribution by Brady Clark, Matthew Goldrick and Kenneth Konopka, the maintenance of that stable end stage can not be accepted without qualifications. At this point, simulation studies share the open questions of the predecesing theories; yet the computational management of the dynamic systems promises the possibility that well-defined end stages of systems can be carried away to be turned into initial stages of new systems. The exciting question then will be, which changes to the system setup are necessary to initiate subsequent developments, and do they mirror known facts about sociohistory?

In some instances, finally, the modelled biases in population models may strongly depend on socio-cultural factors of the speaker community. Consider the fact that many simulations implement preferences towards simple, transparent, regular linguistic structure. Such preferences not only offer good results in large-scale developments like the one towards compositional grammar (see the studies in Briscoe 2003), but are also in accord with traditional linguistic hypotheses like, for instance, Lightfoot's transparency principle, or Elly van Gelderen's theory of economy driven language change, also discussed in her contribution in the present volume.

However, the empirical record looks different. Many independent studies suggest that there is a strong trend for languages to become complex, little transparent, and less economic (according to the measures assumed in computational studies) over time. This seems to happen specifically under circumstances of *undisturbed* developments, that is, in small, close-knit, comparatively isolated communities with little external linguistic influence. Comparative studies like those by Trudgill 1989, McWhorter 2001, Ross 2003, and also Kusters 2003 which is reported in its main results in the present article, offer sufficient evidence to the end that undisturbed linguistic evolution does not necessarily lead towards the simple, elegant but to the contrary, seems to prefer complexity. We acknowledge that there is no categorical distinction between languages of isolated communities, and languages of open societies, and that there are even clear counterexamples to the trend. However, the known facts are sufficient to disrepute any model which uniformly stipulates a bias towards simplicity and transparency. We feel that current simulation studies still fail to address this observation in a convincing manner.

In order to overcome strictly telic models of language change, it will be necessary to give room to the insight that the driving factors operate relative to the initial language stage. Interestingly, the paper by Elizabeth C. Traugott brings this fact to the fore, in completely traditional terms of historical linguistics, by pointing out the force of analogy in language change. The salience and success of new patterns in language depends on the presence or absence of other, similar structures in the current grammar; given that these change as well, the enhancing factor “analogy” for one and the same initial structure changes over time. Likewise, analogy could be the key term in explaining the influence of language contact without having to resort to blunt borrowing which often seems too narrow to capture facts successfully. Traugott suggests that the term “construction” might offer the appropriate ontology to analyse analogy. Thinking in terms of constructions, “analogy” amounts to speakers’ ability to compare chunks of sentences in terms of their closeness or dissimilarity. It is interesting to note that analogy currently receives renewed interest in historical linguistics (see e.g. Fischer 2007). The presentation of Bernd Heine at the colloquium, drawing on joint work with Hiroyuki Miyashita, likewise stressed the role of analogy in the emergence of German quasi-modals, exemplified on basis of the many uses of *drohen* (‘to threaten’) in present-day German.

Analogy may be of wider influence, though, than such strictly language internal investigations suggest. Two of the contributors point out intermodal analogies and their possible influence on the emergent language system in prehistory. Manfred Krifka draws attention to the strong similarities between the organization of bimanual coordination, universally divided into a dominant “figure” hand, and an inferior “ground” hand, and the division of information into theme and rheme. Information structure constitutes one of the overarching universal structuring principles in human languages, and typological studies have repeatedly confirmed the shaping force of information structure in grammars of languages. Alain Kihm, in contrast, rests his study on the universal biological facts of sound production and perception, leading to the universal concept of syllable structure. He proposes that syntactic proto-structures model these phonological patterns, basing his argument on Creole languages which, according to his view, reveal universals most clearly. It needs to be stressed, as Kihm himself does in his article, that he does not view Creoles as default instantiations of UG in the sense of Bickerton (1984) and Roberts (1999).

Moving even further from language internal considerations, the conference program comprised two contributions which addressed the biological foundations of the language capacity in humans. While such considerations may not as yet have direct impact on language internal studies, advances in evolutionary biology have led to significant insights in our understanding of the emergence of language, relativising or refuting earlier hypotheses on the issue. The presentation “A biological perspective on the evolution of language” by Tecumseh Fitch reviewed facts about the emergence of language in the light of modern evolutionary biology. Comparing the selected features in the evolution of higher mammals and humans, Fitch argued that the development of human larynx is selected for because low pitch of voice, the size of individuals and the strength of individuals are positively correlated. This proposal is confirmed indirectly by the observation that males, who are involved in most competitive activity in human social groups, develop an even lower pitched voice which typically arises in puberty. Other facts in the time scale of language acquisition, in turn, suggest that the language capacity as such was not selected for for reasons that have to do with mating success. Specifically, the acquisition device unlike other learning capacities closes down dramatically around age 10 when the individual approaches sexual maturity. This stands against language as a factor in mating; studies in evolutionary biology have shown that aspects in pheno-

type that are relevant for mating typically only start to show up in puberty. In addition, Fitch argued, theories to the end that male courting behaviour was in part verbal, and verbosity guaranteed reproductive success, are falsified by the fact that language in females is, if anything, more skilled than in males (see also Fitch 2005).

Michael Tomasello finally drew attention to a very elementary biological trait that needs to evolve before language can: In brief, his presentation amounts to the insight that language can not emerge without the intention to talk.

We now proceed to offer short summaries of the papers and pointing out common topics as we saw them in the course of the publication project.

The collection is led by *Anette Rosenbach's* article which offers a very careful introduction into the merits and depths of transferring the evolutionary metaphor to the case of language change. Rosenbach starts from the classical Darwinian model of evolutionary processes and proceeds to a meticulous discussion of each of the central terms both in the original (biological) framework as well as in linguistic adaptations. The article surveys a substantial body of recent approaches in linguistics which rest on the evolutionary metaphor. Special attention is devoted to the question of selection: What forces support the survival of one item or construction at the cost of another? What are the items that replicate? After demonstrating the inapplicability of any narrowly biological conception of replication, Rosenbach raises the hypothesis that priming effects be one factor that drives the replication rate of linguistic items in communication. Recent findings on priming at all linguistic levels and its interaction with implicit learning, Rosenbach argues, offer a basis not only for the notion of replication but also an explanation for the unidirectionality of grammaticalization processes. Priming, being inherently asymmetric as well, can lead from one construction to another but not the other way round. A survey of attested pathways of change can easily confirm that directions of language change are mostly in concord with directions of primes. In addition, Rosenbach's analysis suggests interesting new links between psycholinguistic experimenting and diachronic linguistics.

Rosenbach does not aim at taking a stand as to which of these approaches are supposed to be more fruitful than others. The breadth and diversity of studies and theories, to the contrary, cautions the reader against



the misconception that there be one and just one evolutionary theory of language change, and that this one theory could be judged “right” or “wrong” in a simple fashion. In view of the fact that the collection as a whole likewise offers a concert of different ways of reconstructing variation, selection and evolution in the realm of language, Rosenbach’s survey is apt to list a number of relevant questions not only to the theories that she discusses herself, but also to the subsequent articles that constitute the body of the volume.

### **Formal approaches**

This part of the volume presents applications of formal theories of evolutionary processes on questions of language development. Two of the studies (Satterfield and Clark et al.) rest on simulations of evolutionary developments and instantiate these with linguistic parameters. Experimental studies like these allow us to evaluate the appropriateness of simulatory models for issues in linguistics, by comparing the predictions of the simulation to the historically attested developments. To the degree that earlier studies promise a good match between simulation and reality – which has actually been established in many fields, as the authors point out – simulations also allow to evaluate the influence of various factors in language developments and eventually decide on the appropriateness of theoretical claims.

The third study by van Rooij is entirely theoretical and offers an analysis of various universal developments in terms of evolutionary game theory.

*Teresa Satterfield* investigates the development of Sranan, one of the major English-related creole language spoken in Surinam, and specifically aims at discriminating between various possible creolization scenarios that were proposed in the literature. She carefully describes the linguistic properties of Sranan, as well as the socio-historical background of the emergence of Sranan. Satterfield investigates the question whether Sranan as it is known today could have come about by imperfect L2 acquisition in a language contact situation. In order to come to an answer, she builds a simulation model on the basis of an assumed L2 contact scenario and compares the predicted linguistic developments with the actual outcome of the real process. The article offers an accessible presentation of the simulation software used, and details the parameters that were implemented. The reported simulation runs illustrate that the multilevel interaction between even a comparatively small number of parameters leads to unpredictable and unexpected out-

comes, showing that the use of simulation software indeed constitutes a qualitative leap in Creole research.

Satterfield then proceeds to a linguistic evaluation of the simulation. The implemented model, resting on an imperfect L2 acquisition theory of creolization, does not show similarities to the actual historical developments. Under the assumption that the main interactions were correct, she proposes to conclude that the respective theory does not make empirically valid predictions. It is important to note that the proviso that the simulation models as such might be mistaken is not a very strong one. Satterfield can relate her study to related simulation studies, resting on the same software but on basis of other creolization models, notably those where L1 acquisition and/or bilingualism play a role. Given that those simulations match the real facts more closely, we and the author are justified to conclude that the research paradigm can successfully discriminate between different theories of creolization.

The article by *Brady Clark, Matthew Goldrick and Kenneth Konopka* addresses word order variation in English, specifically the development from a verb-final (OV & VAux) to an SVO language. The authors aim to devise a simulation model that mirrors the development, starting from the assumption that subsequent populations of biased variational learners reshape grammar in each acquisition cycle. The article presents and discusses filtered learning models by Kirby and Briscoe, which provide the basis for an implementation of a dynamic system of learners/speakers who acquire and transmit grammars with different features during their life cycles. The eventual simulations show how biases for certain kinds of structure can shift the ratio of usages, and offer first evidence about the rate of change in correlation to other factors. The study is presented in a very accessible and self-contained manner, which allows even the outside reader to understand the merits and difficulties of this kind of simulation study. Even though the rhetorics of the article may be otherwise at times, the most fascinating insight provided by this and related research, to our eye, consists in a deeper understanding of the intricate and surprising interactions and long term developments that emerge even under very simple initial conditions. Studies like this one may caution the reader against postulating factors in language change too freely and lavishly. One great advantage of computer aided research in language change seems to be that the effects of various hypothesized sources of influence can be tested and weighted against each other in an explicit, step by step fashion.

*Robert van Rooij's* contribution is part of his larger project to reconstruct the emergence of linguistic features in terms of evolutionary game theory. Following a classical proposal by David Lewis, human communication is perceived as a signalling game where sender and receiver optimize their form-meaning pairings ("strategies") according to the expected utilities. These interactions converge on points of optimal utility (evolutionary stable strategies), and it is part of the project to argue that under reasonable assumptions about what is useful for a human agent, the approach reveals linguistic universals as points of convergence in signalling games.

Van Rooij starts by considering the emergence of convex properties, a human universal which, as van Rooij argues, rests on two factors. First, we have to acknowledge that human perceptions are organized in a topological manner. We evaluate objects, events and situations in terms of similarity and dissimilarity. The author, as many others, takes this as a given. However, on basis of this observation it is still surprising why human languages not only sometimes but more or less universally decide to lexicalize only convex properties in topological space. Van Rooij shows that convex properties (or, more globally, Vorronoy tessellations) automatically result from evolutionary stable signalling strategies in the effort of signaller and interpreter to optimize the utility of the interaction. Similarly, the trend to interpret joint signals in a compositional manner can be shown to emerge as the meta-strategy with the highest utility. This result goes substantially beyond earlier approaches to compositionality in the literature in that earlier authors only proved the superiority of compositional languages in contrast to holistic languages *given that both were existent and competing*. However, if one assumes a proto-stage of linguistic exchange in a holistic manner (comparable, perhaps, to primate calling), there is no compositional competing linguistic system in the first place, and hence none to win the competition against holistic communication systems. Van Rooij's scenario, in contrast, actually explains how compositional structures in language emerge from a stage of neutral signalling without building the result into the starting conditions of the development. In this sense, the paper contributes to our understanding of the minimal communicative prerequisites for the evolution of human language.

## Forces in language change

This subsection comprises contributions that mainly concentrate on the structural forces which define trends in language change. Interestingly, neither of these contributions operates on basis of the *post hoc* methodology to define a force that is named after its observed effects. To the contrary, all three papers propose independent factors in human thinking and interaction that lead patterns of observed linguistic change.

*Elly van Gelderen*, taking up classical views of David Lightfoot, proposes that innovation in language use tends to lead towards greater structural perspicuity. Hosting this hypothesis in the grammatical framework of minimalism (Chomsky 1995), van Gelderen not only offers a very explicit and testable formulation of diachronic economy principles, but moreover operates in a grammatical paradigm which allows to extend these links in an equally explicit manner to issues of language acquisition. She rephrases typical developments in terms of the structural categories that are passed by items in the course of their development. On basis of the standard categories of specifier, head, complement and adjunct, van Gelderen reveals trends in language change that cut across the investigation of traditional pathways of change, be it in grammatical terms (e.g. free word > clitic > affix) or in semantic terms (e.g. MOVEMENT > FUTURE). On basis of well-chosen examples, and with reference to more material that van Gelderen has brought to the fore in many earlier publications, she demonstrates how the notion of an item “turning into something more grammatical” can be reconstructed in terms of being a more or less integral part of the standard phrase structure schemes proposed in generative grammars.

In her article, *Elizabeth C. Traugott* brings yet another factor to the fore that defines language developments. She discusses the reanalysis of transparent measure constructions of the syntactic type [*a lot* [*of X*]<sub>PP</sub>] into internally unstructured modifiers like *a lot of* ‘many’. and demonstrates that semantically dissimilar source constructions can enter this kind of development, provided that their grammatical structure is of the illustrated type. After providing a very detailed empirical characterization of the patterns of change, Traugott proposes that the notion of a construction is central to an analysis of the development in question. Interestingly, Traugott’s envisaged use of constructions goes far beyond those aspects that any framework of a sound syntax-semantics interface could provide (e.g. HPSG, LFG, or descen-

dants of the well-known Montagovian map between form and meaning). She points out that speakers seem to be in command of a topology of constructions where similarity of newer constructions to older, known ones appears to be one of the driving forces in the developments we witness in language change. Traugott hence manages to integrate the classical notion of *analogy* into modern grammatical analyses, a notion which currently has received revived attention (see e.g. Fischer 2007). While single case studies often stress the uniqueness of each instance of language change in its own right, Traugott invites us to take a broader view and acknowledge conservativity in innovation: Speakers perceive patterns in language use long before these fossilize into grammatical structure. As they model innovative language use after such existent patterns, speakers are hence agents in an invisible-hand process<sup>1</sup> which supports the evolution of homogenous and consistent new stages in grammar.

*Wouter Kusters*, finally, takes up the controversial claim that the social structure of a society can find reflexes in the grammar of the language spoken. Observations in support of such theses have been forwarded, among others, by John McWhorter (2001), Peter Trudgill (1992), or Ross (2003). Others, however, have rejected such claims on basis of counterexamples (notably David Gil, discussing Riau Indonesian) or pointing out that there be no objective measure of a grammar's complexity at all. Drawing on richer material in his PhD thesis, Kusters offers yet another example for the contrast between small, tight-knit language communities (Type 1) and large open language communities (Type 2). He reports a positive correlation between measurable morphological reduction and regularization and the degree to which a community tends towards Type 2. His research is focussed on the development of variants of a source language and the difference between these varieties. It can be shown that certain social conditions favour the development of a more regularized variety. This kind of development nicely complements consistent findings in Pidgin and creolization scenarios that were discussed in earlier literature. On the one hand, Kusters' results will break up the close tie between Creole language and simple grammar that has caused a lot of heated debate in the literature. On the other hand, however, his findings as well as the wealth of other concordant work that he quotes offer support for a more general, and perhaps more interesting correlation between cultural openness and interaction on one side, and efficient

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<sup>1</sup> Our term, not Traugott's.

grammatical systems on the other side. Speculating about the far past, and the far future, Kusters ends his article with the obvious paradox that the oldest languages, following this rationale, must have been the most complex ones. This paradox can only be resolved if we include the stage where the genetic setup for modern languages itself emerged, a stage which Kusters explicitly excludes from his considerations.

## **Cognitive foundations**

This section comprises four articles which address the cognitive underpinnings of language, though in very different ways. The research of Tomasello mainly takes place in the experimental workshop of primate research, feeding the debate on the emergence of language with new and exciting results on specifically human abilities. Krifka's observations about analogies in the bipartite organization of information structure and the structure of bi-manual action rather lay the ground for future experimental studies. Kihm, on the other hand, develops on the old observation that universal structures in phonetics and phonology, notably the natural notion of the syllable, has provided the basic patterns in the emergence of syntactic proto-structure. Finally, McWhorter's challenging debate of the nature and (im-)possible biological implementations of syntactic parameters as part of Universal Grammar is purely theoretical in perspective. In carefully developing the logical consequences of current assumptions about the innateness of parameters, he reveals that many superficially plausible ways to combine concepts in theoretical syntax and biological evolution are in fact logically inconsistent.

In his article, *Michael Tomasello* presents a thorough series of studies which prove that apes don't point. The act of pointing for a conspecific (i.e. ape for ape, human for human, etc.) is dissected in different possibly independent abilities, which are then tested for big apes, and human infants. Specifically, Tomasello and his group take great care to distinguish "pointing for a conspecific" from other, similar abilities that are possessed by both, apes and humans, and that might be quoted as counterevidence to his claim in less careful investigations. Thus the group was able to show that apes can interpret other apes' or human trainers' activities with relevance to own goals (e.g. "will I find food where the other found food"), and that apes possess the ability of gaze-following. Still, in a series of carefully de-

signed experiments many of which are detailed in the article, Tomasello shows that apes, unlike humans from 9 months age on, are not able to understand the relevance of a pointing gesture of their trainer or ape, with the intention to convey information in a deictic manner. This fascinating series of experiments suggests strongly that aside from other language specific abilities that great apes and other higher mammals may lack, they importantly have never developed the very elementary, and apparently unsophisticated, ability to act with the specific intention of thereby conveying knowledge to others, and correspondingly, the ability to understand other individuals' actions as acts of intentional communication.

Tomasello's article offers fresh and challenging input in the debate about the origins of language, as well as the biological implementation of the abilities that together constitute Universal Grammar (in one of the many ways to define the concept). The study focusses on abilities that, to our knowledge, have never been seriously disputed in either philosophical or linguistic literature. In view of the fact that the ability of pointing seems so very elementary, and so little tied to intellectual achievements like the mastery of recursive structure or compositional language interpretation, one may wonder whether the series of genetic leaps towards language might not have taken a very different road from what is hypothesised in large parts of the current literature.

*Manfred Krifka* locates another analogy between pattern in languages and other human activities. He proposes that the split of sentence material into topic and comment, attested in most and perhaps all known languages of the world, is mirrored by a much more elementary split in the way in which bimanual action is organized. Krifka reports a broad range of results in neuromedicine and psychology which support the impression that humans coordinate bimanual action in an asymmetric manner. It has been observed that the asymmetric contribution of either hand in coordinate manipulation is not only defined action-per-action in a case by case fashion, but left and right hand appear to adopt the meta roles of a frame/background hand (typically non-dominant) and the figure/content hand (typically dominant) across many kinds of interaction. Krifka lists a wide range of ways in which this bipartition in manual coordination can be instantiated and compares these to the typical functional splits between topic and comment that were proposed in the linguistic literature. Surprisingly, Krifka argues, we find a striking similarity between the scientific terms that were proposed in order to describe one, and the other kind of asymmetry. This is all the more

relevant as, to our knowledge, none of the two fields of scholarly research has ever served as a model or metaphoric source domain for the other, and one may hence conclude that the overlaps in functional structure are genuine rather than superimposed for the sake of comparison. Krifka's observations with respect to the roots of information structuring, presented in both a detailed as well as accessible manner, point out new directions in the interdisciplinary investigation of the human language capacity.

*Alain Kihm's* article starts from the observation that Creole languages apparently show strong structural similarities to their lexifier languages, as far as NP structure is concerned. The VP structure, in contrast, is more universal across Creoles, and sometimes very different from the lexifiers, as Kihm shows on the basis of four Creole languages (Juba Arabic, Ngukurr Creole, Kriyol, Martiniquais) in detail. Kihm attempts to derive this split from Carstairs-McCarthy's (1999) hypothesis that language structure rests on the fundamental divide between sentence and noun phrase, which in turn patterns after syllable structure. However, he bases his considerations on a modified model of the syllable which rests on the dichotomy of nucleus and onset, which Kihm transfers to a syntactic dichotomy of noun phrase and verb phrase. The resulting predictions about sentence structure universals are tested on basis of Creole languages which, according to Kihm's relativized universal grammar hypothesis, are not exclusively but predominantly modeled after linguistic universals. On basis of a thorough and extremely detailed discussion of copula systems in Creoles, notably West African Portuguese Creoles, Kihm makes a strong point in favour of an independently emerging VP structure which mirrors neither lexifier language nor (as suggested by the relexification hypothesis) the substrate languages. He devises a grammaticalization pathway whereby resumptive pronouns (in the Pidgin stage) turn into predicate markers, which develop into copula. In conclusion, Kihm states that the grammatical function "marking the predicate" is universally exhibited in Creole languages, in one way or another, which lends support to the hypothesised NP-VP dichotomy. The internal structure of NP, in contrast, does not follow any universal patterns and hence borrows much more structural features from the superstrate.

*John McWhorter*, finally, offers a rich and sophisticated étude on the issue whether parameters, in the sense of Chomskian Universal Grammar, can rationally be considered genetic features that spread through selection in human populations. Resting his debate mainly on the exemplary case of



Baker's polysynthesis parameter as well as the related rich agreement parameter, his answer is – perhaps unsurprisingly – to the negative. However, the article's deeper insight consists in the demonstration that we need to be careful when uttering strong universals (even if plausible), and one should take care in using metaphors from biology, as well as making serious claims about the biological entrenchment of the human language capacity, in the specific form of UG. Taking McWhorter's prime case, he argues that the "inflectional parameter", seen as a genetic feature of humans, would have to be a biological trait that *allows* humans to perceive inflectional elements and to take them into account in their instantiation of UG. One would have to admit, though, that inflectional languages as such are not advantageous, because not all languages are inflectional, and specifically new languages usually are not highly inflectional. Yet, the story would go on, in spite of the indifferent value of inflection for survival (of both languages and communities, as far as we can tell), the genetic endowment to distinguish inflectional elements is universal. No genetic features are universal unless at least one of their effects on phenotype leads to a survival advantage. Therefore, if the polysynthesis parameter were selected for, we'd have to tell a story on why a feature which can lead to different phenotypes (= your native language competence) for which no phenotype is superior to others (= languages cover a broad range from analytic/isolating to polymorphic) should be universal in a population, leaving out not a single individual. McWhorter argues that no case in evolutionary biology known today would offer an analogue to this linguistically motivated claim. As an epilogue, McWhorter raises the question whether current formulations of UG parameters might be strongly influenced by the choice of languages that were first investigated in this framework. The article poses the challenge to search for more fundamental and general language capacities which are biologically selected for, whereas "parameters" are secondary features rather than driving forces in natural selection.

The present volume emerged from the fourth Blankensee Colloquium on "Language Evolution: Cognitive and Cultural Factors" in Berlin-Schmöckwitz in July 14–16, 2005, organized by R. Eckardt, G. Jäger and T. Veenstra. It reflects most of the talks given on that occasion, which were complemented by invited contributions in order to round out the spectrum of perspectives in the debate. The three organizers, all Berlin scholars at the time, and each representing one of the three focal areas – evolution, creolization, diachrony – initiated the meeting because we were equally puzzled and fas-

minated by the mutual implications of our three fields of research which we wanted to discuss and share with colleagues around the world. Even if some of our guests were initially hesitant to see their work linked to the term “evolution”, a term that has been famously disreputed since the 1866 ban of the French Academy of Sciences on contributions on the “evolution of language”, we think that our attempt to break this terminological taboo was fruitful in more than one respect. The unique mixture of scholars and schools in a hospitable and focused atmosphere enhanced the fruitful exchange of ideas between experts who otherwise work in disjoint camps. We feel that the present volume reflects the experimental spirit of the conference and its surrounding discussions in that several of the contributions spell out ideas that might not constitute canonical topics on the agenda of the respective fields of research. The collection likewise presents readers with old topics in a new setting, and cross-cuts the boundaries of traditional fields of investigation. We hope that the articles as such, as well as the references therein, may invite readers to take up new lines of thought.

The article “Why don’t apes point” by Michael Tomasello was originally published in the volume *Roots of Human Sociality. Culture, Cognition, and Interaction*, edited by Nicholas Enfield and Stephen Levinson, and published in 2006 with Berg Publishers. We thank the author, the editors and the publisher to give us permission for reprint, and hope that the connection may serve as invitation to explore the links between different fields.

We gratefully acknowledge the generous funding of the Blankensee Colloquium and the subsequent publication by the Berlin-Brandenburg cooperation funds, and warmly welcome the program committee’s decision to choose our proposal as the annual topic in 2005. Andreas Edel and Martin Garstecki of the Wissenschaftskolleg zu Berlin as well as Frau Schröder and Frau Obeth (GWZ Berlin) helped us in managing the administrative side, which we want to thank them for.

Our deepest gratitude goes to Katja Egli who, on several occasions, saved the conference from running aground.

Regine Eckardt  
Göttingen, August 2007

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# Survey



# Language change as cultural evolution: Evolutionary approaches to language change

Anette Rosenbach

## 1. Introduction

The term ‘evolution’ is used in various ways in the linguistic literature. We need to distinguish between the evolution of the human language capacity within *homo sapiens* and the subsequent change of language, once the human language capacity has evolved, as sketched in figure 1 below.<sup>1</sup>

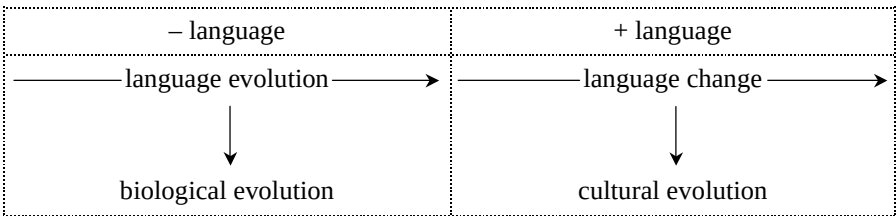


Figure 1. Language evolution as biological and cultural evolution

The two reflect different evolutionary processes: when viewing language as a mental capacity which is biologically grounded (as many linguists do),<sup>2</sup> it follows almost automatically that language must have evolved according to evolutionary processes commonly known for biological systems. Language change, however, is a completely different matter. Given the time span of language change (within historical time) it is highly unlikely that any modifications at the biological, i.e. genetic, level should occur (cf. e.g. also McMahon 2000: 154; Ritt 2004: 26). Rather, language change is commonly viewed as being part of the cultural evolution of language (as opposed to its

<sup>1</sup> In addition, Kirby & Christiansen (2003) also assume evolution on the level of the individual.

<sup>2</sup> Generally, disagreement is not on the question whether the ability for language is innate but on the question of whether this innate language capacity is domain-specific; see e.g. Penke & Rosenbach (2004: 499) for discussion.



biological evolution), with social, communicative, and psychological factors determining language change rather than evolutionary forces known from biological evolution. Both are developmental processes, however, and in recent years it has been claimed that there are general evolutionary processes (such as variation, selection, and self-replication) underlying *any* historically evolved, complex system, subsuming both biological and cultural evolution – although of course operating quite differently in the respective domains (cf. e.g. Dawkins 1986; Dennett 1995), a view which has come to be known as UNIVERSAL DARWINISM.<sup>3</sup> Under this view, language change may be regarded as an instance of cultural evolution and as being subject to the same general evolutionary mechanisms driving biological evolution.

The idea of Universal Darwinism has sparked a number of attempts to account for language change in terms of general evolutionary mechanisms, and the present paper aims to give an overview to the vast literature that has emerged in recent years. I will first briefly introduce the idea of Universal Darwinism (section 2), and then give a broad taxonomy of evolutionary approaches to language change in section 3. The main focus of this article is a discussion of some of the central and controversial questions which arise when studying language change under the perspective of natural selection (section 4). In section 5 I will suggest that the psycholinguistic mechanism of priming may provide a fresh look at some of the old questions discussed in section 4, and then I finally conclude in section 6 with a brief reflection on the value of approaching language change from an evolutionary perspective.

## **2. The analogy between historical and cultural evolution and Universal Darwinism**

The idea of a conceptual transfer of ideas from biological evolution to cultural evolution is not a new one. Historical-comparative linguistics was greatly influenced by Darwin's theory of natural selection in the second part of the 19<sup>th</sup> century. Language at that time was viewed as an organism

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<sup>3</sup> Sometimes, this development in evolutionary theory also goes under the name of NEO-DARWINISM or GENERALIZED DARWINISM. Note that the term 'Neo-Darwinism' refers, strictly speaking, to the integration of Darwin's theory of natural selection with Mendel's theory of genetics and population genetics in the first part of the 20<sup>th</sup> century, but it is also used to refer to UNIVERSAL DARWINISM as a further extension of the Neo-Darwinian approach.

which undergoes evolutionary processes just like biological organisms. Thus, in this (early) conception linguistic evolution was taken as biological evolution, a view which turned out to be highly problematic, and in fact untenable for various reasons (see e.g. the discussions in Sampson 1980: ch. 1, or McMahon 1994: ch. 12), and in modern linguistics, evolution “has become a ‘dirty word’”, as McMahon (1994: 314) put it.<sup>4</sup> In recent years, however, a new strand in evolutionary theory has opened up for new ways of taking an evolutionary look at language. It is particularly the writings by Richard Dawkins (1986), Daniel Dennett (1995), David Hull (1988), Henry Plotkin (1994) and Gary Cziko (1995), which have paved the way for a new and promising transfer of the biological metaphor to the domain of language. The idea of universal Darwinism allows us to view both biological and cultural evolution as being manifestations of the same general (and domain-unspecific) evolutionary mechanisms, and these are the mechanisms of variation, selection, and self-replication.<sup>5</sup> In this conception, biological evolution is taken as one particular instance of evolution (see figure 2), and the conceptual transfer from biological to linguistic evolution is simply meant as a conceptual ‘bridge’ to better understand the way evolution works in language, rather than taking the analogy literally.

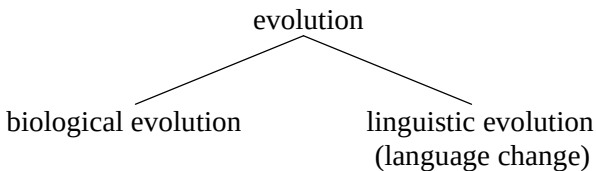


*Figure 2.* Biological and linguistic evolution in Universal Darwinism

<sup>4</sup> One should add, however, that to this day there are approaches which take the biological metaphor in linguistics literally; see e.g. Bichakjian (1989). For a critical discussion of this work see e.g. Lass (1990: 96–97).

<sup>5</sup> Note, however, that the idea of viewing biological and cultural change as instances of the same mechanisms has certainly been around before Dawkins’ and Dennett’s work (e.g. Gerard et al. 1956 or Stevick 1963), just that the idea hadn’t caught on yet, at least not in the linguistic community.

According to Dennett (1995: 343),

evolution occurs whenever the following conditions exist:

- (1) variation: there is continuing abundance of different elements
- (2) heredity or replication: the elements have the capacity to create copies or replicas of themselves
- (3) differential “fitness”: the number of copies of an element that are created in a given time varies, depending on interactions between the features of that element and features of the environment in which it persists.

In biology, the basic Darwinian mechanisms of variation, selection, and self-replication (inheritance) are by now sufficiently well understood. Simply put, VARIATION arises in the form of mutations in the process of gene replication and results in the existence of organisms exhibiting properties that are somehow ‘different’.<sup>6</sup> The process of SELECTION then acts like a kind of ‘filter’ favouring those properties which are in some way advantageous or ‘adaptive’ with respect to the given environmental conditions. Thus, such favourable properties (or rather the genes that encode for them) are selected for and the organisms bearing them will have a higher chance of survival. Finally, the mechanism of REPLICATION (or HEREDITY) allows properties to be passed on, in biology via the process of reproduction on the genetic level (i.e. DNA replication).

The question remains, however, how precisely this general framework should be applied to cultural evolution. In his book *The Selfish Gene* Richard Dawkins (1976) has put forward the intriguing (and provocative) proposal that the so-called MEME is the replicator of ideas in cultural evolution, in analogy to the gene in biological evolution (cf. e.g. also Dennett 1995:chapter 12 or Blackmore 1999). Put simply, the idea is that some ideas, fashions, religions, tunes and whatnot catch on (rather than others) and propagate themselves by jumping from brain to brain, comparable to a virus that replicates itself in organisms. To illustrate the deliberate ‘infectious’ and subconscious character of meme replication, Dennett (1995: 347) mentions how one day he caught himself humming a melody to himself

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<sup>6</sup> On the level of the organism, variation also arises by recombination of the alleles of parents in those of their offspring, as reminded to me by Gerhard Jäger (p.c.). Note, however, that strictly speaking, on the genetic level this only involves the reshuffling of already existing material and not the creation of new variants. In this respect, recombinations only result in phenotypic variation, but not variation on the genetic level.

which he actually greatly disliked, describing it as a “horrible musical virus” (347). In general, memes vary (and compete) with other memes (Dennett, for example, certainly knows more than one melody), some are selected (for whatever reason), and they can replicate (e.g. by humming this tune, Dennett might have ‘infected’ others with it). Thus, on a general level, memes exhibit all processes typical of natural selection, i.e. variation, selection, and self-replication (inheritance). Crucially, the concept of meme captures human’s ability for IMITATION and it has in fact been claimed that anything that is passed on by imitation qualifies as a meme (cf. Blackmore 1999). However, on a more technical level, the concept of meme and meme replication is far from being sufficiently understood, and many open questions remain (see e.g. Blackmore 1999 or Ritt 2004), as e.g.:

- (a) What precisely is the unit of replication, how large can it be? For example, the whole melody, or only certain parts of it, or even single sounds?
- (b) What is the physical or material basis of memes (analogous to the molecular material underlying genes, i.e. DNA or RNA)?
- (c) How precisely does ‘imitation’ work, by what mechanism (analogous to self-replication of genes via sexual reproduction)?

These general questions also transfer to language and to language change. Before I address these questions, however, I will give a broad overview on the various ways in which ideas and concepts from evolutionary theory have entered recent approaches to language change.

### **3. Evolutionary approaches to language change – a brief overview/ taxonomy**

We can distinguish between evolutionary approaches to language change which model long-term linguistic change, resulting in linguistic diversity or the emergence of linguistic universals and those which deal with language change in a more narrow sense; see the overview in table 1 below. Admittedly, the distinctions are arbitrary to some extent. Notice, for example, that the same forces that are responsible for shaping grammars are also likely to operate in language change; this is particularly evident for functional approaches which assume grammar to be constantly shaped by language usage (see e.g. Givón 2002 or Haspelmath 1999), and these can therefore be classified both under ‘emergent properties of grammar’ as well as under

‘language change’.<sup>7</sup> Still, this distinction may be useful as a rough guide through the (vast) literature. What unites these approaches is that they all deal with subsequent modifications of language after the (biological) evolution of the human language capacity, i.e. they all represent language change as an instance of cultural evolution as illustrated in figure 1 above.<sup>8</sup>

Table 1. Evolutionary approaches to language change – an overview

| evolutionary approaches to language change |                                                                                                       |                                                                                                                                                                                                                                      |
|--------------------------------------------|-------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| language change (in a broad sense)         |                                                                                                       | language change (in a narrow sense)                                                                                                                                                                                                  |
| emergence of linguistic diversity          | emergence of linguistic universals/emergent properties of grammar                                     |                                                                                                                                                                                                                                      |
| e.g. Dixon (1997); Nettle (1999a)          | e.g. Lindblom et al. (1984); Kirby (1999); McMahon (2000); Givón (2002); Blevins (2004); Jäger (2007) | e.g. Wildgen (1990); McMahon (1994: ch. 12); Ehala (1996); Keller (1990[1994]); Lass (1990, 1996, 1997, 2003); Haspelmath (1999); Lightfoot (1999); Croft (2000); Clark (2004); Ritt (1995, 1996, 2004); Seiler (2006); Wedel (2006) |

In terms of time-depth, evolutionary approaches to linguistic diversity (e.g. Dixon 1997; Nettle 1999a) presumably span the greatest space of time. This strand of work deals with large scale changes that have led to the phylogenetic diversification of languages into language families. Nettle (1999a) uses an evolutionary model resting on the assumption of (random) variation, selectional pressures and differential replication. In contrast to previous approaches modelling linguistic diversity, Nettle proceeds from linguistic items rather than languages to determine the relatedness between

<sup>7</sup> Haspelmath (1999), for example, primarily wants to account for the adaptive nature of some of the Optimality-theoretic constraints proposed in the literature, but at the same time also makes claims about the nature of language change. As his work is mainly discussed with respect to the latter respect, I have included him under ‘language change (in the narrow sense)’.

<sup>8</sup> Given the vast literature on the topic, this overview only gives a selection (basically, the work I’m aware of) and thus necessarily must remain incomplete. Note also that another line of research not further highlighted here is evolutionary modelling of creole genesis, see e.g. Mufwene (1999, 2001).

languages, as the item approach allows for the consideration of mixed properties of languages (e.g. the fact that English, though traditionally classified as Germanic, also exhibits French and Latin influence), and he uses a computational model to simulate the evolution of linguistic diversity. Like Nettle, Dixon's (1997) account is largely motivated by some dissatisfaction with the traditional family tree model in historical linguistics. Dixon's approach, while not drawing on mechanisms of natural selection, as Nettle does, may be regarded as 'evolutionary' in the sense that he adopts the punctuated equilibrium model known from biology (Eldredge & Gould 1972) to account for the development of languages.<sup>9</sup> Basically, the idea is that there are usually long periods of non-change (the equilibrium); change occurs very suddenly and lasts only for a brief period but then with great effects.<sup>10</sup> Under this view, expansion and split (which is what the family tree model of linguistic diversification tries to capture) only occur in these short periods of punctuation.

Likewise, evolutionary approaches modelling the emergence of linguistic universals or emergent properties of grammar operate on a more long-term basis. They attempt to account for the present-day typological patterns or universal properties of grammar in terms of evolutionary change. For example, Kirby (1999) introduces a computational model (the *Iterated Learning Model*) which simulates how parsing pressures in performance led diachronically to the universal word order preferences (in the form of consistent branching direction) as observed by Hawkins (1994) in a process of functional adaptation.<sup>11</sup> In this model parsing pressures (defined in terms of Hawkins' 1994 principle of *Early Immediate Constituents*) act like a kind of filter to the language acquiring child which may eventually become entrenched in grammar, leading to the observed typological word order distributions. In a similar vein, Jäger (2007) gives an evolutionary account of the

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<sup>9</sup> Note that the punctuated equilibrium model is however perfectly compatible with Darwinian evolution in terms of natural selection, although it is sometimes invoked to support saltationist arguments. For a most forceful refutation of the view that punctuated equilibrium supports saltationist arguments see e.g. Dawkins (1986).

<sup>10</sup> The concept of punctuated equilibrium is also invoked by other historical linguistics to account for the time course of language change, e.g. Lass (1997) and Lightfoot (1999), though this will not be discussed further here.

<sup>11</sup> In subsequent work Kirby and associates also account for the emergence of other linguistic universals, as e.g. the emergence of compositionality (e.g. Kirby & Mortensen 2003 or Kirby, Brighton & Smith 2004 and literature given therein).

emergence of typological case-marking systems. In particular, he uses the mathematical model of *Evolutionary Game Theory* (Maynard Smith 1982) to account for the fact that the languages of the world only exhibit certain case marking systems rather than others (namely accusative systems with differential object marking, and ergative systems with differential subject marking, as well as certain combinations of the two), while other – also logically possible – systems are virtually unattested. The participants in the ‘game’ are speech participants, i.e. speakers and hearers, and their individual payoffs are defined functionally in terms of speaker economy (with forms being as short as possible) and hearer economy (with meanings being as explicitly expressed as possible). Jäger demonstrates that the observed case-marking patterns fall out from an application of the functional forces of speaker and hearer economy in an evolutionary game. Both Kirby and Jäger do not model language change *per se*, but rather use evolutionary thinking in a kind of ‘reverse engineering’ to show under which conditions the typological patterns we observe today may have emerged diachronically. In doing so, their focus is on the selection process that results in the differential replication of variants towards evolutionary stable states. Whereas Kirby (1999) and Jäger (2007) deal with (morpho-) syntax, Blevins (2004) is an evolutionary approach from the realm of phonology, accounting for the recurrent sound patterns of the world in terms of phonetically motivated sound change (see also Lindblom et al. 1984 for accounting for universal phonological pattern). Note that approaches which study the emergence of linguistic universals from an evolutionary perspective bear significantly on the question of how much linguistic knowledge we may reasonably assume to be innate (or encoded in synchronic grammars) and how much may have been acquired/learned after the evolution of our language capacity (or UG, if one wishes). Anything that can be shown to have emerged does not need to be postulated as being already there (i.e. being part of the innate endowment, UG).<sup>12</sup> Approaches which model the emergence of universals or emergent properties of grammar are typically not

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<sup>12</sup> A challenge for such approaches is, however, what Kirby (1999) has called ‘the problem of linkage’, namely the question of how preferences in language usage may become grammaticalized (i.e. part of grammar) over time; see also Haspelmath (1999: 200, 259). For discussion see e.g. Rosenbach & Jäger (forthcoming: §6) and below (§§4.2–4.3). Note also that arguments from evolutionary biology can be used to show that certain allegedly innate properties of grammar are implausible from an evolutionary point of view, as e.g. done by McMahon (2000), who challenges the Optimality-theoretic approach using evolutionary arguments.

considered as approaches to language change in the strict sense (and, in fact, the authors usually don't consider their work so, see e.g. Jäger (2007) in his introductory remarks). Yet, these approaches deal with subsequent modifications of language after the (biological) evolution of the language capacity (see figure 1 above) and are thus addressing diachronic processes as approaches to language change do, if operating within different time-scales and within different domains. That is, all these approaches face, for instance, the fundamental question(s) of what kind of linguistic replicator and selectional processes to assume (see further section 4 below). That these approaches are indeed compatible with accounts of language change (in the narrow sense) is demonstrated, for instance, by Clark (2004) who uses Kirby's (1999) *Iterated Learning Model* of filtered learning to account for the change of OV to VO word order in the history of English.<sup>13</sup>

#### 4. Universal Darwinism from the historical linguist's point of view

In contrast to the evolutionary approaches sketched above, the approaches discussed in this section put the main emphasis on the phenomenon of language change itself (within historical time) rather than on the question of how linguistic universals, synchronic grammars or linguistic diversity have emerged via diachronic change (although, naturally, these two perspectives are connected and need not exclude each other, as argued above). We may identify here various ways in which a transfer from evolutionary theory to historical linguistics is made. First, some scholars make reference to chaos theory, as e.g. developed by Stewart (1990) or Kauffman (1995), or adopt ideas from research on the self-organisation of systems, i.e. both frameworks developed for the evolution of general systems. For such approaches to language change see e.g. Wildgen (1990), Ehala (1996), Lass (1997: ch. 6), Lightfoot (1999), and there are also chaos-theoretic elements in Ritt's (1996, 2004) approach, though all these works differ otherwise greatly in detail. To give just one example to illustrate this line of reasoning, Lass (1997: 293–304, 2000) uses the idea of domain-neutral dynamical systems to account for the often observed directionality and cyclicity of linguistic change (in particular as often observed with grammaticalization processes).

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<sup>13</sup> In contrast to Kirby (1999), Clark proceeds from a stochastic Optimality-theoretic grammar, i.e. a grammar which allows for the encoding of probabilistic preferences, rather than assuming categorical properties only (as in the grammar underlying Kirby's model).



Lass argues that unidirectional change is not necessarily a property of language (and hence language change, or grammaticalization) but more likely falls out from the general ('chaotic') behaviour – in the sense of e.g. Stewart (1990) – typical of “an evolutionary landscape where flow-paths converge on attractors” (Lass 2000: 222). That is, he views unidirectional developments as “paths along a chreod [i.e. the trajectory to which the system tends to return, AR] leading to a point-attractor: each stage gets ‘closer’ to the sink, ... . And once this stage is reached, there is generally no way emerging from it” (Lass 1997: 296). This reduces unidirectional changes to forms of autocatalytic behaviour or positive feedback which is not specific to language but typical of many general systems. In contrast to such chaos-theoretically inspired approaches, I will however in the following more closely focus on evolutionary approaches to language change which view natural selection as the major process driving language change.<sup>14</sup>

Viewing language change (or at least some types of change)<sup>15</sup> as a process of natural selection presupposes that the processes of variation, selection and self-replication (inheritance) are also present in language, but are they? Now, variation certainly abounds in language, and the relation between variation and change has been recognized in (modern) linguistics at least since the pioneering article by Weinreich, Labov & Herzog (1968) in their famous statement that “[n]ot all variability and heterogeneity in language structure involves change; but all change involves variability and heterogeneity” (188). In this article the authors make the important conceptual distinction between the actuation of change and its subsequent transmission (spread/propagation). That is, they distinguish the emergence of new variants in a language from their spread through a speech community. These two processes essentially mirror the evolutionary processes of variation and selection: new variants come into use, and selectional pressures lead to their differential replication (though see §4.1 for a more refined view on ‘actuation’). It is also clear that language is passed on, as not every new generation ‘invents’ language from scratch, though there is still a lot of disagreement among linguists whether language transmission takes place in first

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<sup>14</sup> Note that strictly speaking chaos-theoretic approaches may be subsumed within a larger Neo-Darwinian framework; see e.g. Dennett (1995: ch. 7) for defending such a view against a common position according to which chaos theory is assumed to contradict natural selection (e.g. Longa 2001).

<sup>15</sup> See e.g. Haspelmath (1999) for suggesting that not every type of linguistic change may be adaptive, and he particularly points out grammaticalization as one possible counter-adaptive type of change.

language acquisition only, as is the classic generative position (see e.g. Lightfoot 1999), or exclusively in language usage and thus adults, as maintained by functionalist approaches (see e.g. Croft 2000).

As language is a complex system which exhibits the evolutionary processes of variation, selection, and self-replication/inheritance, we can thus conclude that it must have evolved – and is still evolving – according to the process of natural selection, and accordingly, language change may be viewed as an evolutionary process that is subject to the same (general) forces driving biological evolution. This view has in recent years been put forward by various historical and/or functional linguists, witness e.g. Lass (1990, 1996, 1997, 2003); Keller (1990[1994]), McMahon (1994), Haspelmath (1999), Croft (2000, 2005, 2006), Ritt (1995, 1996, 2004) and Seiler (2006). In this context, Wedel (2006) is another work to be mentioned. Wedel's work combines in various ways aspects of the research focusing on emergent properties of grammar (as the work by Kirby 1999; Jäger 2007; or Blevins 2004 mentioned in section 3 above) and more directly general language change phenomena (such as providing a non-teleological account for the emergence and the avoidance of mergers in sound change). Note further that of these works Croft (2000) and Ritt (2004) are monographs exclusively devoted to developing an evolutionary model of language change and thus represent more comprehensive models which try to specify in more detail the general evolutionary processes underlying language change. Note from the outset that given the space and scope of the present paper, I cannot introduce every single approach in all its details. Rather, I will review the linguistic literature according to how it deals with essential questions concerning the evolutionary processes of variation (and how it comes about), selection and replication.

#### 4.1. Linguistic variation (and how it comes about)

We may take the existence of linguistic variation as a given. Less clearly understood, however, are the mechanisms by which variation comes about, i.e. linguistic innovation. According to natural selection, novel linguistic variants ought to arise as a result of altered replication (see e.g. Lass 1997: 315; Croft 2000: 3), but the question is how precisely? This is one of the 'hard' questions of historical linguistics, and research so far has primarily addressed the spread/propagation of change rather than its innovation. A notable exception is the Milroys' work (Milroy & Milroy 1985; Milroy 1992) in that they explicitly address the process of ACTUATION, which they

account for in terms of a social-network analysis. Yet, their approach does not address the question of how precisely new variants arise, but only the question of how, once there, new variants are taken up by other speakers (or not), hence turning innovations into changes (or not). That is, strictly speaking, the innovation and actuation of change, although often conflated in the literature, are two separate processes, with the innovation of change being logically (and temporally) prior to the ‘uptake’ of this innovation by other speakers.<sup>16</sup> New variants may emerge for various reasons, but they only turn into a ‘change’ (in the true sense of the word) once they come to bear on the linguistic system, i.e. once the innovation catches on and is adopted by others, or as Milroy (1992: 79) put it, “a change is not a change until it has been adopted by *more than one speaker*.” We thus have to slightly modify our statement above that Weinreich et al.’s (1968) terms ‘actuation’ vs. ‘transmission’ are equivalent to the processes of the emergence of variation and selection in evolutionary theory in the following way: the term ‘actuation’ is already part of the selection process, or, as Householder (1972) put it, it’s the ‘minimal step’ an innovation needs to become a change. Croft (2000) makes the distinction between the innovation and the spread of change when distinguishing between “INNOVATION or actuation – the creation of novel forms in the language – and PROPAGATION or diffusion (or, conversely, loss) of those forms in the language” (4–5), though notice the (usual) conflation of the terms ‘innovation’ and ‘actuation’ here.<sup>17</sup> Now, the question remains of how innovation occurs? Note that we can only recognize an innovation when it is already spreading. That is, it’s only the successful innovations that we register, while one-off innovations go unnoticed. And even in the case of ‘surviving’ innovations it may take some momentum for such changes to become visible to the (historical) linguist (see e.g. the discussions in Lass 1997: §6.2 or Hopper & Traugott 1993 [2003]: 48). Yet, there are some attempts to identify general

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<sup>16</sup> The ‘actuation problem’ as originally formulated by Weinreich et al. (1968: 102) is: “Why do changes in a structural feature take place in a particular language at a given time, but not in other languages with the same feature, or in the same language at other times?”

<sup>17</sup> Terminology is actually very confusing in this case. For example, for what we call here ‘actuation’ (in the way defined by Weinreich et al. 1968, see note 16 above), Croft (2000: 120) uses the term ‘actualization’. Milroy (1992: 77) speaks about ‘speaker innovation’ and ‘change in the linguistic system’. For useful discussion of ‘innovation’ vs. ‘spread’ see e.g. Hopper & Traugott (1993 [2003]: 46–50), or McMahon (1994: 248–252).

sources of innovation in the literature. Lass (1997: §6.4) mentions three ways by which new linguistic variants can arise: (a) by borrowing from other languages, (b) by genuine invention/creation out of nothing and, most commonly, (c) by transformation of already existing material. Note that only in the latter case are we, strictly speaking, dealing with ‘altered replication’ in the evolutionary sense, as it is the only way of creating new things by modification of older material.<sup>18</sup> Lass (1997) stresses one particular case here, namely the re-use of old material, a process which he coins in analogy to the corresponding concept from biology as *EXAPTATION* (cf. Gould & Vrba 1982). He defines it as “a kind of conceptual renovation, as it were, of material that is already there, but either serving some other purpose, or serving no purpose at all” (Lass 1997: 316). Such old material can be either ‘junk’ (see also Lass 1990), or perfectly functional material which is simply used for another purpose, a process often leading to grammaticalization (Lass 1997: 318).

Another common way of transforming existing linguistic material is *ANALOGICAL EXTENSION*, which is the generalisation of a form/construction to a new context or to new forms/constructions. A special case of analogical extension is metaphorical change, which is the analogical extension of meaning, or, more precisely, “a type of paradigmatic change, whereby a word-sign used for a particular object or concept comes to be used for another concept because of some element that these two concepts have in common” (Fischer & Rosenbach 2000: 15). Analogy may refer to the regularization of irregular forms, as e.g. in the extension of the -s plural in English, which analogically generalises from the Old English plural masculine form of strong nouns -as to (almost) all other nouns in the course of Middle English. Another example is the creation of the 3<sup>rd</sup> person neuter possessive pronoun *its* in English, which has (presumably) been coined by analogy to other genitive forms (*its* = *it* + (gen)-s), cf. e.g. Lass (2006: 96). It may, however, also more generally refer to any psychological process of similarity-based inference. Gentner & Markham (1997), for example, argue that “similarity is like analogy” (45), showing that both are carried out by the same process of structural alignment. Analogy is usually distinguished from

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<sup>18</sup> Notice, however, Heine & Kuteva (2005) for making a case that in grammatical borrowing rarely ever is completely new material borrowed, but that more often forms/structures already present in the replica language (‘minor use patterns’) are being extended. Such extensions would therefore also qualify as ‘altered replication’, see also section 5 below.

REANALYSIS (see e.g. Hopper & Traugott 1993 [2003]: chapter 3). In contrast to analogy, which is a paradigmatic process where *the same* form/construction is only extended to new contexts/forms, reanalysis operates on the syntagmatic plane, resulting in the creation of *a new* form/construction or rule (e.g. Hopper & Traugott 1993 [2003]: 64; or Fischer & Rosenbach 2000: 14–17). Witness, for example, the case of the English modals, which (allegedly) came to be reanalysed from full (main) verbs to auxiliary verbs in the history of English.

According to Croft (2000: ch. 5), reanalysis, and more specifically form-function reanalysis in language use, is the major source of innovation. As every communicative situation is a unique and novel event, Croft argues, “the recombination in utterances of pre-existing grammatical units and structures will involve some degree of novelty in the form-to-function mapping in each use of language” (p. 118).<sup>19</sup> That is, in the mapping from form to function (> perception) and from function to form (> production) there may be minimal deviations in the way that speakers and hearers produce/interpret linguistic form and meaning. In particular, Croft distinguishes various ways speakers or hearers may recombine linguistic elements, i.e.

1. HYPERANALYSIS: a mechanism for semantic bleaching,
2. HYPOANALYSIS: which is equivalent to Lass’ notion of exaptation,
3. METANALYSIS: a mechanism underlying (invited) pragmatic inference (Hopper & Traugott 1993[2003]), and
4. CRYPTANALYSIS: a mechanism for pleonasm and reinforcement, and sometimes for extension.

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<sup>19</sup> In a recent paper Croft (2005) even more forcefully stresses the fact of novelty in utterances, arguing that under an onomasiological perspective most morpho-syntactic verbalizations are indeed ‘new’. Under this view, there would be barely any difference between ‘normal replication’ and ‘altered replication’, as replications almost always involve alterations, at least at the morphosyntactic level. Similarly, Jäger (2007: §5) has pointed out that linguistic creativity is a source for innovation, in the sense that “[e]very complex linguistic structure that can be grammatically constructed out of several linguemes can be hardened into a new lingueme, as soon as somebody stores it cognitively as an integral part.” Note the analogy to biology, where “*unpredictable novelty emerges* from the blind recombination and mutation of DNA sequences in the genes regardless of the fact that it is always the same old four genetic building blocks that are reshuffled”, as noted by Cziko (1995: 300; emphasis in original).

All these cases imply transformations of things already there (in the form of mismatches in speaker-hearer interactions). This poses the question of what precisely counts as a ‘new’ or ‘altered’ variant? Does a slightly changed interpretation of a given linguistic form/construction suffice? And what kind of meanings would count here? Only changes in the referential (semantic) meaning, or also pragmatic or social meaning? That is, to what extent is a replicated variant still ‘the same’ as its source, and how big a step does it take for variants to be ‘different’? And, how can we possibly operationalize this? All this relates to the question of how we can reasonably distinguish between analogy and reanalysis. Recall that analogy refers to the generalisation of the same form, while it is only reanalysis that involves the formation of some new category, form or rule. Thus, strictly speaking, it is only reanalysis that would qualify as true innovation. In practice, however, it is extremely difficult to tease apart analogy and reanalysis (see also the discussion in Hopper & Traugott 1993 [2003]: 68–69). Alternatively, every single analogical extension (however small) may be viewed as representing a type of reanalysis. Hopper & Traugott (1993[2003]) therefore pose the challenging question “whether everything is not reanalysis” (69),<sup>20</sup> while nonetheless keeping the distinction between analogy and reanalysis as a useful heuristic one. Such a view seems to be also implicit in Croft’s use of the term ‘reanalysis’. Yet, even when conceiving of (some) reanalyses as resulting from many small steps of analogy, the question remains how a speaker makes the small though dramatic step of categorizing, say, a modal verb as a main verb or as an auxiliary verb?<sup>21</sup> Notice that the problem of delineating analogy from reanalysis is related to the problem of how to align the token level in language usage to the type level in linguistic competence. Via analogical extension, tokens may take different forms/meanings, but at what point do these changes result in the creation of a new type? That is, how can minimal alterations we make in language usage come to bear on our mental representations? One solution is to apply an exemplar model, in which categories are defined via ‘clouds’ of memorized (and analogically related) tokens, and minimal shifts in usage (such as analogies) may be immediately registered in the category label; see also section 4.3 below.

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<sup>20</sup> The view that there is no analogy without reanalysis has recently been re-emphasized by Traugott (2006), arguing that every analogy is a small reanalysis.

<sup>21</sup> After all, such a categorization has further morphosyntactic consequences.

To sum up so far: three general sources for altered replication have been identified (with (ii) and (iii) possibly being only two sides of the same coin):

- (i) exaptation ('conceptual renovation' in Lass' 1997: 316 terminology),
- (ii) analogy/analogical extensions, and
- (iii) reanalysis.<sup>22</sup>

A further question is whether innovation is random or not. Under one view variation in language arises randomly, like variation in biology (though of course not by mutation) and it is only the process of selection (see also §4.2) which brings in 'order' into language change (cf. e.g. McMahon 1994: 337). Or does variation arise non-randomly, at least often so, as e.g. argued by Haspelmath (1999: 192)? The question of randomness in linguistic innovation relates to the question of whether innovation in language is amenable to functional motivation. According to Croft (2000) it is, although he also allows for random innovation to occur (cf. Croft 2000: 118–119); see further section 4.3 on where to place the locus of functional factors in evolutionary change. However, non-randomness and functional motivation do not need to go together, as is e.g. apparent from Blevins' (2004) approach to sound change. Blevins (2004) argues that errors in linguistic replication are in the same way random and non-optimizing as are errors in DNA replications (26). However, at the same time she gives various sources and mechanisms for how new variants may arise in speech, distinguishing three processes, which she dubs the CCC-model (see Blevins 2004: 32–33), namely

1. CHANGE: A signal A (uttered by a speaker) may be misheard (by the listener) as a signal B due to the perceptual similarities between them.
2. CHANCE: This refers to sound changes having ambiguous sound segmentation as their source. In this case the listener associates an intrinsically phonologically ambiguous signal differently than intended by the speaker.

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<sup>22</sup> Yet another mechanism for the rise of new variants often invoked in historical linguistics is ABDUCTION (see e.g. Hopper & Traugott 1993[2003]: 41–43). See, however, Deutscher (2002) for arguing that the notion of abduction is redundant in historical linguistics and more adequately subsumed under the notion of reanalysis.

3. CHOICE: This relates to intraspeaker variation only in the sense that the phonetic speech continuum is intrinsically variable and speakers therefore have the choice between various representatives of ‘the same’ sound.

All these sources of new variants in sound change are (in some way or other) phonetically motivated and hence would presumably qualify as cases of functionally motivated innovations in the sense of Croft (2000),<sup>23</sup> but Blevins still considers them as being ‘random’.<sup>24</sup> Blevins introduces thus the subtle distinction between ‘randomness’ on the one hand and (phonetic) ‘motivation’ in innovation on the other, as also witnessed in her final conclusions:

An intrinsic property of human speech is phonetic variability: all phonological categories have variable realizations along specific phonetic parameters. This phonetic variability often serves as the source of sound change, and can be likened to “random” genetic mutations. Though phonetic variability itself may be viewed in terms of minimizing effort (natural speech) and maximizing contrast (careful speech), the sound change which occurs during language transmission when one of these variants is adopted at the expense of the other appears to be based on frequency in language use. Under this view, effort minimization and contrast maintenance do not play a direct role in sound change. From the perspective of the language learner, then, the variability inherent in the grammar can be considered “random”, with change modeled on probabilistic terms. In other grammatical domains, distinguishing the source of variation from forces which give rise to change under variation may allow for more illuminating treatments of recurrent grammatical change. (Blevins 2004: 314)

Notice at this point that the term ‘randomness’ may be interpreted in various ways in biology.

On the one hand, even in biology mutations are not completely random; certain DNA replications are just more likely to occur than others. Mutations are, however, random in the sense that their probability is causally

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<sup>23</sup> As noted by Seiler (2006: 167) it is not clear, however, how Croft’s (2000) scenario of form-function reanalysis accounting for (functionally motivated) innovation should transfer to phonological change; in general, Croft has only very little to say about phonological change throughout his book.

<sup>24</sup> It seems that Blevins uses the term ‘random’ as implying a not goal-directed, i.e. not optimising, process, hence avoiding any teleological flavour.



unconnected to the effect they have on subsequent fitness.<sup>25</sup> That is, new variants are generated ‘blindly’ as they do not have the increased fitness of the organism ‘in mind’, although their emergence is not completely unconstrained (cf. also Czikó 1995: 288ff.). It is in this way that Blevins (2004) may allow for the notion of ‘random’ innovations that are at the same time phonetically motivated.

In the following section I will get briefly back to innovation, zooming in more closely on its logical relation to the selection process.

## 4.2. Linguistic selection

Once there is variation, there are essentially two diachronic pathways that variants may undergo (cf. e.g. Kroch 1994; or Niepokuj 2006):

- (1) One variant eventually ousts the other and becomes the sole expression. In this case variation is simply an interim state leading to the elimination of one variant and the grammaticalization of the other.
- (2) The variants become specialized (functionally and/or socially) – one variant gets systematically used in context A and/or by speaker group X, while the other variant is systematically used in context B and/or by speaker group Y. There may be diachronic shifts in the way the variants are distributed, but essentially in this scenario both variants (and thus variation) persist over time.

Selection operates whenever we find *differential replication* of variants, i.e. whenever there is a shift in the distribution of the two variants. That is, once variants have settled down in their respective functional and/or social niches and have thus reached a stable distribution or equilibrium, selection no longer comes into play. However, selection is present in *the process leading to* functional/social differentiation or whenever variants come to be associated with new values. Under this view, selection does not necessarily reduce variation (as is the commonly held view) but may also be present in *any* process that changes the distribution of variants.

From an evolutionary point of view, selection does not proceed randomly, but those variants are selected which are in some way advantageous

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<sup>25</sup> I’m grateful to Andy Wedel for making me aware of the different notions of ‘randomness’ in biology.

as compared to other (competing) variants. Over time the selection process may result in ADAPTATIONS (or adaptive patterns), i.e. language patterns which are in some way ‘useful’.<sup>26</sup> Notice that both scenarios for the development of variation sketched above may result in adaptation, i.e. both the entrenchment of one variant and the differentiation/specialization of variants, although typically the literature on linguistic selection and diachronic adaptation focuses on the ‘one wins’ case (e.g. Haspelmath 1999; Kirby 1999; Jäger 2007). As, for example, argued by Bock (1982), the availability of syntactic options is user-optimal in the sense that it allows language processor to accommodate information in the order as it becomes available. Likewise, sociolinguistic research has been stressing the fact of stable variation and been identifying the factors leading to and maintaining it (see e.g. Labov 2001, and the discussion in Niepokuj 2006: 1–2). In this sense, then, variation does not only need to be a transitional phase leading from one variant to another one but it may itself be functional and thus ‘adaptive’, too.

The literature is, however, divided on the nature of the linguistic selection process. That is, what is it precisely that, given the choice between two (or more) linguistic variants, favours the selection of one variant over the other? Within an evolutionary approach, the most parsimonious solution is to acknowledge the presence of selectional forces but otherwise remain agnostic about their nature. This is the stance taken by Lass (1990, 1996, 1997: ch. 7), who explicitly rejects any speaker-based forces (either in terms of functional or social pressures), as e.g. expressed in the following passage:

We don’t have to allow our sense of ourselves as persons to get in the way of the outsider’s view of our transactions with language, the world and ourselves as being essentially selectional processes without anybody as it were in the driver’s seat who is ontologically separate from and hierarchically prior to the car and all the processes that move it. (Lass 1996: 5)

Ritt (2004) basically agrees with Lass that it is sufficient for selection to occur but at the same time he goes beyond Lass’ agnostic position in stating that getting to know the selectional pressures (whatever they are) is a

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<sup>26</sup> Usually, the term ‘adaptation’ is evoked in the linguistic literature to refer to functionally adaptive patterns (e.g. Haspelmath 1999), in analogy to biological adaptation, but I’m using it here in a more general sense as to also include ‘social usefulness’.

desirable endeavour which ultimately will bring us closer to an understanding of the nature of change.

It is enough to acknowledge the plain fact that selection seems to have occurred.... But there are reasons for digging deeper. It is evident, first of all, that the specific kind of selection process to which a population of replicators is submitted determines what exactly gets selected. It therefore has an obvious explanatory function, and the more we know about it, the more we understand about the history of a replicator population. (Ritt 2004: 188)

Croft (2000) is much more explicit about the nature of the selection process. He argues that it is social factors – and *only* social factors – that drive the selection process. He refers to the main determinants of linguistic choices known from the sociolinguistic literature, such as accommodation (adaptation of one's speech to that of an interlocutor) and prestige (overt and covert).<sup>27</sup> Croft accounts for 'functional fit' in language (i.e. the fit of form to function) by assuming that the emergence of linguistic novelty (innovation) is functionally motivated, so the outcome of the selection process must necessarily be functionally adaptive, too, as it only operates on user-optimal structures in the first place. In contrast, most scholars assume (or have modelled) functional factors as driving the selection process, see e.g. Haspelmath (1999), Kirby (1999), Givón (2002) or Jäger (2007). These authors focus on the common observation that "grammars code best what speakers do most" (Du Bois 1985: 363) and they attribute such 'functional fit' to selection rather than innovation. The question, then, is how to decide empirically between these two positions. What kind of evidence can clearly identify functional pressures in the selection process (rather than in innovation)? The very fact that we find functional factors at work in linguistic variation is not sufficient, as this may simply reflect those functional forces that brought them about. Croft (2000:184–185) refers to findings by Kroch (1989) which indicate that functional factors affect grammatical variation always to the same extent, at least relatively so. No matter whether the frequencies of variants rise or fall, the (relative) "strength of these effects remains constant as the change proceeds" (Kroch 1989: 238). We therefore

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<sup>27</sup> Note that Croft (2000) also mentions "reinforcement and decay of entrenchment" as a "non-intentional mechanisms of selection" (74). By this he means frequency of exposure, which certainly is *not* a social factor, though this factor is not further highlighted in his book. See also section 5 where I will get back to the idea that reinforcement of entrenchment may constitute selectional pressure.

need to look for cases where variants start out being distributed arbitrarily and only eventually become subject to functional forces.

An interesting case in point is Seiler's (2006) study on the variation of dative marking patterns in Upper German dialects. These dialects have (apparently independently) developed prepositional dative marking; for illustration see example (1) from Bavarian:

- (1) *du muasst es a deiná frau vaschraibn lássn*  
 you must:2S it DM yourDSF wife transfer let:INF  
 'you have to transfer it [=the money] to your wife'  
 (Malching; Ströbl 1970: 66; as cited in Seiler 2006: 170)

For the majority of dialect speakers, the dative marker is optional, resulting in variation between the bare dative and the prepositional dative marker. According to Seiler various dialects exhibit different distributional patterns, which all show functional principles at work. While in Bavarian prepositional dative marking is used as a compensatory strategy for missing case distinctions, in Northern Switzerland (Alemannic) the choice between the two dative marking patterns is mainly determined by information structure, whereas in Central Switzerland (Alemannic) it is phonological factors (avoidance of stress clashes) that determine which pattern is used. The very fact that different dialects took different paths to functionally differentiate the variants *after the prepositional dative marker has come into being* clearly shows that these functional factors cannot be held to be responsible for the innovation of variants but must be attributed to the subsequent selection process.

Another case in point is Mondorf's (2004: section 10) in-depth study on variation and change in English comparative alternation, i.e. the variation between a synthetic and an analytic comparative as in *worthier* vs. *more worthy*. Until the 18<sup>th</sup> century the distribution between the two variants (which have co-existed at least since the Old English period) is still fairly arbitrary (cf. also Lass 2006: 95–96), and a regularization in the sense of a (functional) division of labour only gradually begins to take place in the course of the 18<sup>th</sup> century. In particular, Mondorf shows that "the analytic variant has come to be used in environments that are for some reason cognitively complex, while the *-er* variant prevails in easy to process environments that do not require *more*-support" (§10.7). That is, the two variants eventually settle into a distribution that is (among other things) motivated by cognitive factors and processing ease. Apparently, these factors have *not*

remained the same over the time, as Mondorf convincingly shows, and thus they cannot be attributed to innovation but must be taken to represent (functional) selection pressure.

Undoubtedly, social factors play an important role in the promotion or demotion of variants, too, as diachronic sociolinguistic research has shown (e.g. Labov 1994), but the evidence available does not speak for the *exclusive* role of social factors in the selection process, as far as I can discern. A more realistic stance therefore seems to be to allow for the existence of social *and* functional factors in the selection process, as acknowledged by many other scholars, e.g. Keller (1990 [1994]: 151), Haspelmath (1999), Nettle (1999a) or Jäger (2007).

Notice, however, that in practice it may sometimes be difficult to decide whether functionally adaptive patterns arise due to functionally motivated innovations and then subsequently spread in a process of social selection (as is Croft’s position) or whether speakers simply simultaneously produce the same type of variants, as they are subject to the same functional pressures. Compare the illustrations in figures 3a and 3b below.

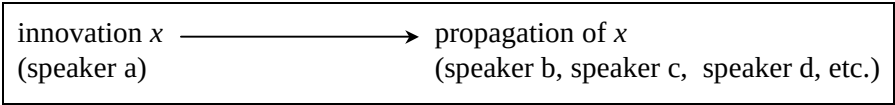


Figure 3a. Innovation – selection according to Croft (2000)

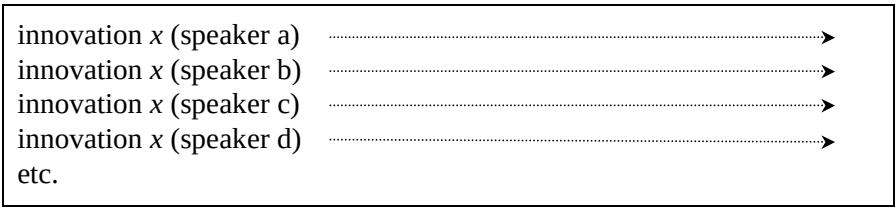


Figure 3b. Spread of a change due to cumulative innovations (‘evolutionary drift’, after Croft 2000: 60)

In the latter scenario (figure 3b) several speakers would *independently* make the same kind of innovation, which, in practice, is hard to distinguish from the adoption of other speakers of one single innovation (though see Seiler 2006, as discussed above, who provides a case study where the two processes can be kept apart empirically). Croft (2000: 59–62) argues that the second scenario in figure 3b represents a typical invisible-hand process

in the sense of Keller (1990 [1994]), according to which change may result from the cumulative effect of many individual speaker actions which were, individually, not intended to change language in a particular way but which still conspire to do so, as they are all independently driven by the same kind of factor(s). According to Croft (2000), such invisible-hand processes are not selection processes in the strict sense but rather represent a kind of “evolutionary drift” (60), which he regards as a valid though “minor propagation mechanism” (62). Notice, however, that the invisible-hand approach is not exclusively an account of linguistic innovation but also – and presumably even more so – an account of selection in the sense that speakers may independently make the same kind of choice among available variants. See e.g. Keller (2006), who says that “[p]eople act more or less in coordination when they make similar choices based on similar behavioral maxims”.

In general, the whole debate depends on a clear-cut distinction between innovation on the one hand, and selection on the other, but is not clear in how far the two processes (i.e. the emergence of new variants and their subsequent propagation) can be neatly kept apart (cf. also Keller 2006). To give an example: Jäger (2007) regards speaker-economy (‘be short’) and hearer economy (‘be clear’) as functional factors in the selection process. Croft (2000: 75) (though somewhat reluctantly) considers user economy to be relevant, too, but he views it as a mechanism for altered replication (i.e. innovation). One possible way of distinguishing between processes of evolutionary drift and selection is by mathematical models. Baxter et al. (2006) develop a mathematical model of Croft’s (2000) evolutionary theory of language change. As the authors say, such a model makes precise predictions about extinction probabilities and timescales, which can then be compared to empirical data of language use (p. 19), helping to decide between the two accounts (drift vs. selection). This work is still in its infancy, though (Croft, p.c.), and further research will have to show how we can empirically decide between the two scenarios, and whether there may even be ways to reconcile them, as it seems plausible that both are interacting and do not necessarily have to mutually exclude each other.

Note, also that clearly distinguishing between functional and social selection presupposes that we can indeed precisely specify what is ‘functional’ as opposed to what is ‘social’. But this is anything but clear, thus further complicating the picture considerably. Croft (2000) essentially refers to Labov (1994) when claiming that functional factors apparently do not play any role in selection. Looking more closely at how Labov (1994) defines ‘functional’, however, reveals that he only includes speakers’ com-

municative intentions and factors such as meaning preservation but not any processing factors (the latter are labelled ‘mechanical factors’ by Labov 1994: §19). Croft’s (2000, 2006) definition of ‘functional’ in the sense of the factors relating to the mapping of meaning onto grammatical form (and of phonological form mapped onto phonetic realizations) would leave, in principle, room for processing factors, but they do not appear to play any significant role in his model.<sup>28</sup> Note, however, that in Kirby (1999) it is precisely parsing pressures in real time that are considered as the crucial selectional force that bring about the typological distributions that Kirby seeks to explain and that go under the name of ‘functional selection’ in his approach. And indeed, within large circles of linguistics, processing pressures certainly count as ‘functional’ factors (see e.g. Newmeyer 1998, or research on grammatical variation as e.g. represented by Mondorf 2004 cited above or in the contributions in Rohdenburg & Mondorf 2003). Note, in general, that processing pressures only operate on *given* alternatives (at least for grammatical choices) and as such are best accounted for as factors operating in selection rather than in genuine innovation. To give another example for the arbitrariness of the ‘functional’ vs. ‘social’ distinction: Croft (2000: 75) argues that the ECONOMY PRINCIPLE, which is typically interpreted as a processing factor, can also be interpreted in terms of speaker-hearer interaction, hence attributing it some social value. Likewise, he gives ECONOMIC MOTIVATION (in the sense that frequent forms tend to be short, going back to Zipf) a social (interactional) interpretation in saying that frequency of use is “not frequency of occurrence in the world, but frequency of being talked about” (76).

In general, the question whether we regard certain forces as functional or social seems to depend heavily on the theoretical framework, i.e. on interpretation, rather than reflecting a genuine ontological distinction. In Dik’s (1989) functional approach, for example, the function of language is what it is used for (see also Keller 1997: 11), and Dik certainly includes social interaction here. Under this view, then, if a speaker uses a certain variant because it is more prestigious (and thus s/he wants to signal identity with the group that typically uses this variant), this counts as ‘functional’. Influencing others has also been singled out by Keller (1990 [1994]: 84–90) as *the* prime function of language, and this is, in Keller’s approach, crucially linked to

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<sup>28</sup> An exception is his treatment of entrenchment as a possible selection force, though this is only mentioned in passing and not further explored in Croft (2000).

social success: in communication speakers use language in such a way that they are socially successful (Keller's maxims, which draw on Gricean speech act theory, are all built on this principle), and thus speakers' choices depend on this. So, what is regarded as essentially 'functional' in Keller's approach, receives an exclusively 'social' interpretation in Croft's approach, who mainly draws on sociolinguistics in his account of selection, although in the end the same sort of factors (e.g. prestige) are being talked about. That is, what counts as 'functional' for, say, speech act theorists may count as 'social' for sociolinguists. Likewise, what counts as 'social' for sociolinguistics, may count as psychological (and thus functional) for psycholinguists. Note, for example, that accommodation, which is one of the major selectional forces referred to by Croft, may be easily given a psychological interpretation in terms of priming (on priming see also section 5 below).

To conclude so far, there appears to be evidence for the presence of functional forces in selection, pace Croft (2000). In the end, however, any labelling of the selection process as either 'social' or 'functional' appears to be futile, as in most cases it simply reflects a terminological choice. Our primary goal should be to identify selectional forces in the first place, and, if possible, to specify their nature. How we label such mechanisms then will be mainly a matter of personal preference (or theoretical orientation). Note, finally, that the selection processes discussed in this section all operate in language usage, which leaves the question open of how they should ever 'make it' into grammar. This fundamental problem has been called the 'problem of linkage' by Kirby (1999: 20):

The problem of linkage: Given a set of observed constraints on cross-linguistic variation, and a corresponding pattern of functional preference, an explanation of this fit will solve the problem: how does the latter give rise to the former?

This is still one of the core problems in evolutionary models of language change, as in approaches to variation and change in general. It is related to the question of where to place the locus of replication, to which I will briefly return in section 4.3. below (see also note 12 above).



### 4.3. Linguistic replication

Various questions are involved here:

1. What are linguistic replicators in the first place?
2. What are the units of linguistic replicators?
3. What is the material basis of linguistic replicators (and replication)?
4. What is the replication mechanism?

In the following these issues will be addressed in turn.

#### *What are linguistic replicators?*

Are there language memes? – Looking at the literature, the answer seems to be positive. Croft (2000: 28) uses the term *LINGUEME* (attributed to Martin Haspelmath), which captures the analogy to Dawkins' term 'meme', while Ritt (2004) literally adopts the term 'meme' for linguistic replicators. Another question, however, is how to fill these concepts with content? How are linguemes and language memes defined? What kind of underlying language ontology is assumed? How big a unit can linguemes/memes be? In other words, what is it, precisely, that replicates (see also Lass 1996)?

Croft (2000) makes the important distinction between *TOKENS* and *TYPES* in replication (cf. also Lass 1996:7). The entities that are physically replicating are tokens, but these tokens exhibit structure, i.e. types. Croft's term 'lingueme' comprises both.<sup>29</sup> Analogous to the suggestion made by Hull (1988: 449, as cited in Croft 2000: 28) that "replicators exist in nested systems of increasingly more inclusive units", Croft (2000: 28) assumes that the *UTTERANCE* constitutes this unit, a kind of "embodied structure".<sup>30</sup> Accordingly, Croft's definition of a linguistic replicator is:

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<sup>29</sup> Croft (2000: 40–41, note 4) admits his ambiguous use of the term *lingueme* though, saying that he's using it interchangeably to refer to either tokens or types. He argues that the ambiguity between the token and the type level is also present in the term *gene* from biological evolution.

<sup>30</sup> See also Lass (1996: 9): "Questions like this suggest that either we are looking in the wrong place for reductionist single driving replicators ('linguistic genes'), or misunderstanding the nature of the replication process. In organisms, replication goes along with the specification of structure: genes don't just replicate, but replicate in company (replicators must get along with the co-members of their genomes if the genome as a whole is to produce good phenotypic vehicles that allow each gene to replicate);..."

Thus, the paradigm replicator in language is the lingueme, parallel to the gene as the basic replicator in biology; an utterance is made up of linguemes and linguemes possess structure. (Croft 2000: 28)

Croft very clearly does not advocate any generative notion of ‘competence’ in his approach, but he does – and in fact needs to – assume some kind of abstract mental knowledge of linguistic structure, though at the same time he remains rather agnostic about its nature (p. 41, note 5), and in general downplays its role.<sup>31</sup> It is therefore easy to misread Croft as to suggest that indeed only physical utterance tokens (i.e. actually occurring stretches of language) replicate and are thus the primary linguistic replicator, as is Ritt’s (2004: 158) interpretation of Croft. Ritt defends the view that it is competence properties that replicate, referring to Dawkins’ (1999) suggestion that ‘the instructions’ are copied and not ‘the product’ (see particularly Ritt 2004: §6.5). If only the product, i.e. an actually occurring, physical token, was copied, we could not, for example, account for the fact that structural properties, as e.g. syntactic categories or structure (e.g. NP VP) or even meaning successfully (and in general faithfully) replicate although they are ‘invisible’ at the surface (see particularly Ritt 2004: 197, note 39). In addition, Ritt (2004: §6.5) assumes that linguistic meme replication is digital, following another suggestion made by Dawkins,<sup>32</sup> referring to the general observation that although actual tokens may be continuous and fuzzy, the mind tends to digitalize information. In linguistics this shows, for example, in the fact that phones, although never being physically realized precisely the same (a [t], for example, will almost never be produced precisely in the same way) are yet perceived by us as either, e.g., a /t/ or a /d/, at least in languages such as English where this contrast is phonemic. That is, Ritt is advocating an essentially categorical (and non-gradient) view of linguistic competence here.<sup>33</sup>

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<sup>31</sup> Croft (2000) alludes to cognitive grammar, in particular Langacker’s (1987) notion of ‘entrenchment’ and Bybee’s (1985) interactive activation model to account for the fact that “cognitive structures can ‘survive’ – become entrenched in the mind – or ‘become extinct’ – decay” (32), without, however, exploring this in more detail.

<sup>32</sup> Ritt (2004: 200) cites a talk delivered by Dawkins at the Austrian academy of sciences in May 2000.

<sup>33</sup> Note that a further difference between Ritt’s and Croft’s approach concerns the role of the speaker in linguistic replication. Ritt (2004) adopts Dawkins’ (1976) notion of ‘selfish genes’ in the title of his book *Selfish Sounds and Linguistic*

The token – type distinction is a difficult one to tackle by evolutionary accounts of language change. It boils down to a question that Ritt (2004) programmatically puts in the headline to his section 6.5.2: “How can one copy what one cannot see?” (196).<sup>34</sup> It also touches on the important question of how linguistic replication, which takes place in language usage, may yet show properties that are obviously sensitive to structure and which comes to bear on people’s mental representation of that structure (cf. also the ‘problem of linkage’ mentioned in note 12 and section 4.2. above). Or, as Lass (1996: 7) put it: “The nub is whether meme-domain replicators are all tokens, or whether there are reasonable candidates for types.” Ritt himself tries to solve this problem by assuming that his language memes are not an idealised ‘type’ but rather something which is spatially and temporally bounded and which is manifested in the material world in the form of Hebbian cell assemblies (see also further below). Under this view, linguistic knowledge is material in the sense that it corresponds to structures within a neuronal network, and language usage represents the actual activation of such structures under specific conditions.

Another possible solution to the type – token dilemma is to incorporate exemplar-models into an evolutionary model of language change, as e.g. done by Wedel (2006). Wedel both does justice to the fact that linguistic replicators are linked in larger units as well as to the fact that linguistic (token) replication involves gradient rather than digitalized (i.e. strictly categorical) units:

Further along the continuum away from full discreteness, we can imagine a system that has no real independent replicators, but simply consists of a distribution that replicates each point along the distribution in each generation by interpolating gradiently between all nearby points. In this case, reproductive ‘units’ have no discernable boundaries, nor is any ‘offspring’ point the faithful copy of any ‘parent’ point. (Wedel 2006: 249)

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*Evolution* and thus Dawkins’ idea that memes actively replicate and that the organism’s (i.e. the speaker’s) role is simply that of a ‘vehicle’, i.e. very passive. Croft (2000), in contrast, adopts Hull’s generalized theory of selection and with it Hull’s idea of (somewhat more active) ‘interactors’ rather than Dawkins’ passive notion of ‘vehicle’.

<sup>34</sup> For a general discussion of this problem within memetics, see e.g. Hull (2001: 58–61) and literature cited therein.

Wedel admits that assuming gradient conditions for linguistic replication is not compatible with biology, where genes are discrete replicators. On the other hand, he argues that “the observation that the infrastructure underlying language is very unlike that of genetic systems is not sufficient for concluding that evolutionary theory cannot be applied to the problem of language change” (p. 249–250). Wedel himself gives an evolutionary account of category change (in particular, phonological categories) within an exemplar model of language (e.g. Johnson 1997; Goldinger 2000; Bybee 2001; Pierrehumbert 2001). The basic idea of exemplar-models is that all tokens, or exemplars, in use are mentally registered, and together, as a ‘cloud’, define the category (or type) at hand. Crucially, by encountering new tokens (with slightly different properties) it is possible to accommodate slight shifts in the value of the particular category label, thus allowing usage properties to become immediately registered on the mental type plane within language use. In such a conception it is therefore easy to allow for the view that linguistic knowledge may also change within the lifetime of an (adult) speaker, rather than restricting grammar change to the process of first language acquisition (e.g. Lightfoot 1999). In general, it is interesting to see how the old controversy between formal and functional approaches to language change on the question of where to assume the locus of change (see e.g. the overview given in Croft 2000: 42–62) may become more ‘relaxed’ when taking an evolutionary approach to language (and change), as e.g. done in Ritt’s memetic language change theory (2004), Jäger’s (2007) game-theoretic approach and the evolutionary approach based on exemplar models by Wedel (2006), which all acknowledge change taking place within adult speakers in language usage as well as in the process of first language acquisition (though notice that Kirby’s 1999 evolutionary approach sticks to the (traditional) generative idea that transmission takes place in the grammar-acquiring child only). That is, from an evolutionary point of view language transmission may be both horizontal (among speakers) as well as vertical (among different generations). It is in this sense that evolutionary models may bridge, or at least reconcile, the gap between formal and functional views on language change, though it should also be noted that the old polarities still persist or may even be highlighted, as e.g. in Croft’s (2000) functional and Lightfoot’s (1999) generative model.<sup>35</sup>

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<sup>35</sup> Note that within a generative evolutionary approach to language change such as Lightfoot’s (1999), it is grammars that replicate via first language acquisition and not linguemes or language memes via language usage.

*What are the units of linguistic replication?*

Another question is what units precisely language memes/linguemes may take? This is an important – and so far unsolved – issue in studies on memetics in general (e.g. Blackmore 1999). For linguistics the question has in particular been raised by Lass (1996) and addressed in great detail by Ritt (2004: ch. 6). In the end, practitioners of evolutionary models of language change more or less seem to agree on the well-known building blocks of linguistic structure, such as phonemes, morphemes, phrases, constructions as well as their corresponding meanings (cf. e.g. Croft 2000: 33; Ritt 2004: ch. 6). Ritt (2004: 132) admits the somewhat shaky status of such established linguistic units, particularly for an evolutionary approach such as his which tries to physically locate linguistic replication in the brain, and he is worried about the fact that linguistic theories are largely ignorant about the mental reality of the representations they propose (as e.g. the phoneme). It should be noted however that there is at least some support for the ‘psychological reality’ of these classic linguistic units from studies of speech errors (see e.g. Fromkin 1971 or Bierwisch 1982). In particular Fromkin (1971) in her pioneering article has shown that the units postulated by theoretical linguists (such as phonetic features, segments, stress, syntactic word classes and phrases and semantic features) are valid units when analysing speech errors. How they are physically implemented in the brain is of course another question; see also below.

*What is the material basis of linguistic replicators (and meme replication)?*

In contrast to biology, it is hard – if not impossible at the present stage – to specify the material basis of cultural replicators. As Dawkins (1999) in his foreword to Blackmore (1999) writes: “Memes have not yet found their Watson and Crick; they even lack their Mendel” (xii). In the same vein Lass (1996: 5) notes: “Memes are troublesome because they’re different from ‘classical’ replicators (genes) in having no immediate canonical physical substrate.” The problem applies also for the specific case of linguistic replicators (or language memes), although its importance is generally played down in the literature. Croft (2000: 12) simply notices a functional equivalent to DNA in linguistic evolution, which he considers to be the utterance. In a similar vein, Ritt (2004: 122, note 1) argues that talking about linguistic replicators does not necessarily have to imply that we can fully specify their physical basis (ibid). Ritt refers to evolutionary theory in

biology, which was successfully formulated by Darwin before the advances of molecular genetics in the 20<sup>th</sup> century, citing also Blackmore (1999) for a similar stance. That notwithstanding, Ritt sets out to sketch a possible physical substrate of linguistic memes in the form of “brain-states” (157), more precisely as Hebbian cell assemblies.<sup>36</sup> Ritt’s definition of a language meme is as follows:

A ‘meme’ represents an assembly of nodes in a network of neurally implemented constituents, which has (a) a definite internal structure, (b) a definable position within a larger network configuration, (c) qualifies as a replicator in Dawkins’ sense. (Ritt 2004: 169)

Yet, Ritt faces the problem that no neurophysiologically plausible linguistic model so far has been put forward in which to accommodate his meme concept (which he freely admits), and thus his sketch, though plausible and a welcome move towards some neurolinguistically realistic approach of evolutionary linguistic change, still lacks empirical and theoretical underpinning from the realm of neurolinguistics and thus remains speculative at this stage.<sup>37</sup>

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<sup>36</sup> The basic idea underlying Hebb’s cell assembly theory is that neurons that are jointly activated also form structural networks. The combination of neurons which can be grouped together as processing units are referred to as ‘cell-assemblies’. Similarly, Changeux & Dehaene (1989) suggest that mental representations (in general) may be spelled out in terms of Hebbian assemblies and they accordingly coined the notion of ‘mental Darwinism’. As Changeux (1985: 272, as cited in Cziko 1995: 69) put it, under such a view “the Darwinism of synapses replaces the Darwinism of genes.” For yet another attempt to give memes (in general, not necessarily exclusively linguistic ones) a neurophysiological basis in terms of neuronal activation patterns, see also Delius (1989).

<sup>37</sup> See however Pulvermüller (2002) for a first step towards a neurobiologically realistic model of language, a work not mentioned by Ritt. Future research will have to show to what extent Pulvermüller’s idea of a ‘neuronal grammar’ may fit in with Ritt’s idea of memes of ‘neurally implemented constituents’. In contrast, connectionist models like those by Rumelhart & McClelland (1986) are so far primarily modelling (or simulating) how things *might* work on the neuronal level, without however claiming to be neurobiologically real in any way. Ritt (2004: 169, note 27), though sympathizing with connectionist models, does not commit himself to a specific model, acknowledging their somewhat problematic status in the literature.

Ultimately, of course, any kind of (phenotypic) behavior can be traced back to some processes going on in the brain, and under such a reductionist view meme replication of course must have a material basis, even if this is still only poorly understood in detail.<sup>38</sup>

*What is the replicating mechanism?*

Even if we can specify the precise nature of linguistic replicators and their relevant units, it still remains unclear how they replicate, i.e. make copies of themselves. That is, what kind of mechanism ensures the (largely) faithful replication of linguemes/language memes? Referring to the typical stance in memetics, Ritt (2004: 196) assumes that cultural replicators (such as language memes) copy via the process of IMITATION (cf. also Blackmore 1999). In Ritt's model, such imitation needs to take place on the neuronal level, though he doesn't go into any details. Building upon earlier suggestions by Ritt (1995, 1996), Lass (2003: 58–59) briefly elaborates on the way that activation may occur in a network organisation of a brain such as Calvin's (1996) diffuse network. Croft (2000) distinguishes between mechanisms for normal replication and mechanisms for altered replication. Basically, he proposes that "conformity to convention" (71) is the mechanism for normal replication, while "violation of convention" (71) is the mechanism for altered replication.<sup>39</sup> Croft does not specify however any material basis or cognitive mechanism for replication, except for briefly alluding to an interactive activation model in the sense used in cognitive grammar (as e.g. Langacker 1987; Bybee 1985) by which "cognitive structures can 'survive'" (32). In general, evolutionary approaches to language change (as to cultural change in general) still face the problem of relating linguistic replication to more specific cognitive mechanisms or even a physical (i.e. neurophysiological) basis. Morrison (2002) argues for a "cognitive neuroscience

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<sup>38</sup> See also Hull (2001: 58) who says that "'phenomenal givens' cannot enter into the causal sequences that produce replication, but for every phenomenal given, there has to be something going on in the brain, and these neuronal memes will do just as well." Note, that such a reductionist view can already be found as early as in Schleicher (1873), who already argued (from his monistic stance) for a material basis of language.

<sup>39</sup> More precisely, Croft elaborates on the various maxims suggested by Keller (1990 [1994]), showing how they either result in the conformity or the violation of convention.

of cultural transmission” (339) and suggests that mirror neurons may play an important part in this (and she is particularly referring to what she calls ‘catchy memes’ and ‘migratory mannerisms’). Mirror neurons are a particular type of neurons found in primates (macaques) which fire when the animal performs an action and when it observes the same action performed by others (Rizzolatti & Arbib 1998; see also the contributions in Stamenov & Gallese 2002). As such, mirror neurons seem to be a good candidate for accounting for meme replication, though it still remains to be seen to what extent they can account for human imitation.

## **5. Taking a fresh look at (some) old problems – the (possible) role of priming in evolutionary models of language change**

In the following I will argue that PRIMING may shed new light on some old problems of evolutionary modelling of language change. Note from the outset that many of the ideas presented here stem from collaborative work with Gerhard Jäger, see also Jäger (2007) or Rosenbach & Jäger (forthcoming) and that, given the scope of the present paper, I can only sketch the general idea behind and suggest directions for further research rather than formulate a comprehensive account.

Priming is a well-known psycholinguistic mechanism that refers to the (usually) increased likelihood of linguistic elements to be repeated in the sense that either speakers are more likely to repeat what they’ve previously said (Bock 1986, and subsequent work) or that hearers may better parse what they’ve previously heard (e.g. Frazier et al. 2000, Luka & Barsalou 2005). Besides production and comprehension, priming effects have also been reported for dialogue (e.g. Pickering & Garrod 2004). Priming is an extremely pervasive phenomenon that operates on the cognitive (and presumably neuronal) level and refers to the ‘pre-activation’ of information, in the sense that the previous activation of a linguistic stimulus (the prime) enhances the likelihood of the same linguistic element (the target) to be repeated.<sup>40</sup> ‘Sameness’ is defined by PRIMEABILITY here, and there is evidence that both identical linguistic elements as well as sufficiently similar linguistic elements can prime each other. To illustrate a case of identity

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<sup>40</sup> Priming in language typically involves facilitation of processing; there is, however, also evidence for inhibitory (i.e. negative) priming effects.



priming, see e.g. the study by Levelt & Kelter (1982), who asked Dutch shopkeepers either (2a) or (2b):

- (2) a. At what time do you close?  
b. What time do you close?

In the first case (2a) people tended to answer with *at six*, while in the second case (2b) they usually replied *six o'clock*, repeating the preposition *at* when it was present in the question. There is, however, also evidence that shows that prime and target need not be identical. For example, Bock & Loebell (1990) show that both a passive prime as in (3a) as well as a prepositional locative as in (3b) could equally prime a passive target construction.<sup>41</sup>

- (3) a. *The 747 was alerted by the airport's control tower*  
b. *The 747 was landing by the airport's control tower*

Note that priming has been shown to operate on all linguistic levels and that there is evidence for the priming of form (on the phonological, lexical, and syntactic level) as well as for the priming of meaning on the semantic level. Priming may now be invoked to connect linguistic evolution more closely to psycholinguistic (or more general cognitive) principles in the following way. First, it provides a plausible cognitive mechanism for both faithful (i.e. normal) replication of linguemes in terms of identity priming as well as for non-faithful (i.e. altered) replication in terms of similarity priming. In the former sense it may be considered as the cognitive mechanism for the imitation, i.e. the self-replication, of language memes (or linguemes) within language usage, cf. also Jäger (2007: §5). In the latter sense priming represents the mechanism for analogical extensions in language use

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<sup>41</sup> The passives and prepositional locatives in (3) are different constructions, but they essentially involve the same type of function words (*as*, *by*). See e.g. Ferreira (2003a: 380–381) for discussing to what extent such cases represent true cases of purely syntactic priming or whether they might be better regarded as priming of identical function words. Ferreira cites evidence, however, showing that it is not only function word repetition which is at stake here. There is also ample evidence from lexical priming showing that the presentation of semantically related objects will enhance the likelihood of a related target to occur (e.g. the previous presentation of a guitar will enhance the naming of a violin, another musical instrument, in contrast to semantically unrelated primes such as e.g. a chair); see e.g. Flores D'Arcais & Schreuder (1987).

in that linguistic elements may prime sufficiently similar targets, and a possible link between priming and analogy has indeed been pointed out within psychological research (e.g. Markman & Gentner 1993; Gentner & Markman 1997), psycholinguistic research (Bock & Kroch 1982) and recently also historical linguistics (Fischer 2007; Rosenbach & Jäger forthcoming).

Rosenbach & Jäger (2006) connect evidence for priming in language usage to unidirectional diachronic change. A case in point discussed in Rosenbach & Jäger (2006) is the well attested fact that temporal expressions may develop from spatial ones, but not *vice versa* (e.g. Heine et al. 1991; Haspelmath 1997). For instance, the temporal use of the English prepositions *at* or *in* (as e.g. in *at noon*, *in January*) developed from their spatial uses (as e.g. in *at the door*, *in the building*), and not the other way round. In a series of experimental studies, Boroditsky (2000) tested whether spatial expressions can prime temporal interpretations and *vice versa*. In these experiments, Boroditsky proceeded from the two basic conceptualisations of time commonly assumed for English, namely

- (a) the ‘ego-moving metaphor’, where an observer (‘ego’) progresses along the time-line and ‘front’ is assigned to the future (as e.g. in *We are coming up on Christmas*), and
- (b) the ‘time-moving metaphor’, where ‘front’ is assigned to the past (as e.g. in *Christmas is coming up*),

corresponding to the two respective spatial metaphors (‘ego-moving metaphor’: *The dark can is in front of me* vs. ‘object-moving metaphor’: *The dark widget is in front of the light widget*). Subjects had then to interpret sentences which were ambiguous with respect to these two conceptualisation, as in (4) and (5).

- (4) Next Wednesday’s meeting has been moved forward two days. When did the meeting take place? (ambiguous time question)
- (5) Which of the two widgets is ahead? (ambiguous space question)

It could be shown that the previous presentation of a spatial description (e.g. *The dark widget is in front of the light widget*; object-moving metaphor) could prime the corresponding time-moving interpretation in (4), with subjects typically assuming that the meeting has been moved to Monday, while the previous presentation of a temporal description as e.g. *Thursday*

*comes before Saturday* (= time-moving metaphor) could not prime the corresponding spatial interpretation in (5). This asymmetry in priming precisely corresponds to the observed historical pathway, and Rosenbach & Jäger (forthcoming) sketch an account of unidirectionality based on such asymmetric priming.

Rosenbach & Jäger (forthc.) also report evidence that priming effects in language usage, which are typically short-lived and decay immediately, may become entrenched; see particularly Chang et al. (2006) for a review of the literature and for presenting a detailed connectionist model in terms of IMPLICIT LEARNING. Similarly to exemplar-models mentioned above, Chang et al.'s (2006) connectionist model allows for token occurrences to have an immediate impact on mental representations, thus bridging the gap between the token and the type levels, and thus usage and competence, considerably.

What makes an evolutionary approach in terms of priming particularly interesting is the fact that it allows for the operationalization of terms which have so far been merely stipulated. Priming as a cognitive mechanism is of course not directly observable; its behavioural effects, however, in terms of priming effects, are, and as such priming is typically used as a *method* in psycholinguistic research. Priming also seems to be well compatible with the neurophysiologically based model that Ritt (2004) proposes, and an approach couched in terms of priming has the apparent advantage of having sufficient support from psycholinguistic research and being able to be subjected to empirical testing, both properties absent from Ritt's model, which is so far basically stipulated only (if quite plausibly so).<sup>42</sup> In particular, priming studies may be used to shed light on the following questions:

1. What are the units of linguistic replication (see also section 4.3 above)?  
– Answer: whatever can be primed.
2. What are possible minimal steps in the process of altered replication (in terms of possible analogical extensions)? – Answer: whatever can be primed.

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<sup>42</sup> Priming may be a useful conceptual 'bridge' for Ritt's approach in that it provides a cognitive mechanism which is subject to empirical testing and is well compatible with a neuronal network like the one suggested by Ritt. From a neurological point of view, priming is the activation of clusters of neurons which store information (corresponding to the 'firing' of neurons). It still remains to be seen, however, to what extent priming on the behavioural level corresponds to priming on the neuronal level, some first (cautious) steps towards connecting the two are made by Pulvermüller (2002).

So, for example, if spatial primes can prime temporal interpretations, as in Boroditsky's (2000) experiments described above, this constitutes evidence that space and time are (i) conceptual categories that can replicate, (ii) that they are sufficiently similar to each other and (iii) that this analogical transfer is indeed a minimal step that can be made in actual usage.

Note, however, that priming as such cannot explain *why* a certain lingueme/language meme is used in the first place. It may only account for the fact of why, once used, this very lingueme/language meme is more likely to be used again. That is, logically prior are selectional pressures (functional, social, or whatever) that favour the use of one variant over the other, and then *once used*, this variant is even more likely to be used as priming kicks in as yet another factor, cf. figure 4 below.<sup>43</sup>

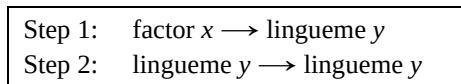


Figure 4. Priming as a secondary factor in linguistic replication

Under this view, priming is at the same time a replication mechanism as well as a kind of secondary factor operating in linguistic selection, which is concordant with work from historical linguistics showing that frequency (which under the present view is inherently related to priming) is both a result as well as a factor in linguistic change (cf. also Rosenbach 2002: 247–248). As a linguistic factor, it corresponds to reinforcing entrenchment effects in terms of repeated activation (see also Croft 2000: 74); in evolutionary terms this amounts to POSITIVE FEEDBACK effects, which are common in evolutionary change (see e.g. the excellent discussion in Dawkins 1986: chapter 8). Given such positive feedback effects, a natural question to ask is why it is that infrequent linguistic items may persist at all in language. This question is addressed by Ferreira (2003b), who shows that dispreferred (i.e. infrequent) structures display a stronger priming effect than preferred (i.e. frequent) structures. Ferreira accounts for this interesting result in terms of Chang et al's (2006) model of implicit learning, arguing that the error-based nature of this model ensures a higher learning rate for infrequent structures. That is, the occurrence of uncommon structures re-

<sup>43</sup> Alternatively, see Rosenbach & Jäger (forthc.: §5.4) for evoking co-activation of meaning and form in the case of space-to-time priming as a possible source for the emergence of the temporal meaning of prepositions.

sults in some ‘deeper’ kind of cognitive entrenchment, which somewhat counteracts their disadvantage of not having a particularly good chance to be primed in the first place, thus rescuing them from dropping out of use completely. In this sense, then, priming as a cognitive mechanism may account for both the reinforcement of frequent variants as well as the maintenance of infrequent ones, and both effects may be attributed to the same general cognitive mechanism, i.e. (error-based) implicit learning.<sup>44</sup>

Another possible area for applying the cognitive mechanism of priming to language change is language contact. There is evidence for structural priming across languages (Loebell & Bock 2003; Meijer & Fox Tree 2003; Hartsuiker et al. 2004) in that the use of a construction in language A may prime the use of the same construction in language B, irrespective of the lexical material. For example, Loebell & Bock (2003) showed that the previous presentation of a German prepositional dative as in (6) can prime an English prepositional dative target in the description of pictures depicting dative events as in (7).

(6) Das Mädchen kaufte eine Zeitung für die blinde Frau.  
‘The girl bought a paper for the blind woman.’

(7) The girl tosses a ball to another girl.

Research on language contact has recently stressed the interaction between bilingual speakers in language usage as the major source for contact-induced change (Heine & Kuteva 2005; Matras & Sakel 2007). Cross-linguistic priming is a plausible cognitive mechanism to account for contact-induced change on the level of bilingual language use (see also Loebell & Bock 2003), and such a psycholinguistic account of language contact can be embedded within an evolutionary framework of language change. As e.g. argued by Heine & Kuteva (2005), rather than borrowing completely new

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<sup>44</sup> A reviewer points out a recent study by Travis (2006), which shows priming of subject pronouns in Colombian Spanish. As overt subject pronouns constitute a marked variant of null subjects in this pro-drop variety of Spanish, the reviewer asks why priming may reinforce a less frequent variant in this case, and how this fits in within an account of priming as a cognitive mechanism of differential replication? Notice, however, that priming only acts as an ‘amplifier’ on structures already produced and thus its effects depend on the likelihood of forms/constructions to be produced in the first place. Less frequent variants therefore overall are less likely chance to be primed at all.

grammatical material, languages more frequently use a minor ‘use pattern’ already present in the language and develop it into a major ‘use pattern’ on the model of another language. For example, Heine & Kuteva (2005: 46) report a study by Riehl (2001) which shows that German speakers in Romance speaking areas tend to use the postnominal possessive *Zeit des Herbstes* (‘time of autumn’) or *das Bündel von Trauben* (‘the bunch of grapes’) instead of the nominal compound *Herbstzeit* (‘autumn time’) or *Traubenbündel* (‘grape bunch’), which usually would be the preferred form in German, modelled on the Romance postnominal pattern (cp. French *le temps d’automne* or Italian *il grappolo d’uva*). That is, the role of contact consists in promoting a construction which is already present (though typically dispreferred) in German as opposed to the otherwise preferred construction. In evolutionary terms, this constitutes a classic case of ‘altered replication’ of already existing material (if occurring between different languages). Empirical studies testing for the plausibility of specific contact-induced changes by way of cross-linguistic priming are still missing, but the phenomena are testable in principle. For the example discussed above, the prediction for a priming study would be that, all other things being equal, (bilingual) German speakers should be more likely to produce the (usually dispreferred) postnominal pattern (*das Bündel von Trauben*) after previously having heard or uttered a corresponding Romance postnominal variant (e.g. French *le temps d’automne*) than otherwise. If this can be shown to be the case, this would provide psycholinguistic evidence that the observed shift of German towards more postnominal variants in Romance areas may be directly related to cross-linguistic priming in language use.

It also remains to be seen to what extent Trudgill’s theory of dialect mixing (Trudgill 1986, and subsequent work) may be related to priming in usage. One important mechanism in Trudgill’s theory is ACCOMMODATION, i.e. speakers’ convergence towards the speech of their interlocutors, which is supposed to lead to rudimentary dialect levelling in the first stage of new dialect formation when speakers of different regional and social varieties engage in face-to-face communication in a new location. Again, priming may be used as a heuristic to test for the likelihood of certain types of accommodations to occur rather than others, and at the same time provide the cognitive mechanism underlying such accommodation.

All this indicates that an evolutionary approach to language change based on cognitive principles is well compatible with the phenomenon of language contact (see e.g. Blevins 2006 and Carstairs-McCarthy 2005 for criticising Ritt 2004 for ignoring matters of language contact in his ap-

proach). Crucially, priming does not only offer a possible cognitive explanation for the observed change phenomena (viz. unidirectionality and contact-induced change), but at the same time also turns the issue of unidirectionality and contact-induced change into an empirical question, as it allows for testing present-day speakers for past changes, under the uniformitarian assumption that the brains of today's speakers do not differ from past speakers' brains. And it can accommodate these phenomena within an evolutionary approach to language change, though admittedly I can only sketch the idea here and an evolutionary framework based on priming still needs to be spelled out in more detail. Despite the very preliminary nature of this proposal, I think that couching an evolutionary approach to language change in terms of priming may allow us to overcome some notorious problems of evolutionary language change models in that it may help

- (i) to ground them more closely within cognition (and ultimately brain structure and behaviour), and
- (ii) to make their assumptions more closely subject to empirical testing by psycholinguistic studies.

## 6. Conclusion

This paper has tried to give an overview on evolutionary approaches to language change, not in the sense that linguistic evolution *is* biological evolution, but in the sense that language change, as an instance of cultural evolution, is driven by the same evolutionary forces driving biological evolution. I have (deliberately) not elaborated on the many differences between biological and cultural evolution, and language change in particular; these issues have been dealt with in detail in other places (see e.g. Gerard et al. 1956; Stevick 1963; Keller 1990 [1994], Croft 2000, Kirby et al. 2004 or Ritt 2004, to mention just a few). Instead I have tried to highlight some of the pertinent questions that are currently being discussed in the literature. Finally, I have argued that the psycholinguistic mechanism of priming may add a novel and interesting perspective to the evolutionary modelling of language change.

So far, so good, but one might reasonably ask what we gain by adopting an evolutionary approach to language change? Why bother at all? For one, the evolutionary perspective allows us to put equal emphasis on both language change *and* stability in language in the sense that normal (or faithful) replication results in stability while only altered replication results in

change, whereas traditionally historically linguistics has primarily focussed on how and why language changes and less on how and why it remains stable (cf. e.g. also Keller 1990 [1994]: §6.1; Croft 2000: 4; Ritt 1995: 45, 2004: 48 and *passim*). The major attraction, however, as I see it, lies in the fact that we may view – and investigate – language change as being subject to the same processes that shape any historically evolved complex system. From a scientific point of view, having the choice between domain-specific and domain-independent processes, the latter (more general) are to be preferred. Taking an evolutionary approach to language change, then, enables us to look at biological evolution, which is far better understood than cultural evolution, to find possible answers to old questions in historical linguistics,<sup>45</sup> though it should again be emphasized (as this is so often subject to misunderstanding) that the two processes only share properties on an abstract level and otherwise differ in detail.

### Acknowledgements

I'm particularly indebted to Gerhard Jäger for many discussions and exchanges on evolution and evolutionary modelling, and some of the ideas presented here in fact stem from our collaborative work. Thanks also to Roger Lass for various discussions on the topic over the past years and to an anonymous reviewer for valuable suggestions. I was also inspired by the discussions in a class given by Rudi Keller in the summer of 2002, where we read and discussed Croft (2000) in detail. I'm also grateful to Bill Croft for discussing aspects of his work with me. Of course, none of these people are to be blamed for any misinterpretations or remaining errors. This work was supported by a grant by the *Deutsche Forschungsgemeinschaft* (Ro 2408/2–2), which is gratefully acknowledged.

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<sup>45</sup> This line of reasoning can, for example, be found in Nettle (1999b), who argues that findings from evolutionary biology may be used to avoid functional arguments in linguistics being *posthoc* by deriving functional motivations from independent models. Another case in point for the useful transfer of insights from evolutionary biology to linguistic evolution is the notion of 'exaptation' (Lass 1990, 1997), which recently has also come to be invoked to shed light on the debate on the unidirectionality in grammaticalization processes (in particular with respect to the status of possible counter-examples to it); see e.g. Norde (2001), Rosenbach (2002: 254–255), or Traugott (2004). A related concept, although without the biological metaphor, is the notion of 'functional renewal' by Brinton & Stein (1995).



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# **Formal Approaches**



# Language change as a source of word order correlations

*Brady Clark, Matthew Goldrick and Kenneth Konopka*

## 1. Structural preferences in typology and change

### 1.1. Introduction

One main way in which natural languages differ is in their word order. For example, auxiliary verbs (Aux) can precede or follow content verbs in both verb-object (VO) and object-verb (OV) languages.<sup>1</sup> All of the logically possible combinations of orderings of auxiliary verbs, content verbs, and objects, given in (1), are attested stable grammatical states.

- (1) a. OV&VAux (e.g. Slave, Siroi; Dryer 2007)
- b. OV&AuxV (e.g. Seme, Sorbian; Matthew Dryer, p.c.)
- c. VO&VAux (e.g. Akan, Gumuz; Matthew Dryer, p.c.)
- d. VO&AuxV (e.g. English)

Despite extraordinary cross-linguistic variation, though, certain word orders are more frequent than others. In the following two sections we provide evidence from typology and change that there are robust preferences for certain word order patterns.

### 1.2. Evidence from typology

Typological work has demonstrated that there are robust word order correlations cross-linguistically (Greenberg 1966; Hawkins 1983; Dryer 1992). For example, there is a strong tendency for auxiliaries to precede the content verb in VO languages (i.e. VO&AuxV), while auxiliaries tend to follow in

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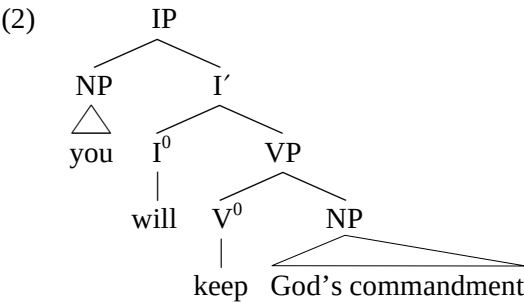
<sup>1</sup> We adopt Dryer's (1992: 100) use of AUXILIARY VERB here: tense/aspect words that are specifically verbal. For English, this category includes *will*, *have*, and progressive *be*, but not the passive auxiliary *be* and modal auxiliaries such as *can* and *should*. A CONTENT VERB is the verb with which the auxiliary verb combines.

OV languages (i.e. OV&VAux) (Dryer 1992). This tendency is illustrated in Table 1.<sup>2</sup>

*Table 1.* Order of content verb and auxiliary verb (Dryer 1992: 100)

|         | Africa | Eurasia | SEAsia&Oc | Aus–NewGui | NAmer | SAmer | Total |
|---------|--------|---------|-----------|------------|-------|-------|-------|
| OV&VAux | 5      | 12      | 2         | 8          | 1     | 8     | 36    |
| OV&AuxV | 3      | 0       | 0         | 0          | 0     | 0     | 3     |
| VO&VAux | 1      | 1       | 0         | 1          | 0     | 1     | 4     |
| VO&AuxV | 15     | 5       | 3         | 0          | 4     | 1     | 28    |

Present-day English is VO&AuxV. English clauses with both an auxiliary and a content verb have a consistently right-branching structure, illustrated in (2), where auxiliaries sit in the head of IP:<sup>3</sup>

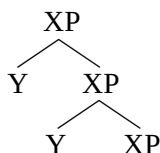


In general, there is a typological tendency for languages to converge on one of two ideals (Dryer 1992): right-branching languages (where phrasal categories such as VP follow non-phrasal categories such as I<sup>0</sup>, e.g. English), as in (3a), or left-branching languages (where phrasal categories precede non-phrasal categories, e.g. Japanese), as in (3b).

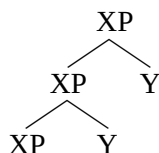
<sup>2</sup> The form of the data in Table 1 is discussed in detail in Dryer (1992). The numbers represent the number of genera that contain languages of the given type in the geographic area listed. A genus is a genetic group roughly comparable in time depth to the subfamilies of Indo-European.

<sup>3</sup> In (2), the category I describes auxiliary verbs, the category N describes nouns, and the category V describes verbs.

(3) a. Right-branching:



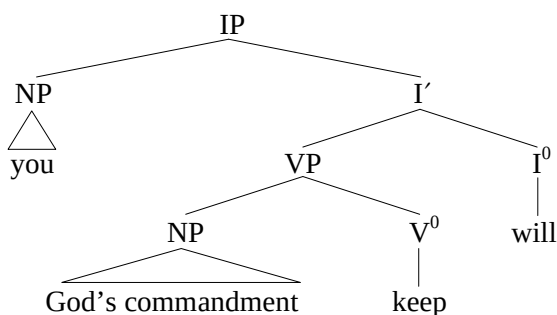
b. Left-branching:



### 1.3. Evidence from change

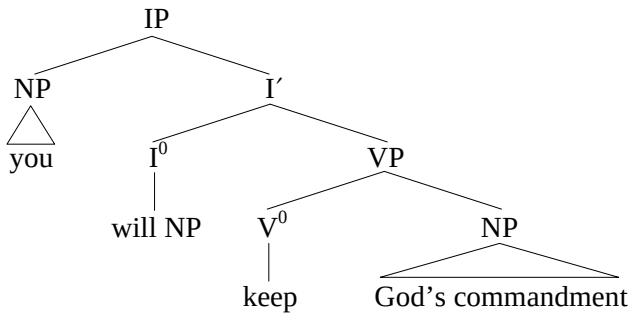
The preference for consistent branching observed in typology can also be seen diachronically, e.g. in the history of English. Late Old English (925–1150) and early Middle English (1150–1325) subordinate clauses displayed both intertextually and intratextually (at least) three structures (Pintzuk 1999; Kroch & Taylor 2001; Clark 2004), given in (4a–c). Note that the BRACE construction in (4c) has inconsistent branching: the non-phrasal category  $I^0$  is a left-sister of the phrasal category VP, while the non-phrasal category  $V^0$  is a right-sister of a phrasal category YP. In contrast, the ALL-FINAL and ALL-MEDIAL constructions in (4a) and (4b) have consistent branching.<sup>4</sup>

(4) a. ALL-FINAL (OV&VAux, *you God's commandment keep will*):

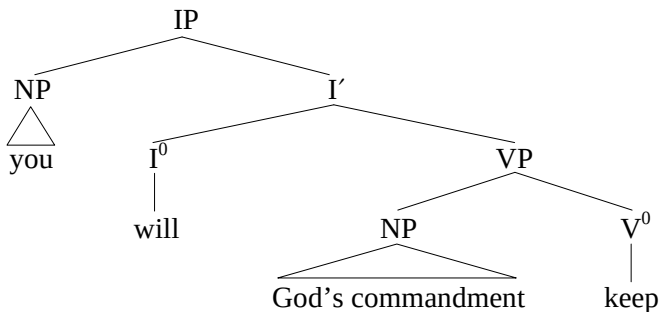


<sup>4</sup> We are simplifying a bit here. Clark (2004) argues for a verbal cluster analysis of the all-final construction.

- b. **ALL-MEDIAL** (VO&AuxV, *you will keep God's commandment*):



- c. **BRACE** (OV&AuxV, *you will God's commandment keep*):



Roughly speaking, of the three variants in (4a–c), the all-final structure in (4a) was most frequent within Old English subordinate clauses. In early Middle English the all-medial structure was most frequent. While the inconsistent brace construction was available at a low frequency at both of these stages of the language, the frequency of the brace construction gradually declined over the course of Middle English. (5)–(7) give examples of all three variants in late Old English. (8)–(10) give evidence from early Middle English.

(5) **ALL-FINAL**

*him þær se gionga cyning þæs oferfæreldeð forwiernan mehte*  
 him there the young king the crossing prevent could  
 ‘... the young king could prevent him from crossing there’

(c800–900, Orosius 44.19–20;  
 Source: Pintzuk 1996: 245)

(6) **ALL-MEDIAL**

*he wolde adraefan ut anne æþeling*  
 he would drive out a prince  
 ‘... he would drive out a prince...’

(c1000–1100, ChronB(T) 82.18–19;  
 Source: Pintzuk 1999: 104)

(7) **BRACE**

*he mæg þa synfullan sawle þurh his gife geliffæstan*  
 he may the sinful soul through his gift endow-with-life  
 ‘He can endow the sinful soul with life through his grace’

(c900–1000, Ælfric’s Homilies I, 33.496.30;  
 Source: Fischer et al. 2000: 143)

(8) **ALL-FINAL**

*3ef 3e þus godes heste halden wulleð*  
 if you thus God’s commandment keep will  
 ‘if you will thus keep God’s commandment’

(c1225, Ancrone Riwe, II.141.1889;  
 Source: Kroch & Taylor 2001: 141, PPCME2)

(9) **ALL-MEDIAL**

*oðet he habbe izetted ou al þet he wulleð*  
 until he has granted you all that you desire  
 ‘until he has granted you all that you desire’

(c1225, Ancrone Riwe, II.68.229;  
 Source: Kroch & Taylor 2001: 145, PPCME2)

(10) **BRACE**

*ðanne hie willeð here ibede to godde bidden*  
 when they will their prayer to God pray  
 ‘when will they pray their prayer to God’

(c1200, Vices and Virtues I, 143.1773;  
 Source: Kroch & Taylor 2001: 154, PPCME2)

Table 2 illustrates the estimated relative frequency of the three variants in (4a–c) within two texts, the late Old English text *Chronicle A* (Scribe 1) and the early Middle English text *Festis Marie*. The numbers are inferred from the frequency information in Pintzuk (1999) (for *Chronicle A*, Scribe 1) and Allen (2000) (for *Festis Marie*). Crucially, all three variants are present in-



tratextually. As we argue below, any model of language acquisition and use must be able to capture intratextual variability of this sort (Kroch 2001; Yang 2002; Clark 2004).

*Table 2.* Estimated frequencies of structures (4a–c) in Chron A, Scribe 1 (OE) and *Festis Marie* (early ME)

| structures                                                        | % for Chron A, Scribe 1 | % for <i>Festis Marie</i> |
|-------------------------------------------------------------------|-------------------------|---------------------------|
| (4a) [ <sub>IP</sub> XP [ <sub>I'</sub> [ <sub>VP</sub> YP V] I]] | 61                      | 8                         |
| (4b) [ <sub>IP</sub> XP [ <sub>I'</sub> I [ <sub>VP</sub> V YP]]] | 1                       | 67                        |
| (4c) [ <sub>IP</sub> XP [ <sub>I'</sub> I [ <sub>VP</sub> YP V]]] | 38                      | 25                        |

In sum, in the history of English we see a gradual convergence on the consistent right-branching structure in (4b), where phrasal categories such as direct objects follow non-phrasal categories such as non-finite verbs. This change is arguably a reflection of the typological preference for consistent branching discussed above. The typologically rare brace order in (4c) was available at each stage of early English, but was never the preferred option, neither within nor across speakers.

**2. Accounting for structural preferences**

The previous section discussed the following two observations:

- i. There is extraordinary cross-linguistic word order variation. For example, all of the logically possible combinations of orderings of auxiliary verbs, content verbs, and objects are attested stable grammatical states.
- ii. Certain word order patterns (e.g. VO&AuxV, OV&VAux) are more frequent than others (e.g. VO&VAux, OV&AuxV) and this reflects a preference for consistently branching structures.

There are several types of explanations for why languages tend to converge on consistently right-branching or consistently left-branching structures. We focus on two related types of explanations here: purely syntactic explanations and cultural evolution explanations.

## 2.1. Purely syntactic accounts

Starting with Greenberg (1966), there is a long tradition of purely syntactic explanations for cross-linguistic word order correlations (e.g. the fact that OV languages tend to be VAux), see, e.g., Greenberg (1966), Lehmann (1973), Vennemann (1973), Hawkins (1983), Svenonius (2000), and Biberauer & Roberts (2005). Syntactic accounts attempt to explain word order correlations solely in terms of constraints on phrase structure relations, e.g. between heads and dependents, or between phrasal categories and non-phrasal categories. For example, Greenberg (1966) suggests that word order correlations reflect a tendency to consistently order heads with respect to their dependents/modifiers. Greenberg's syntactic explanation for word order correlations was the germ for later syntactic explanations, e.g. Hawkins' (1983) principle of Cross-Category Harmony.

An underlying assumption of the syntactic approach is that consistent languages involve simpler grammars while inconsistent grammars involve more complex grammars, and that language learners disprefer complex grammars:

A disharmonic language requires more category-particular rules and thus the grammar of such a language is more complex. (Mallison & Blake 1981)

For example, in recent work Biberauer & Roberts (2005) tie word order correlations to a "least-effort" strategy applied to parameter setting. Biberauer & Roberts (2005: 38) suggest that language acquirers "will, given evidence for a particular setting of one of a series of isomorphic parameters, set all the isomorphic parameters [e.g. for T and v] the same way", unless overridden by primary linguistic data. Consequently, grammars with simpler structural representations (e.g. consistently branching structures) are preferred over grammars with more complex representations (e.g. inconsistently branching structures). The crucial property of this and earlier syntactic explanations is that they seek to explain word order correlations purely in terms of a language-specific predisposition for simpler structures. This predisposition for simpler structures is assumed to be part of the genetic endowment of the language learner. As pointed out by Brighton, Kirby & Smith (2005: 291), this type of account of typological generalizations depends on "the assumption that properties of the cognitive mechanisms supporting language map *directly* onto the universal features of language we observe."

## 2.2. Cultural evolution accounts

In contrast to purely syntactic accounts, typological generalizations such as word order correlations can be explained in terms of non-genetic, cultural evolution, i.e. language change (Kirby 1999; Jäger & van Rooij 2005). In cultural evolution accounts, typological generalizations are emergent in a population from repeated cycles of language use and acquisition. This type of account makes certain key assumptions about how language change progresses. First, in order for a linguistic form to spread after it has been introduced into a population (e.g. as a consequence of language contact), language users must be able to learn and use the new form. Second, language users must have a BIAS (incentive) to do so, i.e. the new form has some social or structural advantage over the old form. There are several reasons why there might be a bias for a particular linguistic form. These reasons include (Jäger & van Rooij 2005):

- Learnability: Some forms are easier to learn than others.
- Processing: Some forms are less costly in processing.
- Use: Some forms are more useful in actual conversation.

A central claim of cultural evolution accounts is that the selective pressure of biases for particular linguistic forms results in the emergence of typological generalizations over many generations. Proponents of cultural evolution accounts are typically concerned with explaining typological generalizations via evidence of fit between structure and language use (Kirby 1999: 10). For example, Hawkins (1994) proposes that word order correlations are ultimately a reflection of parsing complexity. Kirby (1999) shows how parsing principles such as those proposed by Hawkins can have a selective effect on the forms that make up the learning experience.

Functionalist explanations for typological generalizations such as Hawkins (1994) have been criticized on methodological grounds, e.g. that they are constructed after the fact “in the sense that there tends to be an *ad hoc* search for functions that match the universals to be explained” (Kirby 1999: 13). One tool that can be used to circumvent this criticism is computer simulations of language use and acquisition. Computer simulations enable us to model cultural evolutionary explanations for language universals and explore the consequences of varying side conditions (Jäger & van Rooij 2005).

## 2.3. Overview of this paper

Our goal in this paper is to provide a cultural evolution account for the two observations presented at the beginning of this section. We use agent-based modeling to demonstrate that these properties of word order variation emerge in a population of BIASED VARIATIONAL learners. Along the way, we will contrast several different models of language use and acquisition, highlighting results that would not have been discovered by looking at solely one model. We first examine properties of FILTERED LEARNING MODELS (Kirby 1999). As demonstrated by Kirby (1999), this class of model can explain the emergence of typological generalizations such as word order correlations. However, the Filtered Learning Model presented by Kirby (1999) makes questionable assumptions about language learners. The second class of models we analyze are what we call VARIATIONAL LEARNING MODELS (Yang 2002; Clark 2004). This class of model makes reasonable assumptions about language learners but fails to capture the emergence of typological generalizations. We discuss computer simulations that suggest that Filtered Learning Models can capture the emergence of typological generalizations, independent of assumptions about the language learner. Further, we demonstrate that a filtered version of the Variational Learning Model proposed by Yang (1999, 2000, 2002) overcomes the limitations of Kirby's (1999) Filtered Learning Model, while simultaneously capturing the emergence of typological generalizations.

## 3. Filtered Learning Models

### 3.1. Filtering

Filtered Learning Models (Kirby 1999, Briscoe 2001) introduce biases toward certain linguistic structures, e.g. consistently branching structures (Kirby 1999). In this class of model, these biases act as filters on the language data that speakers produce and acquirers perceive. As a consequence, the input that acquirers use to establish their language model<sup>5</sup> are adjusted in favor of the preferred structures. Correspondingly, preferred structures increase in frequency over time. (11) presents our assumptions about the

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<sup>5</sup> We use *language model* here as a neutral term for a language user's mental linguistic competence. A language model could include multiple grammars.

transmission process from speakers to acquirers.<sup>6</sup> Filtering can happen at Step 1 and/or Step 2 in (11).

(11) Steps of the transmission process:

1. The language model of the speaker is used to produce utterances.
2. The acquirer perceives utterances.
3. The acquirer uses perceived utterances to establish their language model.

In the Filtered Learning Model presented by Kirby (1999), only successfully parsed (i.e. perceived) observations affect learning. The parser will occasionally fail, and, consequently, acts as a filter on the raw language data, i.e. filtering happens at Step 2 in (11). The set of utterances that is used by acquirers to establish their language model is a subset of the raw language data. Thus, we can capture the observation that more parsable (learnable) variants increase in frequency over time. In contrast, rather than claiming that differential parsability causes differential learnability, Hawkins (1994: 83–95) argues that parsing influences GENERATION, and that more parsable variants will be used more frequently than less parsable ones, i.e. filtering happens at Step 1 in (11). Briscoe (1998) shows that either Hawkins' model or Kirby's, or a combination thereof, accounts for language change in favor of more parsable variants. The implementation of the Filtered Learning Model discussed in this section is completely agnostic about whether parsing influences acquisition or generation and about the source(s) of biases for particular linguistic forms.

### 3.2. Kirby's (1999) Filtered Learning Model

In this section, we discuss the individual components of the Filtered Learning Model. The discussion is modeled after Kirby (1999: 42–47). Imagine a language with the brace construction as the basic order for clauses with a nonfinite verb and an auxiliary verb. Such languages do exist, as indicated

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<sup>6</sup> (11) is meant to encompass PURELY VERTICAL, OBLIQUE, and HORIZONTAL transmission (Cavalli-Sforza & Feldman 1981; Niyogi 2002). Purely vertical transmission involves transmission from parents to children. Oblique transmission involves transmission where members of the parental generation other than the parents affect acquisition. Horizontal transmission involves transmission where members of the same generation influence the acquirer.

in Table 1. For example, Koopman (1984) argues that the West African language Vata is an INFL-medial OV (OV&AuxV, brace) language. If the consistent all-medial structure is introduced into the language (by language contact, by expressiveness), then the relative well-formedness of the all-medial structure and the brace structure predicts that the all-medial structure should win over time. The acquisition process in the Filtered Learning Model is described in (12) (adapted from Kirby 1999: 45):

(12) Acquisition process with filtering:

1. consider the number of brace constructions and all-medial constructions in the input;
2. convert these numbers into probabilities reflecting the chance of each variant being chosen at random from the sample to trigger acquisition;
3. scale those probabilities (e.g. by using the Early Immediate Constituents metric for the variants; see Hawkins 1994 and Kirby 1999) so that the probability of the all-medial construction being used for acquisition is raised and the probability of the brace construction being used is lowered.

The equation in (13) accounts for the way in which the filtered subset of utterances that is used by acquirers to establish their language model is selected.<sup>7</sup>  $p(f)$  (e.g.  $p(\text{all-medial})$ ) is the probability of the construction  $f$  occurring in the trigger experience.  $n_f$  is the number of tokens of the construction  $f$  in the language data.  $\alpha$  corresponds to the learning bias for the preferred structure.

$$(13) \quad p(\text{all-medial}) = \frac{n_{\text{all-medial}}}{n_{\text{all-medial}} + (1 - \alpha)n_{\text{brace}}}$$

In order to explore the consequences of the Filtered Learning Model, we constructed computer simulations within Swarm, a software package for multi-

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<sup>7</sup> (13) is equivalent to Kirby's (1999: 46) equation, given in (i):

$$(i) \quad p(\text{all-medial}) = \frac{.89.n_{\text{all-medial}}}{.89.n_{\text{all-medial}} + .61.n_{\text{brace}}}$$

Assuming a two-word NP, the values .89 and .61 correspond to the aggregate Immediate Constituent-to-word ratios for the all-medial construction and brace construction, respectively (see Hawkins 1994 and Kirby 1999). For this case,  $(1 - \alpha)$  in (13) would equal  $\frac{.61}{.89}$ .

agent simulation of complex systems.<sup>8</sup> The simulations include the following components (Kirby 1999: 43f.):

- (14) a. Utterances: Features of sentences, e.g. OV, VO.
- b. Arena of use: An unstructured pool of utterances.
- c. Grammars: List of possible utterances, e.g. [OV].

The implementation of the Filtered Learning Model described in Kirby (1999) makes certain key assumptions about speakers and language learners that differentiate it from the Variational Learning Model we describe in Section 4. As noted in the introduction, during periods of word order change, linguistic behavior is variable both at the level of the community and at the level of the individual (see Table 2). For Kirby (1999: 37), the frequency of use of a particular word order during language change is taken to be a reflection of the use of that order by a particular speech community. Individual language learners, though, do not learn the frequency of use of a particular word order. For example, individual linguistic competence can only be OV or VO, not both (Kirby 1999: 45):

...it is possible to have different frequencies for different orders without compromising a theory of ‘all-or-nothing’ competence. (Kirby 1999: 37)

In this section, we discuss computer simulations of Kirby’s Filtered Learning Model, showing that this model can capture the emergence of typological generalizations. We then discuss certain architectural and empirical limitations of Kirby’s model.

In addition to the components described in (14), Kirby’s Filtered Learning Model includes the components in (15).

- (15) a. Speakers: A speech community which is made up of a set of speakers each of which consists of a single grammar. These grammars produce utterances for input to the arena of use.
- b. Acquirers: There are speakers who have not been assigned grammars. They take as learning data utterances from the arena of use.

Two dynamic processes – production and acquisition – govern the interaction of the components in (14) and (15) (Kirby 1999: 44). These processes

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<sup>8</sup> [www.swarm.org](http://www.swarm.org)

are described in (16). A key feature of the acquisition process is the assumption that the trigger experience is mapped directly to a unique grammar. In this way, Kirby's model of acquisition is an example of the TRANSFORMATIONAL LEARNING APPROACH to language acquisition: the state of the acquirer undergoes direct changes as an old hypothesis is replaced by a new one. As Yang (2003: ch. 2) points out, this approach is formally insufficient and incompatible with what is known about child language acquisition. We return to this point in the conclusion to this section.

- (16) a. Production: Speakers add utterances to the arena of use in line with their grammars.
- b. Acquisition: Acquirers develop a grammar (and become speakers) by
1. taking a random subset of utterances from the arena of use,
  2. modifying the subset through the process of filtering described in (12),
  3. choosing an utterance – the *trigger* – from the modified subset, and
  4. mapping the trigger directly to a grammar

### 3.3. Population-level characteristics of Kirby's (1999) model

Figure 1 shows the time course of change for simulation runs of Kirby's Filtered Learning Model, at varying levels of the learning bias (henceforth,  $\alpha$ ) for the preferred variant. The initial frequency of the preferred structure was held constant at 20% for each run. Likewise, the random subset of utterances that acquirers take from the arena of use (the sample rate) was held constant at 10%. Each simulation was run for 100 iterations, after which the arena of use consisted entirely of the preferred variant when  $\alpha > 0$ . A key feature of this graph is that when  $\alpha > 0$  (i.e. any positive bias) the slopes of change resemble the S-curve (Weinreich et al. 1968; Kroch 1989). When  $\alpha = 0$  (no bias), random walk behavior is observed.



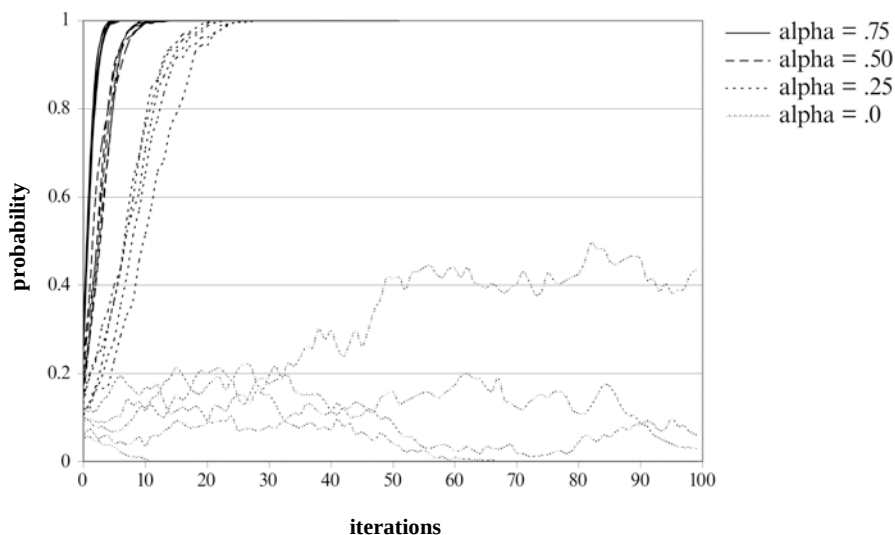
**Filtered Learning Model (Kirby 1999)**

Figure 1. Filtered Learning Model (Kirby 1999) with varying levels of bias

These simulation results suggest that languages can adapt to an asymmetric functional pressure through a process of non-genetic cultural evolution (i.e. language change). If this type of model is on the right track, we should reject accounts that depend entirely on a direct mapping between the cognitive mechanisms supporting language and typological generalizations such as the purely syntactic accounts described in Section 2.1 (Kirby 1999: 135).

### 3.4. Individual-level characteristics of Kirby's (1999) model

The Filtered Learning Model presented in Kirby (1999) is inadequate for several reasons hinted at earlier. Kirby assumes a TRANSFORMATIONAL LEARNING APPROACH<sup>9</sup> to language acquisition in which the state of the acquirer undergoes direct changes as an old hypothesis is replaced by a new one. This approach has been shown to be formally insufficient, see Yang (2002: 18–20) for a summary. The transformational learning approach has also been demonstrated to be incompatible with what is known about chil-

<sup>9</sup> This term is borrowed by Yang (2002: 15) from evolutionary biology (Lewontin 1985).

dren's linguistic development. In all transformational learning models, including the one assumed by Kirby, the learner is always identified with a single, unique grammar. This predicts, among other things, that abrupt changes in the use of linguistic expressions should be observed as the acquirer shifts from grammar to grammar. There is no evidence of this development. Rather, language development is gradual (Yang 2002).

Figure 2 presents a simulation run illustrating the gradual spread of a preferred grammar through a speech community. In contrast to Figure 1, which showed the time course of change for the entire population, Figure 2 shows the distribution of variants at the level of individual speakers. At each stage of the change we see INTERSPEAKER, but not INTRASPEAKER, variation.

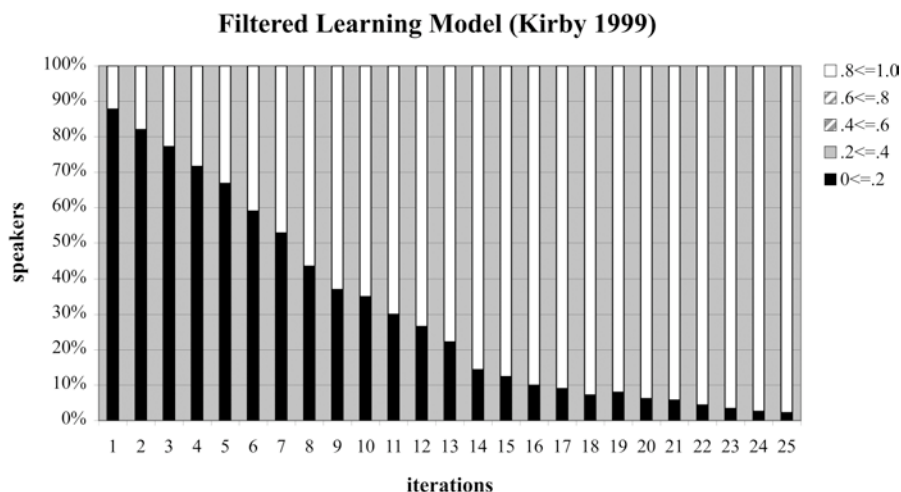


Figure 2. Grammars in the speech community during a period of change

The historical linguistics and variationist literature strongly suggest that the trajectory predicted by Figure 2 is completely unattested: both intraspeaker and interspeaker variation are always observed during periods of change (Weinreich et al. 1968; Kroch 2001; Clark 2004). Work in the variationist tradition (starting with Weinreich et al. 1968) has provided ample evidence that linguistic competence accommodates and generates variation. Intratextual variability was illustrated in Table 2, repeated here as Table 3. For the authors of the two texts described in Table 3, at least three orderings are available for subordinate clauses with a subject, an object, a (pre-)auxiliary finite verb, and nonfinite verb.

Table 3. Estimated frequencies of structures (4a–c) in Chron A, Scribe 1 (OE) and Festis Marie (early ME)

| structures                                                       | % for Chron A, Scribe 1 | % for <i>Festis Marie</i> |
|------------------------------------------------------------------|-------------------------|---------------------------|
| (4a) [ <sub>IP</sub> XP [ <sub>I</sub> [ <sub>VP</sub> YP V] I]] | 61                      | 8                         |
| (4b) [ <sub>IP</sub> XP [ <sub>I</sub> I [ <sub>VP</sub> V YP]]] | 1                       | 67                        |
| (4c) [ <sub>IP</sub> XP [ <sub>I</sub> I [ <sub>VP</sub> YP V]]] | 38                      | 25                        |

Data such as that in Table 3, along with the generalizations about child language development noted above, suggest that our model should make it possible to have different frequencies for different structures without adopting an empirically false notion of ‘all-or-nothing’ competence. Rather, we need a model of linguistic competence that accommodates and generates variation.

Lastly, a key underlying assumption of Kirby’s Filtered Learning Model is that language acquisition is probabilistic. In Kirby’s model, a language acquirer’s grammar is determined by the probability of the grammar in the filtered subset of utterances taken from the arena of use. For example, if the frequency of the preferred variant in the filtered subset of utterances is 90%, there is a 10% chance that the learner will acquire the less preferred structure. Kirby’s assumption that language acquisition is probabilistic is contrary to what is known about language acquisition. Research on language acquisition has shown that children are highly competent and robust learners: “it seems unlikely that, given similar experience, children would attain languages that differ substantially” (Yang 2000: 237).

In the next section, we describe a model of language acquisition and use which overcomes the limitations of Kirby’s Filtered Learning Model. We show how this model can be extended to capture the emergence of typological generalizations such as word order correlations.

## 4. Filtered Variational Learning Models

### 4.1. Variational Learning Models

In this section, we focus on approaches that capture intraspeaker variable linguistic behavior in terms of models of linguistic competence that accommodate and generate variation (Clark 2004; Yang 2002). Following Yang

(2002), we call models of this sort VARIATIONAL LEARNING MODELS. The key property of Variational Learning Models is their guiding assumption that learning involves “coexisting hypotheses in competition and gradual selection” (Yang 2002: 34). Consequently, Variational Learning Models avoid the architectural and empirical problems with the Filtered Learning Model presented by Kirby (1999). To illustrate the variational learning approach, we focus on the model presented by Yang (1999, 2000, 2002). Clark (2004) discusses a related Variational Learning Model within the framework of Stochastic Optimality Theory.

In the Variational Learning Model presented by Yang (1999: 431; 2002: 26–30), intraspeaker variable linguistic behavior is modeled in terms of a population of grammars. Each grammar  $G_i$  is associated with a weight  $p_i$ ,  $0 \leq p_i \leq 1$  and  $\sum p_i = 1$ . Each of these weights denote the probability with which the learner can access the associated grammar. In a learning environment  $E$ , the weight  $p_i(E, t)$  is determined by the learning function,  $E$ , and  $t$  (the time since the onset of language acquisition). Learning is modeled in terms of the changing weights of grammars in response to the sentences incrementally presented to the acquirer. Suppose that there are  $N$  grammars in the population. Write  $G_i \rightarrow s$  if a grammar  $G$  can analyze sentence  $s$ . Write  $\gamma$  for the learning rate.<sup>10</sup> Write  $p_i$  for  $p_i(E, t)$  at time  $t$ , and  $p'_i$  for  $p(E, t + 1)$  at time  $t + 1$ , where each time instance corresponds to the presentation of an input sentence. Learning takes place as in (17), the Linear Reward-Penalty scheme (Bush & Mosteller 1951, 1958). In this paper, we are only going to look at a simple two grammar case.

(17) Given an input sentence  $s$ , the learner selects a grammar  $G_i$  with probability  $p_i$ :

$$\begin{array}{ll} \text{a. if } G_i \rightarrow s \text{ then } & \begin{cases} p'_i = p_i + \gamma(1-p_i) \\ p'_j = (1-\gamma)p_j \text{ if } j \neq i \end{cases} \\ \text{b. if } G_i \not\rightarrow s \text{ then } & \begin{cases} p'_i = (1-\gamma)p_i \\ p'_j = \frac{\gamma}{N-1} + (1-\gamma)p_j \text{ if } j \neq i \end{cases} \end{array}$$

Yang (1999, 2002) shows that, in the general case, when learning stops, grammars more compatible with the learning data are better represented in the population than other, less compatible grammars. As a consequence, in a

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<sup>10</sup> In what follows, the value of  $\gamma$  is low (.01), i.e. the learner does not alter the weight of grammars too radically in response to input sentences (Yang 2002: 32).

heterogeneous learning environment where no single grammar can analyze every input sentence, the acquirer converges on a stable combination of grammars. This consequence of the Variational Learning Model is very relevant for language change: the linguistic competence of speakers during periods of language change is modeled as the combination of multiple grammars. This result is supported by much work in historical syntax, see, e.g., Pintzuk (1999), Kroch (2001), and Clark (2004).

#### 4.2. Incorporating filtering in Variational Learning Models

As Yang (2002: 54) notes, there is no bias in grammar evaluation in the Variational Learning Model: “all grammars are there to begin with, and input-grammar compatibility is the only criterion for rewarding/punishing grammars.” Consequently, while the Variational Learning Model overcomes the Filtered Learning Model’s failure to capture intraspeaker variation, it cannot account for typological generalizations such as those observed by Greenberg (1966), Hawkins (1983), and Dryer (1992). The simulations described in Section 3 suggest that languages can adapt glossogenetically to an asymmetric functional pressure through a process of non-genetic cultural evolution (i.e. language change).

To illustrate the problem, consider a situation in which the all-medial and brace construction are in competition, and the brace construction is more frequent than the all-medial construction. The Filtered Learning Model presented by Kirby predicts that the consistent, all-medial construction should win out over time. In contrast, for Yang’s Variational Learning Model, we expect relative random fluctuations in the relative frequency of the brace construction and the all-medial construction (i.e. stable variation), barring external changes such as language contact.

To capture the emergence of typological generalizations, we can extend the Variational Learning Model presented in Yang (2002) so that certain structures are preferentially selected for by the learner. We do this by adding a filtering function  $F$  to the Variational Learning Model.<sup>11</sup> Figure 3 illustrates

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<sup>11</sup> The filtering function  $F$  is given in (i):

$$F(p) = \frac{1}{1 + e^{-z'(p, \alpha)}}, \text{ where } z'(p, \alpha) = 5((10(\alpha + 1))2p - 1)$$

A reviewer asks why we did not just use the the sigmoid function in (ii) and scale  $\alpha$  appropriately. The sigmoid function in (ii) captures the observation that one form will always drive the other form out of use and that there is pressure to

the application of the filtering function  $F$  to the weight  $p_{\text{preferred}}$  associated with the preferred grammar at different levels of bias ( $\alpha$ ). The filtering function  $F$  has two key properties. First,  $F$  drives probabilities to extrema. Consequently, one form will always drive the other form out of use. Second, there is an asymmetric drive towards extrema. Correspondingly, there is pressure to adopt the preferred form over time. In this way, typological asymmetries such as word order correlations are captured.

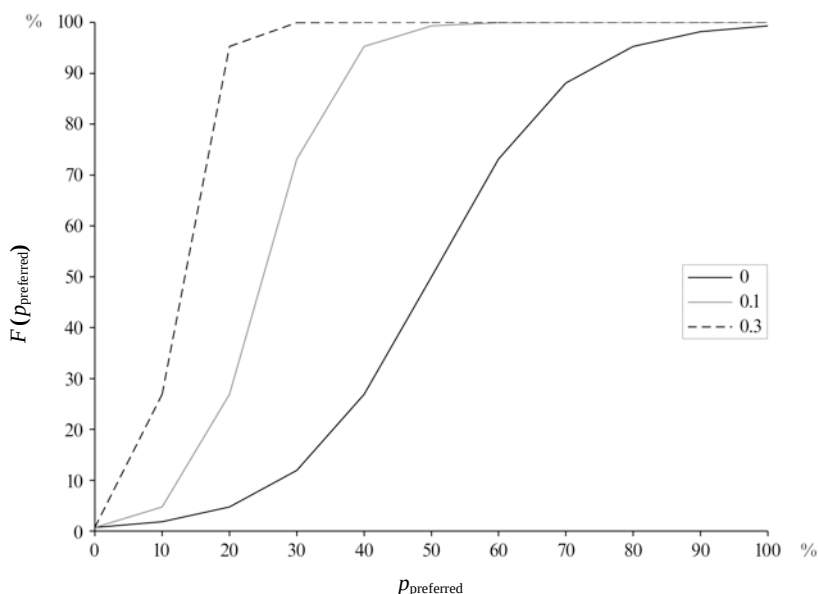


Figure 3. Filtering with different levels of bias

adopt the preferred form over time. However, the function in (ii) does not map from  $\{0,1\}$  to the range  $\{0,1\}$ . Rather, it has a range of  $-1$  to  $1$ . While less elegant than (ii), the function in (i) correctly maps to the range  $\{0,1\}$ , i.e.  $z(p) (= 5(2p-1))$  compresses the function  $F$  to  $\{0,1\}$ .

$$\text{ii. } F(p) = \frac{1}{1 + e^{-\alpha \times p}}$$

The reviewer also states that “the sigmoid function [in (i)] gives no principled way to generalize to multiple grammars”. So long as the grammar space is defined by binary parameters, biases can be attached to every parameter. As a consequence, we get an asymmetric drive towards extrema in a grammar space containing more than two grammars. This type of approach is adopted by Kirby (1999: 48–49).

In the Variational Learning Model, language change is defined in terms of the diffusion of grammars in successive generations of language learners. In order to explore the consequences of the Variational Learning Model for language change, we ran computer simulations within NetLogo, a cross-platform multi-agent programmable modeling environment for the simulation of complex systems.<sup>12</sup>

The simulations discussed in this section share the components in (18) with the Filtered Learning Model presented in Kirby (1999: 43–44):

- (18) a. Utterances: Features of sentences, e.g. OV, VO.
- b. Arena of use: An unstructured pool of utterances.
- c. Grammars: List of possible utterances, e.g. [OV].

As noted above, Variational Learning Models crucially differ from the Filtered Learning Model presented by Kirby (1999) in the assumptions they make about language users. (19) describes speakers and acquirers in our computer simulations of the Variational Learning Model.

- (19) a. Speakers: A speech community which is made up of a set of speakers each of which consists of two grammars  $G_1$  and  $G_2$ . Each grammar is associated with a weight,  $p_1$  and  $p_2$ . These grammars produce utterances for input to the arena of use.
- b. Acquirers: There are speakers whose grammars have been assigned neutral weights,  $p_1 = .5$  and  $p_2 = .5$ . They take as learning data utterances from the arena of use.

As with Kirby's Filtered Learning Model, two dynamic processes, production (described in (20)) and acquisition (described in (21)), govern the interaction of the components in (18) and (19). As noted above, the learning algorithm in Yang's Variational Learning Model is the Linear Reward-Penalty scheme given in (17), rather than the probabilistic learning algorithm assumed by Kirby's Filtered Learning Model. We make the simplifying assumption here that there are no overlapping generations. Note that the acquisition process encapsulates multiple interactions with different speakers. In the simulations described below, the speaker's linguistic knowledge is stable after acquisition.

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<sup>12</sup> <http://ccl.northwestern.edu/netlogo/>

(20) Production: Speakers probabilistically add utterances to the arena of use in line with the weights  $p_1$  and  $p_2$  associated with  $G_1$  and  $G_2$ .

(21) Acquisition:

1. interact with a speaker;
2. receive the preferred form with probability  $F(p_{\text{preferred}})$  (where  $F$  is the filtering function and  $p_{\text{preferred}}$  is the weight the speaker associates with grammar  $G_{\text{preferred}}$ ), else less preferred form;
3. apply the Linear Reward-Penalty learning algorithm in (17).

The amount by which the probability of receiving the preferred structure from a speaker is raised is dependent on  $\alpha$  (the degree of bias), as shown in Figure 3.

### 4.3. Results

Recall the guiding observations that we wish to account for:

- i. There is extraordinary cross-linguistic word order variation. For example, all of the logically possible combinations of orderings of auxiliary verbs, content verbs, and objects are attested stable grammatical states.
- ii. Certain word order patterns (e.g. VO&AuxV, OV&VAux) are more frequent than others (e.g. VO&VAux, OV&AuxV) and this reflects a preference for consistently branching structures.

The left side of Figure 4 shows the time course of change for multiple simulation runs of the filtered Variational Learning Model at a single level of  $\alpha$  ( $= 0.0015$ ). The initial random distribution of the preferred structure was varied from .25 to .75 in .05 increments. 10 simulations were performed for each initial distribution. Learners received 500 utterances as input (50 per speaker, 10 speakers total (1% of the population)). Each simulation was run for 20 iterations. The right side of Figure 4 shows that the proportion of outcomes that resulted in convergence to the preferred form (e.g. the all-medial construction) was larger than the proportion of outcomes that resulted in convergence to the dispreferred form (e.g. the brace construction). Crucially, the dispreferred form is a possible stable grammatical state; the population occasionally converged on the less preferred form. In this way, the filtered Variational Learning Model captures both typological asymmetries



and multistability (i.e. the fact that both typologically preferred and dis-preferred structures are possible stable states).<sup>13</sup>

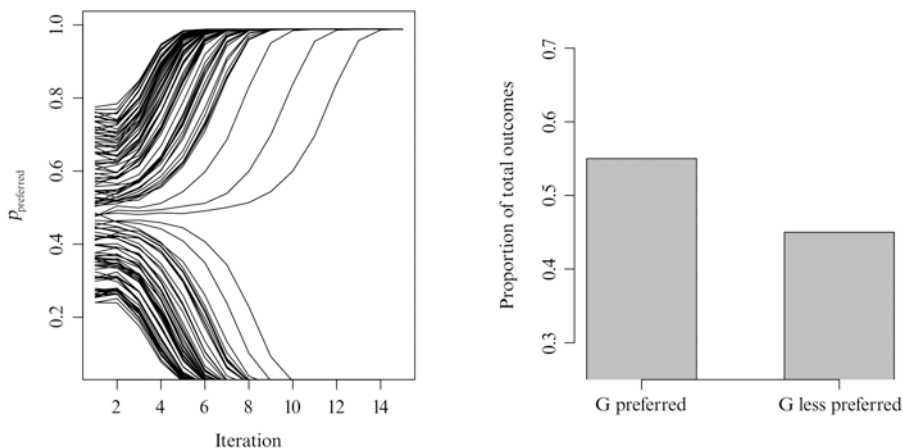


Figure 4. Filtered Variational Learning Model

Figure 5 shows that that the higher the bias ( $\alpha$ ), the more likely it is that the population will converge on the preferred grammar. The number of simulation runs in each column of the graph is 75, with the initial distribution of the preferred structure held constant at .50. The error bars in Figure 5 show the standard error of the mean.

Recall that a guiding assumption of the Filtered Learning Model presented in Kirby (1999) is that linguistic competence is ‘all-or-nothing’: speakers can be OV or VO, but not both. Figure 2 presented a simulation run illustrating the gradual spread of a preferred grammar through a speech community. At each stage of the change we saw interspeaker, but not intraspeaker, variation.

<sup>13</sup> Note that the simulations reported here assume equal probability over a range of initial conditions; this assumption is critical for the conclusions drawn here (thanks to Gerhard Jäger for drawing our attention to this point). To the extent that the initial conditions do not cover a sufficiently wide range, the number of typologically stable states will decrease. For example, as shown in Figure 4, above an initial probability of .6  $G_{\text{less-preferred}}$  is never observed as a final state. (Conversely, below .4  $G_{\text{preferred}}$  is never observed as a final state.) Similarly, if the initial distributions are not equiprobable, the relative probability of final states may not reflect the biases of learners. For example, if initial states are more likely to be drawn from below .4, the influence of the bias towards the preferred grammar will be less apparent.

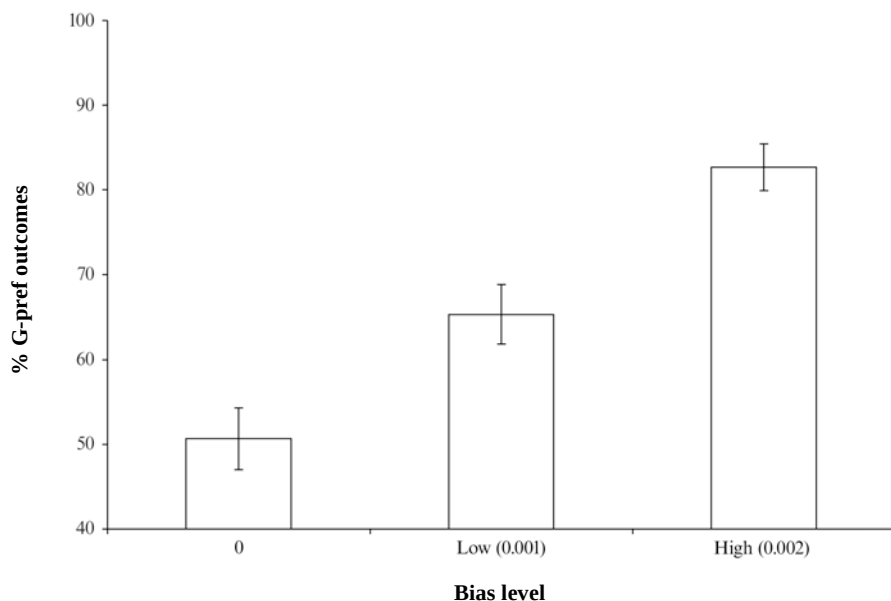


Figure 5. Filtered Variational Learning Model with varying levels of bias

In contrast, Variational Learning Models are able to capture both inter-speaker and intraspeaker variation during periods of language change. Figure 6 illustrates a single simulation run in which a preferred structure (e.g. the all-medial construction) is spreading through a community of speakers. At each stage of the change, we can see that the community is composed of individuals whose linguistic competence accommodates and generates variation (Weinreich et al. 1968; Kroch 2001; Clark 2004). At Stages 0 and 1, the majority of the speech community nearly categorically produces utterances of the less preferred form. At Stages 2 and 3, the majority of the speech community variably produces both the preferred and the less preferred form. Lastly, at Stage 4, the majority of the population nearly categorically produces the preferred form. This result demonstrates that Variational Learning Models are more empirically adequate than the Filtered Learning Model presented in Kirby (1999).

Further, the Variational Learning Model gives us a way to capture the process of GRADUAL LANGUAGE DEATH. Gradual language death is language loss due to “the gradual shift to the dominant language in a contact situation” (Wolfram 2002: 766). During periods of gradual language death, “there is often a continuum of language proficiency that correlates with different generations of speakers” (ibid.). In Figure 6, monolingual speakers of the

preferred grammar only take over the whole speech community after a period in which the majority of the speech community is bilingual. The Filtered Learning Model presented in Kirby (1999) is not able to capture this kind of change.

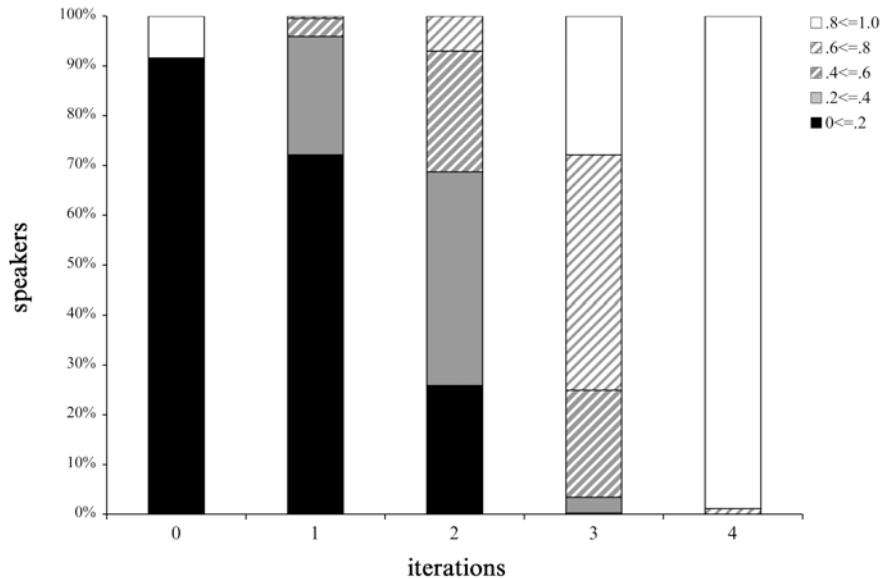


Figure 6. Grammars in the speech community during a period of change

### 5. Summary and discussion

In sum, in this paper we have presented computer simulations that suggest that Filtered Learning Models can account for the emergence of typological generalizations over time, independent of particular assumptions about the learner. Filtered Variational Learning Models avoid the architectural and empirical limitations of the Filtered Learning Model presented in Kirby (1999). We have shown that filtered Variational Learning Models are able to capture the emergence of typological generalizations through a process of non-genetic cultural evolution, thus preserving the insights of Kirby (1999).

This paper reports on ongoing work. We are currently building on what was presented here, and extending it in two directions. First, one of the major benefits of computer simulations of language acquisition and use is the ability to explore the ramifications of different side conditions. In related work

we have demonstrated that differing side conditions of the various models described here (e.g. sample rate) have important consequences (Konopka 2006). Second, we have assumed, with Kirby (1999: ch. 2), that speakers input into an unstructured arena of use, and that language learners sample from random points in the arena. In future work, we hope to incorporate models of local social structures (Milroy 2002) such as social networks into our simulations.

## Acknowledgements

For valuable comments on this work, we would like to thank Gerhard Jäger, Janet Pierrehumbert, the audience at the Blankensee Colloquium 2005, and an anonymous reviewer.

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# Evolutionary motivations for semantic universals<sup>1</sup>

*Robert van Rooij*

Most work in ‘evolutionary linguistics’ seeks to motivate the emergence of linguistic universals. Although the search for universals never played a major role in semantics, a number of such universals have been proposed concerning connectives, property- and preposition-denoting expressions and quantifiers. In this paper we suggest some evolutionary motivations for these proposed universals using game theory.

## 1. Introduction

The majority of work on the evolution of language concentrates on the evolution of syntactic and phonetic rules and/or principles, and says virtually nothing about semantics. This is, on the one hand, surprising, because languages without meanings associated with their expressions hardly make any sense. This is obvious if language is thought of as the main vehicle for transmitting information, but is equally true if one thinks that natural languages are primarily used as internal representation codes in which thinking can be carried out. On the other hand, however, the focus on syntactic principles is understandable, because syntax seems to give the evolutionary linguist more things to explain. This is due to the fact that in contrast to semantics, in syntax the search for *universals* always played an important role. The major goal of linguists working in the Chomskian tradition is to find the grammatical principles that isolate the subclass of all possible *human* languages from the class of all possible languages. This set of abstract grammatical principles then forms the universal grammar, and it is exactly the

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<sup>1</sup> This paper was presented at the Blankensee conference in Berlin and at the first Scottish-Dutch workshop on Language Evolution, both in the summer in 2005. I would like to thank the organizers of those workshops and the participants for their comments. In particular, I would like to thank Bernhard Schröder for discussions on section 3.2 of this paper, and Gerhard Jäger, Samson de Jager and an anonymous reviewer for their comments on an earlier version of this paper.



features of this universal grammar, or language faculty, that most evolutionary linguists want to explain. The search for universals as innate features of the language faculty traditionally played a much less important role in semantics. In traditional typological work, the discussion of semantic universals is normally limited to color- and kinship terms (cf. Leech 1974). This lack of interest in universals can partly be explained by the fact that denotational semantics – the standard, and certainly most productive approach towards natural language meaning – stems from logicians like Frege and Montague who held a rather anti-psychologistic view of their subject matter. More recently, however, the search for universals started to play a more important role here as well. One of the reasons for this was that around that time semanticists became more aware of the fact that although making use of sets and functions of many different types allows one to describe the meanings of expressions in a simple and compositional way, an unlimited use of this machinery is all too powerful, and would demand too much of our cognitive resources.

There are, in fact, many constraints involving the interpretation of their expressions, or at least in their use, shared by all languages of the world. For instance, it seems to be the case that in all languages more can be communicated than what is explicitly said, e.g., all languages make use of conversational *implicatures*. The exact mechanisms by which we are allowed to do so are still controversial, but it is safe to assume that the explanation for this fact of language (use) involves considerations of efficiency. Similarly, in terms of utility and efficiency one can explain why the use of negative predicates is marked, although logically there is no reason for this. Other semantico/pragmatic universals concern also linguistic *structure*. One can observe, for instance, that of all the speech acts that we can express in natural language, only three of them are normally grammaticalized, and distinguished, in mood (i.e., declarative, imperative, and interrogative). Finally there are universals that make claims about what kinds of meanings are expressed by short and simple terms in natural languages. One of them concerns *indexicals*, short expressions corresponding to the English *I*, *you*, *this*, *that*, *here*, etc., the denotations of which are essentially context-dependent. It seems that all languages have short words that express such meanings (cf. Goddard 2001), and this fact makes evolutionary sense: it is a useful feature of a language if it can refer to nearby individuals, objects, and places, and we can do so by using short expressions because their denotations can normally be inferred from the shared context between speaker and hearer. The latter kind of semantic universal is what I am most interested in

in this paper: of all possible meanings that we *can* potentially express in natural language, which ones are expressed in ‘simple’, or lexicalized, terms in all languages? The major goal of this paper, however, is not to come up with such universals. Rather, I will rely on some descriptive work in this area and concentrate on giving evolutionary motivations of these universals. While admitting that Chomskian-like ‘explanations’ by innateness might be used as a last resort, we would like to find deeper motivations for why we have simple expressions for some particular meanings but not for others in natural language. Most naturally, our explanatory motivation should make use of notions like *utility*, *learnability* and *complexity*: we typically want to express those meanings in simple terms that are (i) useful, (ii) easy to learn and remember, and (iii) easy to use.

As a formal model of evolution, I will make use of evolutionary game theory (EGT). This theory stems from biology to model *natural* selection, but has acquired an important role in economics as well to model *cultural evolution*. As a consequence – and perhaps in contrast to what is suggested by Nowak and associates – an ‘explanation’ of universal features of natural language in terms of EGT is quite neutral on the issue of whether these features have biologically evolved to become part of our ‘language faculty’ or that they are established anew by reinforcement due to language learning or use.<sup>2</sup>

## 2. Evolution and signaling games

David Lewis (1969) introduced *signaling games* to account for linguistic conventions, and these games were developed further in economics and theoretical biology. In this framework, signals have an underspecified meaning, and the actual interpretation the signals receive depends on the equilibria of sender and receiver strategy combinations of such games. Recently, these games have been looked at from an *evolutionary* point of view to study the evolution of language. I will first sketch these games here and then look at them from an evolutionary point of view.

A signalling game is a two-player game with a *sender*, *s*, and a *receiver*, *r*. This is a game of *asymmetric* information: the sender starts off knowing something that the receiver does not know. The sender knows the state  $t \in T$

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<sup>2</sup> See Jäger & van Rooij (to appear) for extensive discussion on how to most naturally interpret EGT to account for these universal features.

she is in but has no substantive payoff-relevant actions.<sup>3</sup> The receiver has a range of payoff-relevant actions to choose from but has no private information, and his prior beliefs concerning the state the sender is in are given by a probability distribution  $P$  over  $T$ ; these prior beliefs are common knowledge. The sender, knowing  $t$  and trying to influence the action of the receiver, sends to the latter a signal of a certain message  $m$  drawn from some set  $M$ . The messages don't have a pre-existing meaning. The other player receives this signal, and then takes an action  $A$  drawn from a set  $A$ . This ends the game. Notice that the game is *sequential* in nature in the sense that the players don't move simultaneously: the action of the receiver might *depend* on the signal he received from the sender. For simplicity, we take  $T$ ,  $M$  and  $A$  all to be finite. A pure *sender strategy*,  $S$ , says which message the sender will send in each state, and is modeled as a (deterministic) function from states to signals (messages):  $S \in [T \rightarrow M]$ . A pure *receiver strategy*,  $R$ , says which action the receiver will perform after he received a message, and is modeled as a (deterministic) function from signals to actions:  $R \in [M \rightarrow A]$ . Mixed strategies (probabilistic functions, which allow us to account for ambiguity) will play only a minor role in this paper and can for the most part be ignored.

As an example, consider the following signalling game with two equally likely states:  $t_1$  and  $t_2$ ; two signals that the sender can use:  $m_1$  and  $m_2$ ; and two actions that the receiver can perform:  $a_1$  and  $a_2$ . Sender and receiver each have now four (pure) strategies:

|        |       |       |       |
|--------|-------|-------|-------|
|        |       | $t_1$ | $t_2$ |
| Sender | $S_1$ | $m_1$ | $m_2$ |
|        | $S_2$ | $m_1$ | $m_1$ |
|        | $S_3$ | $m_2$ | $m_1$ |
|        | $S_4$ | $m_2$ | $m_2$ |

|          |       |       |       |
|----------|-------|-------|-------|
|          |       | $m_1$ | $m_2$ |
| Receiver | $R_1$ | $a_1$ | $a_2$ |
|          | $R_2$ | $a_2$ | $a_1$ |
|          | $R_3$ | $a_1$ | $a_1$ |
|          | $R_4$ | $a_2$ | $a_2$ |

Sender strategy  $S_1$ , for instance, says that s sends message  $m_1$  in state  $t_1$  and message  $m_2$  in state  $t_2$ , while receiver strategy  $R_3$  reacts by action  $a_1$  independent on which message he receives.

To complete the description of the game, we have to give the *payoffs*. The payoffs of the sender and the receiver are given by utility functions  $U_s$  and  $U_r$ , respectively, which state for each state-action pair its payoff, mod-

<sup>3</sup> In game theory, it is standard to say that  $t$  is the *type* of the sender.

eled by a real number.<sup>4</sup> Formally, they are functions from  $T \times A$  to the set of reals,  $\mathbf{R}$ .

Coming back to our example, we can assume, for instance, that the utilities of sender and receiver are in perfect alignment – i.e., for each agent  $i$ ,  $U_i(t_1, a_1) = 1 > 0 = U_i(t_1, a_2)$  and  $U_i(t_2, a_2) = 1 > 0 = U_i(t_2, a_1)$ :<sup>5</sup>

| $U_i(t, a)$ | $a_1$ | $a_2$ |
|-------------|-------|-------|
| $t_1$       | 1     | 0     |
| $t_2$       | 0     | 1     |

Notice that according to this utility function, action  $a_1$  is for both the preferred action in state  $t_1$ , while action  $a_2$  is for both the best in state  $t_2$ . We can model this by means of a 1-1 function  $f$  from situations to actions:  $f(t_1) = a_1$  and  $f(t_2) = a_2$ . This 1-1 function will play an important part later in the paper.

An *equilibrium* of a signalling game is described in terms of the strategies of both players. If the sender uses strategy  $S$  and the receiver strategy  $R$ , it is clear how to determine the utility of this profile for the sender,  $U_s^*(t, S, R)$ , in any state  $t$ :

$$U_s^*(t, S, R) = U_s(t, R(S(t)))$$

The receiver does not know in which situation he is, which makes things a bit more complicated for him. Because it might be that the sender using strategy  $S$  sends the same signal,  $m$ , in different states the receiver doesn't necessarily know the unique state relevant to determine his utilities. Therefore, he determines his utilities, or *expected* utilities, with respect to the *set* of states in which the speaker could have sent message  $m$ . Let us define  $S_t$  to be the *information state* (or information set) the receiver is in after the sender, using strategy  $S$ , sends her signal in state  $t$ , i.e.  $S_t = \{t' \in T : S(t') = S(t)\}$ . With respect to this set, we can determine the (expected) utility of receiver strategy  $R$  in information state  $S_t$ , which is  $R$ 's expected utility in state  $t$  when the sender uses strategy  $S$ ,  $U_r^*(t, S, R)$  (where  $P(t' | S_t)$  is the conditional probability of  $t'$  given  $S_t$ ):

<sup>4</sup> Just like Lewis (1969) we assume that sending messages is costless, which means that we are talking about *cheap talk* games here.

<sup>5</sup> This assumption allows Hurford (1989), Nowak & Krakauer (1999) and others to represent sender and receiver strategies by convenient transmission and reception matrices.

$$U_r^*(t, S, R) = \sum_{t' \in T} P(t'|S_t) \times U_r(t', R(S(t')))$$

A strategy profile  $\langle S, R \rangle$  forms a *Nash equilibrium* iff neither the sender nor the receiver can do better by unilateral deviation. That is,  $\langle S, R \rangle$  forms a Nash equilibrium iff for all  $t \in T$  the following two conditions are obeyed:

- (i)  $\neg \exists S' : U_s^*(t, S, R) < U_s^*(t, S', R)$ , and
- (ii)  $\neg \exists R' : U_r^*(t, S, R) < U_r^*(t, S, R')$ .

As can be checked easily, our game has 6 Nash equilibria:  $\{\langle S_1, R_1 \rangle, \langle S_3, R_2 \rangle, \langle S_2, R_3 \rangle, \langle S_2, R_4 \rangle, \langle S_4, R_3 \rangle, \langle S_4, R_4 \rangle\}$ . This set of equilibria depends on the receiver's probability function. If, for instance,  $P(t_1) > P(t_2)$ , then  $\langle S_2, R_4 \rangle$  and  $\langle S_4, R_4 \rangle$  are not equilibria anymore: it is always better for the receiver to perform  $a_1$ .

In signalling games it is assumed that the messages have no pre-existing meaning. However, it is possible that meanings can be associated with them due to the sending and receiving strategies chosen in equilibrium. If in equilibrium the sender sends different messages in different states and also the receiver acts differently on different messages, we can say with Lewis (1969: 147) that the equilibrium pair  $\langle S, R \rangle$  fixes meaning of expressions in the following way: for each state  $t$ , the meaning of message  $S(t)$  can either be thought of *descriptively* as  $S_t$  as the *set of states* in which message  $S(t)$  is sent:  $S_t = \{t' \in T \mid S(t') = S(t)\}$ , or *imperatively* as the *action* performed by the receiver:  $R(S(t))$ . Notice that  $S_t$  is just the same as  $S^{-1}(S(t))$  – the inverse sender strategy applied to message  $S(t)$  – a notion that will play an important role in section 3 of this paper. Following standard terminology in economics (e.g. Crawford & Sobel 1982), let us call  $\langle S, R \rangle$  a (fully) *separating* equilibrium if there is a one-to-one correspondence between states (meanings) and messages, i.e., if there exists a bijection between  $T$  and  $M$ . Notice that among the equilibria in our example, two of them are separating:  $\langle S_1, R_1 \rangle$  and  $\langle S_3, R_2 \rangle$ . According to Lewis (1969), only these separating equilibria are appropriate candidates for being a convention, and he calls them *signaling systems*.

Unfortunately, however, Lewis's characterization of conventions as separating equilibria has a number of difficulties. First, Lewis is working in the framework of traditional, or rational, game theory, where unreasonably strong assumptions have to be made concerning rationality and common knowledge in order to motivate the equilibria from an individualistic point

of view. Second, not all Nash equilibrium languages of signaling games are separating, so Lewis cannot really give a game-theoretical motivation for why linguistic conventions – as separating equilibria – are more self-purporting than equilibria that are not separating.

As it turns out, if we look at signaling games from an evolutionary, rather than a standard rationalistic point of view, both of Lewis's problems can be solved. Thinking of signaling games from an evolutionary perspective, it is natural to think of players as *users of languages*, rather than as senders or receivers. To do this, it is useful to think of signaling games first from a strategic perspective.

In the above description of a Nash equilibrium, we have taken the game to be one in *extensive form*, where the sender acts first, and has information that the receiver lacks. But we can also think of the game from a *strategic* point of view, according to which these asymmetries of action and information no longer hold. If we think of the game from a strategic point of view, we assume that the sender also has incomplete information about the actual state, and this information is represented by a probability function. We assume that the information of sender and receiver is the same, and can be represented by a *a common* (prior) probability distribution  $P$ . Sender and receiver now have to make their choices simultaneously, and these depend on their expected utilities. These expected utilities are defined in terms of the common probability distribution  $P : EU_i(S, R) = \sum_{t \in T} P(t) \times U_i(t, R(S(t)))$ , with  $i \in \{s, r\}$ . A combination of a sender and receiver strategy  $\langle S, R \rangle$  is now a Nash equilibrium if neither the sender nor the receiver can receive a higher expected utility by unilateral deviation. Thus  $\langle S, R \rangle$  is a Nash equilibrium iff the following two conditions are met:

- (i)  $\neg \exists S' : EU_s(S, R) < EU_s(S', R)$ , and
- (ii)  $\neg \exists R' : EU_r(S, R) < EU_r(S, R')$ .

Until now we have assumed implicitly that individuals have fixed roles in coordination situations: they are always either a sender or a receiver. In this sense it is an *asymmetric* game. It is natural, however, to give up this assumption and turn it into a *symmetric* game: we postulate that individuals both speak and listen, and can take both the sender- and the receiver-role. Now we might think of strategies as that which individuals will do if they play their two roles. An individual strategy can now be modeled as a pair like  $\langle S, R \rangle$  and can be thought of as a *language*. Notice that because in our example we had 4 sender strategies and 4 receiver strategies, there will now

be 16 individual strategies, or languages, that individuals can choose from. Defining  $S_i$  and  $R_i$  as the sender and receiver strategies of language  $L_i$ , we take  $U_s(t, L_i, L_j) = U(t, R_j(S_i(t)))$  and  $U_r(t, L_i, L_j) = U(t, R_i(S_j(t)))$ .

Consider now the symmetric strategic game in which each player can choose between languages. Notice that for our very simple example this is already a  $16 \times 16$  game, which is hard to represent by a payoff table on one page. To determine the payoffs of the (not represented) table, we have to know how to define the utility of playing language  $L_i$  with an other agent who plays  $L_j$ . We assume that this will be the same as the utility of playing  $L_j$  with an other agent who plays  $L_i$ . On the assumption that individuals take the sender role half of the time and the receiver role half of the time, the following utility function,  $U(L_i, L_j)$ , is natural for an agent with strategy  $L_i$  who plays with an agent using  $L_j$  (where  $EU_i(L, L')$  denotes the expected utility for  $i$  to play language  $L$  if the other participant plays  $L'$ , i.e.  $\sum_t P(t) \times U_i(t, L, L')$ ):

$$\begin{aligned} U(L_i, L_j) &= [\frac{1}{2} \times (\sum_t P(t) \times U_s(t, L_i, L_j))] + [\frac{1}{2} \times (\sum_t P(t) \times U_r(t, L_i, L_j))] \\ &= \frac{1}{2} \times (EU_s(L_i, L_j) + EU_r(L_i, L_j)). \end{aligned}$$

Suppose now that two players are playing a language game. The pair  $\langle L_i, L_j \rangle$  is a Nash equilibrium under the standard condition that no player can profit from unilateral deviation. Because the language game is symmetric (meaning that  $U_1(L_i, L_j) = U_2(L_j, L_i)$ ), we are interested in so-called symmetric equilibria, where each agent chooses the same strategy, i.e., that the language chosen is a *convention*. Then we say that  $L_i$  is a symmetric (Nash) equilibrium of the language game iff  $L_i$  is an optimal language to use if the other player uses  $L_i$  as well. Thus  $L_i$  is a symmetric Nash equilibrium iff  $U(L_i, L_i) \geq U(L_j, L_i)$  for all languages  $L_j$ . It is straightforward to show that language  $L_i$  is a (strict) symmetric equilibrium of the (symmetric) language game iff the strategy pair  $\langle S_i, R_i \rangle$  is a (strict) equilibrium of the (asymmetric) signalling game. But this means that our move from asymmetric signaling games to symmetric language games does not solve Lewis's (1969) problems mentioned above. These problems can be solved, however, if we think of language games from an evolutionary point of view and look at evolutionarily stable states rather than at Nash equilibria.

The first problem is that the rationality and knowledge assumptions required to explain the attractiveness of a Nash equilibrium in standard game theory are unreasonably strong. Evolutionary game theory (EGT) doesn't make such strong assumptions, but rather takes individuals to be very lim-

ited in their computational resources for reasoning and takes them to be fully uninformed about what strategies other individuals will choose. The notion of stability used in EGT is not reached in one try by thorough interactive reasoning of the agents, but reached after a (possibly) long trajectory of trial and error by all agents involved. Thus, using evolutionary rather than standard game theory to explain linguistic conventions as equilibria seems to solve Lewis's first problem. Lewis's second problem was to give a motivation for the selection of separating equilibria among other equilibria. As we will see soon, the separating equilibria correspond exactly with the languages that are evolutionarily stable in our simple setup.

So, under what circumstances is language  $L$  evolutionarily stable in our language game? Thinking of strategies immediately as languages, standard evolutionary game theory (see e.g. Maynard Smith 1982) gives the following answer. Suppose that all individuals of a population use language  $L$ , except for a small fraction  $\varepsilon$  of 'mutants' which have chosen language  $L'$ . Assuming random pairing of strategies, the expected utility, or *fitness*, of language  $L_i \in \{L, L'\}$ ,  $EU^\varepsilon(L_i)$ , is now:

$$EU^\varepsilon(L_i) = (1 - \varepsilon)U(L_i, L) + \varepsilon U(L_i, L')$$

In order for mutation  $L'$  to be driven out of the population, the expected utility of the mutant needs to be less than the expected utility of  $L$ , i.e.,  $EU^\varepsilon(L) > EU^\varepsilon(L')$ . But this means that either  $U(L, L)$  is higher than  $U(L', L)$ , or they are the same but  $U(L, L')$  is higher than  $U(L', L')$ . Thus, we have derived Maynard Smith & Price's (1973) definition of an evolutionarily stable strategy (ESS) for our language game.

**Definition 1** (*Evolutionarily Stable Strategy, ESS*)

*Language  $L$  is Evolutionarily Stable in the language game with respect to mutations if*

1.  $U(L, L) \geq U(L', L)$  for every  $L'$ , and
2. if  $U(L, L) = U(L', L)$ , then  $U(L, L') < U(L', L')$ .

Because the first condition means that  $\langle L, L \rangle$  is a Nash equilibrium, we see that the standard equilibrium concept in evolutionary game theory is a *refinement* of its counterpart in standard game theory. As it turns out, this refinement gives us an alternative way from Lewis (1969) to characterize the Nash equilibria that are good candidates for being a convention.



In an interesting article, Wärneryd (1993) proves the following result: for any sender-receiver game of the kind introduced above, with the same number of signals as states and actions, a language  $\langle S, R \rangle$  is evolutionarily stable if and only if it is a (fully) separating Nash equilibrium.<sup>6</sup> But this means that our evolutionary perspective on signaling games explains why only languages that give a 1-1 mapping between signals and meanings (whether imperative or descriptive) can be evolutionarily stable. Such a language is, of course, a very simple holistic language, but in this paper we will discuss to what extent signaling games from an evolutionary perspective can be used to discuss more interesting features of languages. Wärneryd's result is interesting, but not enough for our purposes: it could be that stable states can never be reached. To see whether they can be reached, we have to look at the dynamics 'behind' EGT.

Taylor & Jonker (1978) defined their *replicator dynamics* to provide a continuous dynamics for evolutionary game theory. It tells us how the distribution of strategies playing against each other changes over time. For our language game this can be done as follows: on the assumption of random pairing, the *expected utility*, or fitness, of language  $L_i$  at time  $t$ ,  $EU_t(L_i)$ , is defined as:

$$EU_t(L_i) = \sum_j P_t(L_j) \times U(L_i, L_j).$$

where  $P_t(L_j)$  denotes the proportion of individuals in the population at time  $t$  that play language  $L_j$ . The expected, or average, utility of a population of languages  $\mathbf{L}$  with probability distribution  $P_t$  is then:

$$EU_t(\mathbf{L}) = \sum_{L \in \mathbf{L}} P_t(L) \times EU_t(L).$$

The *replicator dynamics* (for our language game) is then defined as follows:

$$\frac{dP(L)}{dt} = P(L) \times (EU(L) - EU(\mathbf{L})).$$

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<sup>6</sup> This result doesn't hold anymore when there are more signals than states (and actions). We will have some combinations  $\langle S, R_i \rangle$  and  $\langle S, R_j \rangle$  which in equilibrium give rise to the same behavior, and thus payoff, although there will be an unused message  $m$  where  $R_i(m) \neq R_j(m)$ . Now these combinations are separating though not ESS. Wärneryd defines a more general (and weaker) evolutionary stability concept, that of an evolutionarily stable *set*, and shows that a strategy combination is separating if and only if it is an element of such a set.

A *dynamic equilibrium* is a fixed point of the dynamics under consideration. In a language game with 16 languages, the vector  $\langle P(L_1), \dots, P(L_{16}) \rangle$  is a fixed point of the language game dynamics if for each language  $L_i$ ,  $\frac{dP(L_i)}{dt} = 0$ . This means that the proportion of languages doesn't change anymore over time once such a probability distribution is reached. A dynamic equilibrium is said to be *asymptotically stable* if (intuitively) a solution path where a small fraction of the population starts playing a mutant strategy still converges to the stable point. Asymptotic stability is a refinement of the Nash equilibrium concept -one that is closely related to the concept of ESS. Taylor & Jonker (1978) show that every ESS is asymptotically stable. Although in general it isn't the case that all asymptotically stable strategies are ESS, on our assumption that a language game is a *cooperative* game (and thus *doubly symmetric*)<sup>7</sup> this is the case. Thus, we have the following

**Fact 1** *A language  $L$  is an ESS in our language game iff it is asymptotically stable in the replicator dynamics.*

This result is appealing, but not exactly enough for our purposes. What we would like to see is that a separating Nash equilibrium will evolve in our evolutionary language game *by necessity*. But the above result does not imply that every solution will converge toward an asymptotically stable state. In fact, as shown independently by Huttegger (to appear) and Pawlowitsch (ms), our evolutionary language games have stable (mixed) states (or better, neutrally stable states) which are not asymptotically stable. However, Huttegger (p.c.) claims that if we add a little bit of mutation to the evolutionary dynamics, all stable states will be asymptotically stable, from which we *can* infer our desired conclusion.

In general, it is very hard to find out what (asymptotic) fixed points look like. For simple  $2 \times 2$  symmetric games, however, this is relatively easy. Suppose we have a symmetric language game with only two languages involved, with the following payoff table (with  $a - d \geq 0$  and  $b - c \geq 0$ ):

| $U(L_i, L_j)$ | $L_1$  | $L_2$  |
|---------------|--------|--------|
| $L_1$         | $a, a$ | $c, d$ |
| $L_2$         | $d, c$ | $b, b$ |

<sup>7</sup> Our symmetric language games are *doubly symmetric* because for all  $L_i, L_j$ ,  $U(L_i, L_j) = U(L_j, L_i)$ .

Suppose that at time  $t$  the proportion of individuals playing language  $L_1$  is  $P_t(L_1)$ . The replicator dynamics for  $L_1$  says that this proportion will be  $P_t(L_1) \times (EU_t(L_1) - EU_t(L))$  in the next time-point  $t'$ . But this is just

$$P_{t'} = P_t(L_1) \times ((P_t(L_1) \times a) + P_t(L_2) \times c) - [[P_t(L_1) \times (P_t(L_1) \times a) + P_t(L_2) \times c]] \\ + [P_t(L_2) \times ((P_t(L_1) \times d) + P_t(L_2) \times b))].$$

Let us now abbreviate  $P_t(L_1)$  by  $p$ ,  $P_t(L_2)$  thus by  $(1-p)$ , and  $P_{t'}(L_1)$  by  $p'$ . Then the last formula abbreviates to

$$p' = p \times ((p \times a) + ((1-p) \times c)) - [[p \times (p \times a) + ((1-p) \times c)]] + [((1-p) \times ((p \times d) \\ + (1-p) \times b))].$$

This simplifies to

$$p' = p \times [(1-p) \times ((p \times a) + ((1-p) \times c))] - [(1-p) \times ((p \times d) + ((1-p) \times b))].$$

This again simplifies to

$$p' = p \times (1-p) \times [(p \times a) + ((1-p) \times c) - (p \times d) + ((1-p) \times b)],$$

which finally results in

$$p' = p \times (1-p) \times [p \times (a-d) + ((1-p) \times (c-b))].$$

The vector of probability distributions  $\langle p, (1-p) \rangle$  is a restpoint of the replicator dynamics iff  $p = p'$  (and thus  $(1-p) = (1-p')$ ). This is obviously the case iff one of the three arguments of the above formula is 0. Thus this is the case if either (i)  $p = 0$ , (ii)  $(1-p) = 0$  and thus  $p = 1$ , or (iii)  $[p \times (a-d) + ((1-p) \times (c-b))] = 0$ . The latter is the case whenever:

$$\begin{array}{lll} (p \times (a-d)) + ((1-p) \times (c-b)) & = 0 & \text{iff} \\ p \times (a-d) + (c-b) - (p \times (c-b)) & = 0 & \text{iff} \\ (c-b) + (p \times (a-d)) - (p \times (c-b)) & = 0 & \text{iff} \\ (p \times (c-b)) - (p \times (a-d)) & = c-b & \text{iff} \\ (p \times c) - (p \times b) - (p \times a) + (p \times d) & = c-b & \text{iff} \\ (p \times (- (a+b) + (c+d))) & = c-b & \text{iff} \\ \frac{(c-b)}{-(a+b)+(c+d)} & = p & \end{array}$$

Although the vector  $\langle p, (1-p) \rangle$  is a restpoint of the replicator dynamics if  $p = \frac{(c-b)}{-(a+b)+(c+d)}$ , this restpoint is *not asymptotically stable*. If  $p$  is any higher than  $\frac{(c-b)}{-(a+b)+(c+d)}$ , the proportion of individuals in the population that play language  $L_1$  will grow, and we will eventually end up with a population where all individuals play this language. If  $p$  is lower than the above fraction, we will end up eventually with a population where all individuals play  $L_2$ . Thus only situations where either everybody plays  $L_1$  or everybody plays  $L_2$  are asymptotically stable. The condition under which one of the languages will grow will play a role in section 3.2 of this paper.

### 3. Towards natural language

As we have seen above, evolutionary game theory predicts that, if there are equally many messages as there are situations, in all and only all evolutionarily stable languages there exists a 1-1-1 correspondence between situations, messages, and actions, if in each situation there is exactly one action that is optimal for both speaker and hearer. It is obvious that in this simple communication system there can be no role for property-denoting expressions and connectives: the existence of a property-denoting message, or of a disjunctive or conjunctive message, would destroy the 1-1 correspondence between (types of) situations and messages. That gives rise to the question, however, under which circumstances messages with such more complex meanings could arise.

#### 3.1. Properties

As noted in the introduction, standard formal semantics predicts that (for a simple fragment) any function from the set of worlds to the set of functions from individuals to truth values is a property that can be denoted by a property denoting expression. It is obvious, however, that in any language only a tiny fragment of all these functions are, in fact, denoted by simple words or constructions. This gives rise to the following questions: (i) can we *characterize* the properties that are denoted by simple expressions in natural language(s), and, if so, (ii) can we give a pragmatic and/or evolutionary *explanation* of this characterization?

The first idea to limit the use of all possible properties that comes to mind, is that only those properties will be expressed a lot in natural language that are *useful* for sender and receiver. Using our signaling game framework,

it is easy enough to show how usefulness can influence the existence of property-denoting terms when we either have fewer messages, or fewer actions than we have situations.<sup>8</sup>

Let us first look at the circumstances under which the signaling strategy sends the same message in equilibrium in different situations, when there are fewer signals than situations. Suppose that we have three situations and three actions, but only two messages. Because the receiver strategy is a function from messages to actions, in equilibrium only two actions can really be performed. Which of those actions that will be depends on the utilities and probabilities involved. Consider the following utility tables:

| $U(t, a)$ | $a_1$ | $a_2$ | $a_3$ |
|-----------|-------|-------|-------|
| $t_1$     | 6     | 0     | 0     |
| $t_2$     | 0     | 4     | 0     |
| $t_3$     | 0     | 1     | 2     |

| $U(t, a)$ | $a_1$ | $a_2$ | $a_3$ |
|-----------|-------|-------|-------|
| $t_1$     | 4     | 0     | 0     |
| $t_2$     | 0     | 4     | 0     |
| $t_3$     | 0     | 1     | 4     |

In both cases there exists a 1-1 correspondence between situations and messages. If there are three messages, in each situation the sender will send a different message, and the receiver will react appropriately. When there are only two messages, however, expected utility will play a role. In the left-hand table above it is more useful to distinguish  $t_1$  from  $t_2$  and  $t_3$ , than to distinguish  $t_2$  from  $t_3$ . As a consequence, in equilibrium  $t_2$  and  $t_3$  will not be distinguished from each other and in both situations the same message will be sent (the receiver will then perform action  $a_2$ ). We have implicitly assumed here that the probability of the three situations was equal. Consider now the table on the right-hand side, and suppose that the probability that  $t_1$  is the case is  $\frac{5}{9}$ ,  $P(t_1) = \frac{5}{9}$ , while  $P(t_2) = \frac{3}{9}$  and  $P(t_3) = \frac{1}{9}$ . Again, it will be more useful to distinguish  $t_1$  from  $t_2$  and  $t_3$ , than to distinguish  $t_2$  from  $t_3$ . Thus, also here we find that in equilibrium  $t_3$  will not be distinguished separately, and meshes together with  $t_2$ .<sup>9</sup>

<sup>8</sup> These abstract formulations might be used to model other ‘real-world’ phenomena as well, such as noise in the communication channel which doesn’t allow receivers to discriminate enough signals; a limitation of the objects speakers are acquainted with, perhaps due to ever changing contexts; and maybe also non-aligned preferences between sender and receiver.

<sup>9</sup> It should be noted, though, that in case  $U(t_3, a_2)$  were 0, the clustering where  $t_3$  is meshed with  $t_1$  would be equally good in both cases. Thus, the clustering that emerges can depend crucially on the specific numbers in the table. See Donaldson et al (ms) for more discussion.

Utility also plays an important role when there are fewer (relevantly different) actions than situations. Consider the well-known alarm call signaling system of vervet monkeys: what has evolved is a signaling system in which 3 different predators (snake, eagle, and leopard) are correlated with 3 different signals. This signaling system made sense because in the three (relevantly) different situations, three different actions were triggered by the fellow vervet monkeys: look on the ground, look upward, and go to the nearest tree, respectively. Suppose now, however, that for two of these different predators (say snake and leopard) the same action was called for. In that case, the vervet monkeys have no 'reason' to send a different alarm call when a snake or when a leopard approaches. In fact, we can now think of the situation as having only two (relevantly) different states, and two actions. In equilibrium, two messages are used; one for each relevantly different state, where the meaning of 'relevantly different' depends on which action should be performed in that state.<sup>10</sup> This discussion is obviously relevant for the emergence of a language with property denoting expressions: although individuals might be able to distinguish among several different situations, or objects, there is no reason to do this, as reflected in language, when it doesn't have any practical use. Thus, some messages will denote in equilibrium sets of states, not because the agents cannot distinguish between the states, but because making a distinction is not useful.<sup>11</sup> Putting this in a slightly different way: in natural language we collect different objects together to form a property, when there is a practical incentive to scramble them together.

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<sup>10</sup> I have been somewhat sloppy here. If these monkeys still have three or more signals at their disposal, an evolutionarily stable strategy is ruled out for technical reasons: there will be different strategy combinations that give rise to the same 1-1 correspondence between situations and actions, but differ only in the unused message. Because such strategy combinations give rise to the same behavior, and do equally well, none of them can be an evolutionary stable strategy, ESS. However, these strategy combinations will all be part of a set of neutrally stable strategies (Wärneryd 1993), a slightly weaker notion of stability than ESS, but one that reduces to it in case there are equally many states, messages, and actions.

<sup>11</sup> There are other reasons why, in equilibrium, a non-separating language will evolve. One reason might be that there is a difference in preferences between the agents on the actions to be performed in the situations. Another circumstance, discussed in Nowak & Krakauer (1999), is when there is noise in the signaling system: the hearer is not sure which signal is being sent. In this paper I will limit myself to fully cooperative games, however, with noiseless channels.

A common complaint of Chomskian linguists (e.g. Bickerton, Jackendoff) against explanations like the one above is that usefulness can't be the only constraint: there are many useful properties, or distinctions 'out there' that are still not really named, or distinguished, in simple natural language terms. It seems that other, additional, constraints are called for, constraints that should be explained in terms of *learnability* and/or *complexity*. It is easy to see that a property is easier to learn, remember, or use, when it has some structural features. Bickerton (1981), for instance, hypothesizes that 'simple' expressions can only denote *connected*, or *convex*, regions of cognitive space, and hypothesizes that the preference for convex properties is an innate property of our brains. Unfortunately, when we think of properties as in standard denotational semantics, it is impossible to distinguish properties that have, from properties that don't have such 'structural features'. Partly for reasons like this both linguists and philosophers (van Fraassen 1967; Stalnaker 1981) proposed alternative, and *richer*, frameworks to represent meanings and suggested that they can be used to distinguish 'natural' from 'unnatural' properties. A prominent idea found in those proposals is to assume that meaning spaces have spatial, or *geometrical* structure, and should be modeled as *vector spaces*. Properties are now just subsets of this vector, or meaning space. All properties distinguished within standard denotational semantics can be distinguished in terms of these meaning spaces as well. However, because meaning spaces have some additional structure, i.e., the *a priori* given coordinate structure,<sup>12</sup> we can now distinguish 'natural' from 'unnatural' properties. This is exactly what Gärdenfors (2000) proposes. In terms of the framework of meaning spaces, called 'conceptual spaces' by him, he called those subsets of a Euclidean meaning space 'natural' properties which form *convex regions* of this meaning space. For a set of objects to be a convex region, or set, it has to be closed in the following sense: if  $x$  and  $y$  are elements of the set, all objects 'between'  $x$  and  $y$  must also be members of this set. More formally, if we assume that  $x$  and  $y$  are points in the meaning space that can be characterized by a vector, then we say that  $z$  is a *convex combination* of  $x$  and  $y$  iff  $z = \alpha x + (1 - \alpha)y$ , with  $\alpha \in [0, 1]$ , and where  $\alpha \langle x_1, \dots, n \rangle = \langle \alpha x_1, \dots, \alpha x_n \rangle$ . Geometrically, this means that  $z$  is located somewhere in between  $x$  and  $y$ . It is easy to see that the set of all convex combinations of  $x$  and  $y$  is the straight line segment joining  $x$  and  $y$ . A set of vectors  $C$  is a *convex set* iff whenever  $x \in C$  and  $y \in C$ , it follows that also

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<sup>12</sup> In denotational semantics, this 'apriori' knowledge can be represented in terms of a restriction on accessible worlds.

any convex combination of  $x$  and  $y$  is in  $C$ , i.e.,  $\alpha x + (1-\alpha)y \in C$ . Notice that among all the subsets of the meaning space, the set of those that build convex regions of it forms only a very small minority. In consequence, Gärdenfors' proposal that many, if not all, properties denoted by simple natural language expressions form such convex regions of the conceptual space is, potentially, a very strong one. It is appealing as well, because if we know that a set is convex, the extension of the set is much easier to learn than it would be without this knowledge. For instance, in the vector space  $\mathbf{R}^2$ , the convex set with vertices  $\langle 0, 0 \rangle$ ,  $\langle 1, 0 \rangle$  and  $\langle 0, 1 \rangle$  contains infinitely many points, but to learn which elements are in this set if one assumes (as a learning bias) that sets are convex, one only has to learn the three edges. Of course, the strength of Gärdenfors' hypothesis crucially depends on what the coordinates could be. For some categories of property denoting expressions, like colors, it is quite clear what the coordinates could be, and Gärdenfors' proposal is quite successful. For other expressions, however, it is less clear whether something like the correct coordinate system can be found, and, in consequence, his proposal is much harder to test here. But even if the hypothesis that properties expressed by simple natural language expressions form convex regions of the meaning space is correct, two questions still have to be addressed: (i) in what sense are meaning spaces given *a priori*, and (ii) what makes convex regions so 'natural'? The first question is important, because whether a set of individuals forms a convex region or not crucially depends on the meaning space: in a different meaning space, the same set of objects might not form such a convex region anymore. Perhaps the *a priori* character of a coordinate system, together with its notion of 'similarity' is just due to a social convention, but perhaps it is, in fact, an innate property of our brain. As even an empiricist like Quine (1968) acknowledges, the latter seems natural for at least some cases, and allows for a standard Darwinian explanation.<sup>13</sup> As for the second question, it is shown in Jäger & van Rooij (2005) that all evolutionarily stable sender-receiver strategy combinations assign descriptive meanings to messages that

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<sup>13</sup> 'A standard of similarity is in some sense innate. [...] why does our innate subjective spacing of qualities accord so well with the functionally relevant groupings in nature as to make our inductions tend to come out right? [...] There is some encouragement in Darwin. If people's innate spacing of qualities is a gene-linked trait, then the spacing that has made for the most successful inductions will have tended to predominate through natural selection.' (Quine 1968: 123–126). Recent work of Kirby (2005) might suggest, though, that cultural evolution is involved as well.



partition the Euclidean meaning space into convex regions each with a particular point in its center such that all points in a region are closer to its own central point than to any other. Partitions like this are also known as *Voronoi Tessellations*. Thus, Gärdenfors' proposal can be given an evolutionary explanation.

Let me illustrate the evolution of Voronoi Tessellations by a very simple example. Let us take as our conceptual-, or vector-, space the line segment  $[0, 1]$ . Each element of  $[0, 1]$  is a possible situation from which the sender can send a message. We assume, as before, that the speaker strategy is a function from  $T = [0, 1]$  to  $M$ , and we assume that  $M$  consists only of two messages,  $m_i$  and  $m_j$ . This means, obviously, that we have many more situations than messages, and thus that in equilibrium there will be at least one message that will be sent in (many) different situations. Assume that  $P$  is a probability measure over  $T$  which assigns to each point an equal probability. As usual, we take the hearer strategy to be a function from  $M$  to  $A$ , but because we assume again the existence of a 1-1 correspondence between  $A$  and  $T$ , modeled by function  $f$ , we might think of the hearer's strategy also as a function from  $M$  to  $T$ . The goal of the hearer is to 'guess' what situation the speaker is in. To account for this goal, we assume that the utility function is defined in terms of a *similarity measure*, or *distance function*, between points. We will assume that the utility  $U(t_i, t_j)$  is higher if the distance between  $t_i$  and  $t_j$  is lower. Now one can easily see that according to the evolutionarily stable strategies, the *descriptive meanings* of the messages gives rise to the following partition of the state space: the descriptive meaning of the one message,  $S^{-1}(m_i)$ , will be the *first half* of the line segment, while the descriptive meaning of the other message,  $S^{-1}(m_j)$ , will be the *second half* of the line segment. Notice that  $\{S^{-1}(m_i), S^{-1}(m_j)\}$  is not just a partition of  $[0, 1]$ , but a very special one in the sense that both cells form convex regions.

Until now we have only looked at descriptive meanings, but what about *imperative meanings*? Recall from section 2 that the imperative meaning of the message sent in  $t$  according to language  $\langle S, R \rangle$  is  $R(S(t))$ . On our assumption that  $A = T$ , and thus that the receiver strategy is a function from  $M$  to  $T$ , the imperative meaning will always be a particular point in  $[0, 1]$ . Which points will that be for our messages  $m_i$  and  $m_j$ ? On our assumption that each point in  $[0, 1]$  is equally likely, it will be that  $R(m_i) = 0.25$  while  $R(m_j) = 0.75$ . Notice that  $R(m_i)$  is just right in the middle of  $S^{-1}(m_i)$ , and the same is true for  $m_j$ . In fact, we can think of the imperative meanings of the messages as just the *stereotypes* of their descriptive meanings. What's more, once we assume a particular meaning space together with an a priori given

distance function (or more generally, a particular utility function), we can *derive* the descriptive meaning of a particular message from its imperative meaning.<sup>14</sup> Indeed, as shown by Gärdenfors (2000), that is one of the beauties of Voronoi Tessellations. The distinction between descriptive and imperative meaning will play a major role in the following sections as well.

### 3.2. Compositionality and the meaning of concatenation

Remember that each (evolutionary) equilibrium  $\langle S, R \rangle$  was always a solution to a particular coordination problem. For instance, the problem of how to classify a number of objects, or individuals, with respect to color. Of course, there might be another coordination problem involving the same set of objects, but now the problem is how to classify them, e.g., with respect to shape. The combination  $\langle S', R' \rangle$  might evolve as the equilibrium for this problem, and also this equilibrium will partition the set  $T$  based on a (new, and disjoint to  $M$ ) set of messages  $M'$ . It is now not unreasonable to assume that conjunction is one of the things that might arise the moment we consider *conjoined* coordination problems, in our case of how to classify the objects with respect to color *and* shape.

The natural suggestion is that if the descriptive meaning of  $m \in M$  is  $S^{-1}(m)$  and of  $m' \in M'$  it is  $S'^{-1}(m')$ , then the combination of the two signals,  $m \cap m'$ , will denote those objects that have both the property denoted by  $m$  *and* the property denoted by  $m'$ , i.e.  $S^{-1}(m) \cap S'^{-1}(m')$ .

Notice that the above analysis of conjunctive messages already presupposes that compositional languages can and will emerge. In the previous paragraphs we have explained how the messages of a language can receive a meaning, but those messages were just unstructured wholes. Now we have assumed that messages can be structured, and have suggested how the meanings of these structured wholes can be determined from the meanings of its parts in a compositional way. Why is compositionality so important? A traditional answer is that in this way one can explain why a competent language user is capable of *interpreting* a theoretically infinite number of sentences in finite means.<sup>15</sup> A more interesting answer, perhaps, is that if a language

<sup>14</sup> Of course, the other way round is possible as well.

<sup>15</sup> It should be noted, though, that compositionality is only one way to achieve this goal, which means that the fact that we are successful in interpreting an infinite number of natural language sentences doesn't prove that natural languages are, in fact, compositional.

is (assumed to be) compositional, it helps a lot to learn the syntax and semantics of this language: ‘any information about either syntax or semantics would provide some evidence about the other, and an optimal learning mechanism would presumably exploit all available evidence’ (Partee 1984: 285).

But why and how can such a compositional language emerge in the first place? In the literature there exist two different types of approaches to address this issue. In the first approach, dubbed *synthetic* by Hurford (2000), one assumes that the set of messages is already partitioned – into nouns and verbs, for instance –, with already established meanings, and that new messages emerge from combining two messages from different types, e.g., sentences consisting of nouns and verbs. This approach is adopted by Nowak & Krakauer (1999), and it is shown by using evolutionary game theory that if compositional languages are taken to be more robust against noise than holistic languages, only compositional languages are evolutionarily stable, and thus will emerge. Notice that our above ‘story’ of conjunction is really in line with the first approach, because it was assumed that the sets of messages  $M$  and  $M'$  were disjoint, and that all these messages have already separate meanings.

Unfortunately, Nowak & Krakauer’s (1999) explanation of the emergence of compositional languages has been criticized on several points, and I believe these points are valid. Zuidema (2004), for instance, rightly criticizes the implicitly adopted assumption that we just compare a holistic versus a (pre-existing) compositional language to see which one comes out best (in terms of invasion barrier). By adopting this assumption one already assumes that compositional languages are possible, but does not explain how they can emerge ‘out of’ holistic ones. More generally, the *synthetic* point of departure has been claimed to be wrong by Wray (1998), for instance. She claims that the meaningful messages in a holistic language should not be thought of as words, but rather as whole *utterances* that describe not objects of a particular type, but rather particular kinds of *situations*. In the footsteps of the Quinean “radical translation” tradition, she proposes an *analytic* analysis to explain the emergence of compositional languages. According to this kind of analysis, compositional languages do not arise through the combination of meaningful words, but rather through the correlation between (i) features of meaningful messages, and (ii) aspects of the situations that these utterances describe.<sup>16</sup> Notice that if we assume

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<sup>16</sup> This analytic approach is not limited to the field of language evolution, but can be found also in philosophy of language (e.g. Quine and Davidson) and language acquisition (e.g. Tomassello).

that meanings are represented in an  $n$ -ary vector space, we can already take apart a situation, or objects, in various ‘aspects’. So let us see how we could work out the analytic approach.

Let us look at a very simple example. Suppose that we have a meaning space consisting of four meanings, given by the vectors  $\langle 0, 0 \rangle$ ,  $\langle 0, 1 \rangle$ ,  $\langle 1, 0 \rangle$ , and  $\langle 1, 1 \rangle$ , and suppose we have a holistic language where, for some reason, all messages are expressions of length 2 using as its first part an element of  $\{a, b\}$ , and as its second part an element of  $\{c, d\}$ . It is now clear that, among others, the following two languages, or message-meaning combinations, are possible:

| meaning                | $L_s$ | $L_c$ |
|------------------------|-------|-------|
| $\langle 0, 0 \rangle$ | $ac$  | $ac$  |
| $\langle 0, 1 \rangle$ | $ad$  | $ad$  |
| $\langle 1, 0 \rangle$ | $bc$  | $bd$  |
| $\langle 1, 1 \rangle$ | $bd$  | $bc$  |

We assume here that both  $L_s$  and  $L_c$  are holistic languages, and because both are equally expressive, for communication they are equally good. If we now adopt the analytic approach towards the emergence of compositional languages, we must be looking for correlations between features of meaningful messages and features of the meanings, such that also the parts of the messages receive an independent meaning. Such a correlation is found easily in the *simple* language  $L_s$ : ‘ $a$ ’ and ‘ $b$ ’ can be thought of as meaning  $\{\langle 0, 0 \rangle, \langle 0, 1 \rangle\} = \langle 0, i \rangle$  and  $\{\langle 1, 1 \rangle, \langle 1, 0 \rangle\} = \langle 1, i \rangle$ , respectively, while ‘ $c$ ’ always means  $\{\langle 0, 0 \rangle, \langle 1, 0 \rangle\} = \langle j, 0 \rangle$ , and ‘ $d$ ’ always means  $\{\langle 0, 1 \rangle, \langle 1, 1 \rangle\} = \langle j, 1 \rangle$ . In the more *complex*  $L_c$ , on the other hand, it seems that this kind of correlation cannot be found: although ‘ $a$ ’ and ‘ $b$ ’ can be thought of as having the same meanings as in  $L_s$ , the meanings of ‘ $c$ ’ and ‘ $d$ ’ seem to depend on whether ‘ $a$ ’ or ‘ $b$ ’ was used first. Thus, it seems that  $L_s$  is fully compositional, while  $L_c$  is not. Although this is not the case in this particular example, compositional languages are in general preferred to holistic languages because they are more robust under a learning bottleneck (e.g. Kirby 2000) and noisy communicative situations (Nowak & Krakauer 1999). If we would slightly complicate our example by going to a two- to a three-dimensional meaning space, learning a fully compositional language would only require learning the meaning of 6 symbols, while learning a (completely) holistic language involves learning 9 (or 8) independent message-meaning combinations. Thus, a language like  $L_s$  seems to be preferred to a language like  $L_c$ .

because by being compositional it behaves better under a learning-bottleneck. But, then, complicating the semantics of  $L_c$  slightly turns also this language into a compositional one: ‘ $c$ ’ and ‘ $d$ ’ can also be given *context-independent* meanings, if we think of meanings in a more abstract way. The meaning of ‘ $c$ ’ in  $L_c$ , for instance, can be thought of as being a *function* that says:  $\langle i, 0 \rangle$  if  $i$  is 0, and  $\langle i, 1 \rangle$  if  $i$  is 1 (in set terms, this would be  $[[c]]_{L_c} = \{\langle \langle 0, i \rangle, \langle 0, 0 \rangle \rangle, \langle \langle 1, i \rangle, \langle 1, 1 \rangle \rangle\}$ , and  $[[d]]_{L_c} = \{\langle \langle 0, i \rangle, \langle 0, 1 \rangle \rangle, \langle \langle 1, i \rangle, \langle 1, 0 \rangle \rangle\}$ .) Thus, if we assume that meanings can be slightly more abstract than we thought before, we can still claim that  $L_c$  is a perfectly compositional language. This is only a very simple example, but it seems that languages can usually be tricked this way. Rather than arguing for this by giving more examples, or formal proofs, I will just rely on authority:

If the syntax is sufficiently unconstrained and meanings are sufficiently rich, there seems no doubt that natural languages can be described compositionally. (Partee 1984: 281)

So, it is not compositionality all by itself that makes languages behave better under a learning-bottleneck. If a language can be analyzed compositionally only by assuming unconstrained syntactic permutation or transformation rules, or by assuming that the meaning of the signs that complex expressions are made of are difficult to learn, or to compute, it doesn’t have an evolutionary advantage. Thus, (ignoring syntax) if we take the analytic approach towards the emergence of compositional languages seriously, what computational and learning limitations select for is not so much compositionality, but rather languages that can be given a compositional analysis such that the meanings of the simple expressions are easy to compute, use, or learn.<sup>17</sup>

I know of three ways in which complexity can be made to have a selective effect in evolutionary game theory. Either one complexity directly into account when defining the utility function, or one assumes that complexity functions to be a filter for successful communication, which only indirectly influences the utility of a language, or one assumes that complexity influences learning. Let me just give a *very* simplifying but high-level analysis

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<sup>17</sup> This point is mostly ignored in much of the work on the evolution of compositional languages. Still, it doesn’t make that work nonsensical. In most of this work strong assumptions are (implicitly) made concerning the complexity of syntax and semantics, and if such assumptions are made, whether a language can be analyzed compositionally or not becomes an empirical issue again.

here. Assume that we restrict ourselves in our language game to only two languages,  $L_1$  and  $L_2$ . If these languages are used by different agents, this might give rise to the following payoff table:

| $U(L_i, L_j)$ | $L_1$ | $L_2$ |
|---------------|-------|-------|
| $L_1$         | 1, 1  | 0, 0  |
| $L_2$         | 0, 0  | 1, 1  |

What this table models is not only that users of different languages don't understand each other, but also that as far as communication is concerned, it doesn't matter which language is used. On these assumptions it follows in the replicator dynamics behind evolutionary game theory that language  $L_1$  grows if and only if the majority of the population (however tiny this majority is) uses  $L_1$ . What if we give up our two assumptions? Then we end up with the following table:

| $U(L_i, L_j)$ | $L_1$  | $L_2$  |
|---------------|--------|--------|
| $L_1$         | $a, a$ | $c, d$ |
| $L_2$         | $d, c$ | $b, b$ |

We have seen in section 2 that in the replicator dynamics behind evolutionary game theory  $L_1$  grows whenever the distribution of players that play  $L_1$  is higher than  $\frac{(c-b)}{-(a+b)+(c+d)}$ . If we now still assume that users of different languages don't understand each other, it follows that  $c = d = 0$ , and language  $L_1$  grows whenever the distribution of players that plays  $L_1$  is higher than  $b/(a+b)$ . Thus, if  $a$  is higher than  $b$ ,  $a > b$ ,  $L_1$  has a better chance to grow in a population of players than  $L_2$ . Technically it means that  $L_1$  has a greater 'basin of attraction'. But why should  $b$  be lower than  $a$ ? One reason might be that using  $L_2$  to communicate about the world is computationally more complex, and that this complexity has an immediate payoff-relevant penalty (e.g. van Rooij 2004 a,b; Jäger (ms)). Another reason might be that using  $L_2$  is more complex and thus less successful under noisy situations (e.g. Nowak & Krakauer 1999). If one assumes a noisy communication channel,  $U(t, L_i, L_j)$  should not simply be defined as  $\frac{1}{2}U(t, R_j(S_i(t))) + \frac{1}{2}U(t, R_i(S_j(t)))$ . Rather, the message sent by  $S_i$  (or  $S_j$ ) in  $t$  can be distorted, and hence  $R_j$  (or  $R_i$ ) applies to the distortion of  $S_i(t)$  (or  $S_j(t)$ ), modeled by a distortion matrix.

Another reason that  $L_1$  might be 'preferred' (in the sense of being more likely to emerge and harder to invade) to  $L_2$ , of course, is that  $L_2$  is harder to

learn. This, too, can be represented in evolutionary game theory, at least if we slightly complicate the dynamics ‘behind’ the evolutionary stable states (cf. Komarova et al. 2001). In standard replicator dynamics, the proportion of players using language  $L_i$  in a particular generation depends only on the proportion of players playing  $L_i$  in the previous generation, and the difference between (i) the average utility of playing  $L_i$  in this previous generation and (ii) the average utility of the whole population. Once we take learning into account, we don’t have to assume anymore that all ‘children’ of agents playing  $L_i$  also play  $L_i$ : if  $L_i$  is difficult to learn, not all, or even just a few ‘children’ of agents using  $L_i$  will perfectly acquire this language. This will have the result that  $L_i$  won’t have a very good chance of becoming the shared language of the members of a population.

We have seen above that not only  $L_s$  but also  $L_c$  can be analyzed such that all the expressions have context-independent meanings. However, there was still a difference between the independent meanings of ‘ $c$ ’ in the two languages: the meaning of ‘ $c$ ’ in  $L_s$  is the same as its denotation, but this is not the case for the meaning of ‘ $c$ ’ in  $L_c$ . In  $L_c$ , the denotation of ‘ $c$ ’ depends not only on its (context-independent) meaning, but also on the context in which it is used (whether it is used in the context of ‘ $a$ ’ or in the context of ‘ $b$ ’). Thus, what evolution seems to select for is not languages consisting of expressions with a context-independent meaning (for that can almost always be arranged), but rather for languages consisting of expressions whose *denotations* are *context-independent*, i.e., whose denotations equal their meanings.<sup>18</sup>

Think of the languages  $L_s$  and  $L_c$  as subject predicate sentences, or, for instance, as adjective noun combinations. Taking the first alternative, we can think of  $\{a, b\}$  as subject expressions and of  $\{c, d\}$  as predicate expressions. In each language we know already what the meanings of all potential subject predicate combinations are, but how can they be determined in terms of the meanings of their parts? In  $L_s$  things are very simple: the meaning of a sentence of the form  $x \cap y$ ,  $[[x \cap y]]_{L_1}$  is just  $[[x]]_{L_s} \cap [[y]]_{L_s}$ . In  $L_c$ , however, we have to make use of a more general mode of semantic combination: *functional application*:  $[[x \cap y]]_{L_c} = [[x]]_{L_c} \cap [[y]]_{L_c} ([[x]]_{L_c}) = [[y]]_{L_c} ([[x]]_{L_c})$ . Thus,  $L_s$  seems to be preferred to  $L_c$  because in the semantics for the latter

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<sup>18</sup> This cannot be the whole story, of course, because for reasons of efficiency, all languages make use of personal pronouns and tenses, for instance, whose denotations are very context-dependent. Still, it remains true that expressions are selected for whose denotations depend on context in very *predictable* ways.

we require this more general mode of semantic combination, while in the former we don't. This point is not different from what we noted above, although it perhaps highlights once more how costly some standard analyses in denotational semantics really are.

*An argument for descriptive meaning?* Assume, as before, that all messages are expressions of length 2 using as their first part an element of  $\{a, b\}$ , and as its second part an element of  $\{c, d\}$ . But let us now look at a completely different meaning space: the meanings are points in the line segment  $[0, 1]$ . What will be the meanings of the 4 complex expressions? It is easy to see that this will depend on the probability distribution over  $[0, 1]$ . Let us first consider the case where all points are equally likely. In that case, the following type of meaning assignment is natural:

| $L_1$ | imperative meaning | descriptive meaning |
|-------|--------------------|---------------------|
| $ac$  | 0.125              | $[0, 0.25]$         |
| $ad$  | 0.375              | $[0.25, 0.5]$       |
| $bc$  | 0.625              | $[0.5, 0.75]$       |
| $bd$  | 0.875              | $[0.75, 1]$         |

After analyzing this language, we can also give imperative and descriptive meanings to the 4 simple symbols, such that concatenation is analyzed as imperatively meaning '+', and as descriptively meaning ' $\cap$ '. There are several ways to do this<sup>19</sup>, but if we assume that  $L_1$  emerged from a simpler language that described the line segment using only  $a$  and  $b$  as messages, only the one below will result:

| $L_1$ | imperative meaning | descriptive meaning          |
|-------|--------------------|------------------------------|
| $a$   | 0                  | $[0, 0.5)$                   |
| $b$   | 0.5                | $[0.5, 1]$                   |
| $c$   | 0.125              | $[0, 0.25) \cup [0.5, 0.75)$ |
| $d$   | 0.375              | $[0.25, 0.5) \cup [0.75, 1]$ |

Now assume that the probability is not equally distributed over  $[0, 1]$ , but that it gives rise to, for instance, a normal distribution with a peak at 0.5 and

<sup>19</sup> Thanks to an anonymous reviewer on this point.



minima at 0 and 1. Then something like the following meaning assignment will follow:

| $L_2$ | imperative meaning | descriptive meaning |
|-------|--------------------|---------------------|
| $ac$  | 0.25               | $[0, 0.35)$         |
| $ad$  | 0.4                | $[0.35, 0.5)$       |
| $bc$  | 0.6                | $[0.5, 0.65)$       |
| $bd$  | 0.75               | $[0.65, 1]$         |

After analyzing this language, we can also give imperative and descriptive meanings to the 4 simple symbols, such that concatenation is analyzed as imperatively meaning ‘+’, and as descriptively meaning ‘ $\cap$ ’:<sup>20</sup>

| $L_2$ | imperative meaning         | descriptive meaning          |
|-------|----------------------------|------------------------------|
| $a$   | 0                          | $[0, 0.5)$                   |
| $b$   | 0.5                        | $[0.5, 1]$                   |
| $c$   | 0.25, if $a$ , 0.1, if $b$ | $[0, 0.35) \cup [0.5, 0.65)$ |
| $d$   | 0.4, if $a$ , 0.25, if $b$ | $[0.65, 0.5) \cup [0.65, 1]$ |

What this simple example might suggest is that meanings should preferably be thought of descriptively, rather than imperatively, and thus why concatenation should mean ‘ $\cap$ ’ rather than ‘+’. The reason is that although the two are equivalent if the meanings, or points in  $[0, 1]$ , are equally distributed, the descriptive meaning-analysis combined with a Boolean analysis of concatenation seems to allow for simpler meanings of the parts if the probability distribution is not uniform, and the language is still analyzed compositionally. Our example involved only a very simple meaning space, but it is easy to see that the same would follow if we considered more complicated meaning spaces (such as the binary meaning space  $[0, 1] \times [0, 1]$ ), or when the meaning space does not give rise to ordered coordinates in the first place. Thus, a Boolean analysis of concatenation seems to be preferred, because in general it allows for simpler, or context-independent, meanings of its parts, when the language is interpreted compositionally.

Perhaps unfortunately, however, this argument is not as convincing as it might seem. It is not really clear why the descriptive meanings of ‘ $c$ ’ and

<sup>20</sup> Assuming again that  $L_2$  emerged from a language that used only  $\{a, b\}$ .

'd' are simpler than their imperative meanings. Notice first that if the best way to represent a meaning is in terms of a set, it means that its meaning is just an *enumeration* of instances. So, what we are after is compact representations of this set. Although at first sight the descriptive meanings of symbol 'c' in  $L_1$  and  $L_2$  are equally complex – just the union of two sets of points – in  $L_1$  the descriptive meaning of 'c' can be represented more compactly than in  $L_2$  in terms of how to *compute* this descriptive meaning: it just means something like 'the first half of'. Something like this cannot be done as easily for the meaning of 'c' in  $L_2$ . So, also the descriptive meaning of 'c' in  $L_2$  is perhaps not as simple as it seemed at first, and it is not clear why this meaning is any simpler than the imperative meaning of 'c' in  $L_2$ . Thus, our suggested motivation for the primacy of descriptive meaning, combined with a Boolean analysis of concatenation, is not as convincing as it might have seemed.<sup>21</sup>

Still, once we want to determine the meaning of a complex expression from the meanings of its parts, it is generally assumed that this cannot be done by looking only at imperative meanings, or stereotypes. What should the mode of combination be if stereotypes are modeled by vectors? Addition, or convex combination will not do in most cases. Still, Gärdenfors (2000) suggests that there might be another mode of combination after all. Consider adjectives-noun combinations like *stone lion*. It is clear that analyzing the meaning of this complex phrase by means of intersection of the descriptive meanings of its parts won't give the correct result. A standard conclusion out of this in denotational semantics is to assume that the adjective and the noun should not be given meanings of the same type, and that concatenation should be interpreted as functional application. But, as argued for by Gärdenfors (2000), we don't always need such a computationally complex analysis which requires the use of higher order logic. Assume that the adjective and the noun have meanings of a different type in the sense that whereas the *imperative* meaning of the noun is a vector with two coordinates, characterized in terms of numbers on both the x-axis (say, material) and the y-axis (e.g. shape), the imperative meaning of the adjective is

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<sup>21</sup> Notice that our discussion also suggests that if we want to think of meanings from an evolutionary perspective, we shouldn't think of them in the first place as sets, as in denotational semantics. Rather, we should think of them in *richer* ways such that we can determine how difficult it is to compute or learn a given meaning. Notice that this points in the direction of *procedural semantics*, but it also suggests that the imperative, or stereotypical, meaning of a simple expression is more basic than its denotational meaning.

a vector with only one coordinate, e.g., only characterized in terms of a number on the  $x$ -axis. Gärdenfors proposes now *substitution* as a general mode of interpretation for concatenation: the (imperative) meaning of ‘stone lion’ is the same as the imperative meaning of ‘lion’, but with the value on the  $x$ -axis replaced by the imperative meaning of ‘stone’. Thus, ‘stone lion’ denotes an object with the stereotypical shape of a lion, but that is made of stone. It is clear that substitution can account for some meanings of concatenation where functional application has been used in denotational semantics. It remains unclear, however, how general it really is.

### 3.3. Truth conditional connectives

*Disjunction.* Under what circumstances can a language evolve in which we have a message that means ‘ $t_i$ ’, one that means ‘ $t_j$ ’, and yet another with the disjunctive meaning ‘ $t_i$  or  $t_j$ ’? We have seen above that if there exists a 1-1 function,  $f$ , from situations to (optimal) actions to be performed in those situations, a language can evolve with a 1-1 correspondence between messages and meanings. The existence of this function  $f$ , however, won’t be enough to ‘explain’ the emergence of messages with a disjunctive meaning. What is required, instead, is a 1-1 function from *sets* of situations to (optimal) actions. We can understand such a function in terms of a payoff table like the following:

| $U(t, a)$ | $a_1$ | $a_2$ | $a_3$ | $a_4$ | $a_5$ | $a_6$ | $a_7$ |
|-----------|-------|-------|-------|-------|-------|-------|-------|
| $t_1$     | 4     | 0     | 0     | 3     | 3     | 0     | 2.3   |
| $t_2$     | 0     | 4     | 0     | 3     | 0     | 3     | 2.3   |
| $t_3$     | 0     | 0     | 4     | 0     | 3     | 3     | 2.3   |

Notice that according to this payoff table, action  $a_1$  is the unique optimal action to be performed in situation  $t_1$ , and the same holds for combinations  $\langle t_2, a_2 \rangle$  and  $\langle t_3, a_3 \rangle$ . So far, this is the same as what we had in section 2. This table, however, contains more information. Suppose that the speaker (and/or hearer) knows that the actual situation is either  $t_1$  or  $t_2$  and that both situations are equally likely. In that case the best action to perform is neither  $a_1$  nor  $a_2$  – they only have an expected utility of 2 –, but rather  $a_4$ , because this action now has the highest expected utility, i.e., 3. Something similar holds for information ‘ $t_1$  or  $t_3$ ’ and action  $a_5$ , and for ‘ $t_2$  or  $t_3$ ’ and action  $a_6$ . Finally, in case of no information, which corresponds with information

' $t_1$  or  $t_2$  or  $t_3$ ', the unique optimal action to perform is  $a_7$ . Thus for all (non-empty) subsets of  $\{t_1, t_2, t_3\}$  there exists now a unique best action to be performed. Notice that each such subset may be thought of as an *information state*, the (complete or incomplete) information an agent might have about the actual situation. Suppose now that we change the sender-strategy from a function that assigns to each *situation* a unique message to be sent, to one that assigns to each *information state* a unique message to be sent. It is not difficult to see that now we will end up (after evolution) with a communication system in which there exists a 1-1-1 correspondence between information states (or sets of situations), messages, and actions to be performed (cf. Skyrms 2004).<sup>22</sup> Thus, there will now be messages which have disjunctive meaning. This by itself doesn't mean that we have a separate message that denotes disjunction yet, but only that we have separate messages with disjunctive meanings in addition to messages with simple meanings. However, as convincingly shown by Kirby (2000) and others, under a learning bottleneck, languages are forced to become compositional. Given that in all evolutionarily stable linguistic conventions there will be separate messages that denote the most informative information states – i.e. for all situations  $t_i$  there will always be a message with (descriptive) meaning  $\{t_i\}$  –, what will evolve under iterated learning forced by a learning bottleneck is a complex message with meaning  $\{t_i, t_j\}$  that consists of three separate signals: one signal denoting  $\{t_i\}$ , one signal denoting  $\{t_j\}$ , and one signal that turns these two meanings into the complex meaning  $\{t_i, t_j\}$ , which is done by (set theoretical) *union*. The latter signal, of course, might then be called 'disjunction'.

Although there appears to be some contrasting evidence, it is standardly assumed that humans, and only humans, have (compositional) languages containing messages which have a disjunctive meaning. In terms of the above result we might speculate why this is the case. Recall that for disjunctive messages to evolve, it was crucial that we took *information states*, or *belief states* into account. We might now speculate that it is the existence of such belief states that sets us apart from (other) animals, and why we, but not them, could make use of messages with disjunctive meanings.

In principle, once we take information states into account, we cannot only state under which circumstances disjunctive messages will evolve, but also when negative and conjunctive messages will evolve. The main difference is that we have to assume more structure of the set of information

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<sup>22</sup> This is a general result if there is a 1-1 correspondence between sets of situations and actions, and is not restricted to the particular example discussed above.

states (for disjunction we only required that this set be an *i*-join semi-lattice, for both disjunction and conjunction to evolve we most naturally have to assume that the set forms a full lattice, while for negation to evolve as well we even have to assume that the set is a free lattice, which contains many more elements, and is of a more complicated structure than a lattice, and certainly than a *i*-join semi-lattice).

Notice that there is a distinction between the circumstances under which disjunction, and under which conjunction can arise. For disjunction to arise, we needed the existence of information states: we required that the sender has, and knows that he has, incomplete information about the actual situation, and we had to ‘lift’ the speaker strategy to a function from information states to messages. In that case, messages will evolve that have a disjunctive meaning and denote *sets* of situations. For conjunction to make sense, we also had to assume that there would be messages that (descriptively) denote sets of situations. However, for that to be the case we didn’t require that a speaker strategy is a function taking sets of situations as input: it can just be a function that maps situations to messages. In that sense, conjunction is ‘simpler’ than disjunction. There is another sense in which conjunction is ‘simpler’ than (standard) disjunction, and this involves the notion of ‘convexity’. This might have evolutionary impact as well.

Above I have assumed that disjunction has its standard Boolean meaning and only investigated under which circumstances expressions can evolve with this meaning. But why should we make this assumption, and why shouldn’t we take into account the historical roots of expressions which now have the standard meaning? As for the assumption, perhaps it is justified because meanings correspond with ‘innate’ Boolean laws of thought. But, then, even if disjunction has an innate Boolean meaning, we would like to explain how this could have evolved.

It is widely assumed that in the animal kingdom what counts is not descriptive meaning, but rather imperative meaning. Moreover, this imperative meaning is just an action. In manipulative communicative situations, however, it might be good to leave the addressee a *choice* between several alternatives, and it might be that ‘or’ is used first to signal this. This would mean that if the imperative meanings of *X* and *Y* are *x* and *y* respectively, the imperative meaning of ‘*X* or *Y*’ could be described as  $\alpha x + (1 - \alpha)y$ , with  $\alpha \in \{0, 1\}$ . Now suppose that later in evolution, the natural meanings of *X* and *Y* are descriptive, rather than imperative, and that both denote *sets* of objects/situations. In that case, the meaning of ‘*X* or *Y*’ could be the following set  $\{\alpha x + (1 - \alpha)y : x \in X, y \in Y, \alpha \in \{0, 1\}\} = X \cup Y$ .

Perhaps the last step was too quick. Recall that *convexity* is both a useful constraint on properties, and one whose emergence can be explained. What this suggests is that as many properties as possible that we use should denote convex sets. Unfortunately, if we assume that disjunction is interpreted as set theoretical union, in contrast to intersection, the union of two convex sets need itself not be convex. So we would like to explain the emergence of our non-convex notion of disjunction from a closely corresponding convex notion of disjunction. If we assume that the meaning space has the form of a vector space, I think we can give such an appealing explanation. The reason is that there is another natural operation that can be associated with disjunction that results in convex sets. This operation is just that of *convex combination*. Remember that a convex combination of two vectors  $x$  and  $y$  is any vector  $\alpha x + (1-\alpha)y$  with  $\alpha \in [0, 1]$ . This operation can be lifted naturally from vectors to sets of vectors in the following way: if  $X$  and  $Y$  are sets of vectors, we define the convex combination of  $X$  and  $Y$ ,  $X + Y$ , as  $\{\alpha x + (1-\alpha)y \mid x \in X, y \in Y, \text{ and } \alpha \in [0, 1]\}$ . Unlike Boolean disjunction,  $X + Y$  might have elements that are neither in  $X$  nor in  $Y$ .<sup>23</sup> One can show that if  $X$  and  $Y$  both denote convex sets, what  $X + Y$  gives us is the smallest convex set that contains both  $X$  and  $Y$ . If convexity is a strong constraint not only on ‘simple’ properties, but on ‘complex’ ones as well, I believe that convex combination is a natural candidate for being a meaning of disjunction. It should be clear that the standard Boolean meaning of ‘ $X$  or  $Y$ ’ is just the same as  $X + Y$ , except that  $\alpha$  should be an element of  $\{0, 1\}$  instead of an element of  $[0, 1]$ . Can we assume that the former evolves out of the latter? I think we can. Notice that ‘convex combination’ is, in general, an analog, or continuous, notion. It is a relatively standard assumption in epistemology (e.g. Dretske 1981) that the crucial difference between perceptual and cognitive phenomena is that the former, but not the latter, is mostly analog in character: cognitive information is normally *discrete* or *digital*. According to Sebeok (1962), a similar difference exists between animal and human communication systems. What I would like to suggest is that the step from interpreting disjunction as convex combination to interpreting it in the standard way is just this step from analog to discrete. In our

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<sup>23</sup> See Widdows & Peters (2003) for some uses of ‘or’ that perhaps can be analyzed in terms of  $X + Y$ . Widdows (2004) argues that what is denoted by ‘mammals’ can be thought of as the convex closure of the denotations of ‘rabbits’, ‘pigs’ and ‘dolphins’, and contains many creatures that do not belong to any of the three sets.

case, it would mean that  $\alpha$  should be an element of  $\{0, 1\}$  instead of an element of  $[0, 1]$ . As we have seen above, what results is  $X \cup Y$ , i.e., set-theoretical, or Boolean, union.

*Why not more connectives?* Above we have given some motivation for why, and under which circumstances, certain truth-functional connectives could gain cognitive salience. We have motivated the existence of one unary truth-functional connective, negation, and two binary truth-functional connectives, disjunction and conjunction. However, once we assume that each (declarative) sentence is either true or false, there are *four* potential unary connectives, and as many as *sixteen* potential binary connectives. Although all these potential connectives *can* be expressed in natural language, the question is why only one unary and only two (or perhaps three) binary truth-functional connectives are expressed by means of simple words in all (or most) natural languages? That is, can we give natural reasons for why languages don't have some truth-functional connectives that are mathematically possible? Fortunately for us, this problem has already been solved convincingly by Gazdar & Pullum (1976). To illustrate their proposal, let us look at the four possible unary connectives,  $c_1$ , ...,  $c_4$ :

| $p$ | $c_1 p$ | $c_2 p$ | $c_3 p$ | $c_4 p$ |
|-----|---------|---------|---------|---------|
| 1   | 0       | 1       | 0       | 1       |
| 0   | 1       | 0       | 0       | 1       |

Connective  $c_1$  is, of course, standard negation. Why don't we see the others in natural language(s)? The second one obviously doesn't make a lot of sense:  $c_2 p$  just has the same truth-value as  $p$  itself, is thus superfluous, and its nonexistence can be explained in this way. Connectives  $c_3$  and  $c_4$  are equally strange: the truth values of  $c_3 p$  and  $c_4 p$  are independent of the truth value of  $p$ . Assuming, as a *strict compositionality* requirement, that all arguments of a connective have to be potentially relevant to determine the truth value of the whole,  $c_3$  and  $c_4$  are ruled out. Thus, only  $c_1$  is left, just as desired.

Gazdar & Pullum (1976) show that when we (i) assume the above principle of *strict compositionality*, (ii) require (binary) connectives to be *commutative*, and (iii) assume a principle of *confessionality*, which forbids natural languages from having (binary) connective which yields the value true when all its arguments are false, then all potential binary connectives are ruled out except for the following three: conjunction, standard (inclusive) disjunction, and what is known as *exclusive* disjunction. This is an appealing

result, given that strict compositionality makes perfect sense (a language that doesn't obey it is not efficient), the principle of confessionality can be explained by the difficulty of processing negation, while the constraint of commutativity is motivated by the not unnatural idea that the underlying structures of the connected sentences are linearly unordered. But it still leaves us with exclusive disjunction, a connective of which many people have argued that it is not expressed by a simple word in any language.<sup>24</sup> Fortunately, there are several ways to rule out this connective as well: based on the observation that when taking more than 2 arguments, exclusive disjunction gives rise to unnatural predictions (' $p$  or  $q$  or  $r$ ' is predicted to be true when either exactly one of the arguments is true, or all of them!), Gazdar & Pullum show that it is ruled out by generalizing their analysis by assuming that connectives take sets rather than sequences of truth values as arguments. Horn (1989), on the other hand, argues that the existence of a connective expressing exclusive disjunction is not required anyway, because this meaning already follows from standard inclusive disjunction in combination with the scalar implicature that not both disjuncts are true.

### 3.4. Relations and prepositions

Just as for connectives and properties, also for relations it is the case that many more relation-meanings *can* be expressed between a number of objects, than that we typically express by simple natural language expressions. And because there are many more subsets of  $D \times D$  (the set of denotations of relational expressions) than that there are subsets of  $D$  (the set of denotations of property expressions), or subsets of  $\{1, 0\} \times \{1, 0\}$  (the set of binary truth-functional connectives), the problem is much more serious here, but also more difficult to solve. But also here utility, learnability and complexity seem to be important factors.

First, utility obviously plays an important role, and this can again, to some extent, be explained in terms of our signaling game perspective. When we think of the meaning of a relation as a set of ordered pairs, the sender strategy,  $S$ , is now a function from ordered pairs of situations, or objects,  $\langle t, t' \rangle$ , to messages, while the receiver's strategy,  $R$ , is one from messages to actions. In analogy to what we saw for properties, it holds that if there are either fewer messages or fewer actions than there are ordered pairs of situations, at least some messages will denote, in equilibrium, sets of ordered pairs.

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<sup>24</sup> Although others have argued that Latin *aut* does express exactly this connective.



Consider the case of four individuals consisting of a man, his wife, and their two children – a boy, and a girl. Given that we have 4 individuals, we have  $4 \times 4 = 16$  ordered pairs. What is an example of a natural partition of this set of ordered pairs? A natural partition could be, for instance, the following set of relational-expressions:<sup>25</sup> {*father-of*, *mother-of*, *husband-of*, *wife-of*, *son-of*, *daughter-of*, *brother-of*, *sister-of*, *identical-to*}.<sup>26</sup> Another example of a natural partition would be the relation of individuals situated on a length-scale into {*longer than*, *equally long as*, *shorter than*}.

The latter example suggests already that Gärdenfors' (2000) analysis of 'natural properties' can sometimes be extended to 'natural relations'. To do so, Gärdenfors proposes that one dimension of the meaning space measures the length of individuals. The comparative relation *longer than* would then be represented by all ordered pairs of points in the space defined by this dimension. How can we think of this relation as denoting a convex set? Well, we might think of a new binary meaning space where the first and second dimension measure the length of the first and second object of each ordered pair, respectively.<sup>27</sup> In that case, the set of ordered pairs that constitutes the denotation of the relation 'longer than' forms a convex region of the above mentioned meaning space. In fact, this holds for all kinds of comparative relations where the generating dimension is isomorphic to the set of all (positive) real numbers (e.g. *larger*, *earlier*, and *before*). For other relation-denoting expressions we can think of a relation *R* as a function that takes an object *x* and maps it to the set of objects *y* such that *xRy*. Consider, for instance, a *locative preposition* like 'in front of'. Combined with a noun phrase, this preposition denotes the set of objects in front of the denotation of the noun phrase. In Zwarts's (1997) vector-based semantics, objects can be thought of as vectors, and so the set of objects in front of the denotation of the noun phrase is a set of vectors as well. Zwarts (1997) states three semantic universals that involve locative prepositions: (i) the set of vectors denoted by any simple locative preposition applied to an object is closed

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<sup>25</sup> Certainly if we disregard the last relation, which is there for technical reasons only.

<sup>26</sup> In an extensive discussion of semantic universals, Leech (1974) suggests that because all basic kinship relations of (all) languages can at least be expressed in traditional componential analyses like that of Lounsbury (1956) and others in terms of the relations in this set, that this set of kinship relations might be thought of as a universal categorization of kinship relations.

<sup>27</sup> Gärdenfors (2000: 92) attributes this idea to Holmqvist.

under shortening;<sup>28</sup> (ii) this set of vectors is both linearly, and (iii) radially *continuous*.<sup>29</sup> What matters here is that all three universals follow immediately if these sets of vectors (or objects) are taken to denote convex subsets of the meaning space.<sup>30</sup> Of course, proposing that also ‘natural relations’ are those sets of  $n$ -tuples that form, from a certain perspective, a convex region of a meaning space is interesting from our perspective, because we have seen that we can give a natural evolutionary motivation for the notion of convexity.

Convexity can be used to constrain meanings of simple relational expressions in other ways as well. In the footsteps of generative semanticists, Dowty (1979) proposes that many verb meanings can be decomposed in terms of meanings of stative predicates plus some abstract notions like CAUSE and BECOME whose meaning is defined in his aspect calculus. The transitive verb *open*, for instance, is decomposed in terms of the stative predicate ‘being open’ as follows:  $\lambda x \lambda y [CAUSE(x, BECOME(be - open(y))]$ . Dowty suggests that we should exclude predicates whose interpretation depends on the state of the world at more than one time (or in more than one possible world) in any way other than in the ways explicitly allowed for by the tense and modal operators of his calculus. This by itself does not sufficiently constrain the possible verb meanings, but – as explicitly suggested by Dowty (1979: section 2.4) – it would be a strong constraint if we now assumed that the stative predicates (or at least the stage-level ones) should only be predicates that denote (convex) regions of logical space.

Recall that the assumption of convexity is of considerable help in determining (learning or computing) the extension of a set, or relation. The reason is that convexity is a very strong closure condition. But relations might be closed under other conditions as well, and this will also help determine their extensions.<sup>31</sup> For instance, a relation might have the higher order

<sup>28</sup> A region of vectors  $R$  is closed under shortening iff for every  $\mathbf{v} \in R$ ,  $s\mathbf{v} \in R$ , for every  $0 < s < 1$ , where  $s$  is a scalar (Zwarts 1997).

<sup>29</sup> A vector  $\mathbf{v}$  is *linearly between*  $\mathbf{u}$  and  $\mathbf{w}$  if  $\mathbf{v}$  is a lengthening of  $\mathbf{u}$  and  $\mathbf{w}$  is a lengthening of  $\mathbf{v}$ . A vector is *radially between* two vectors  $\mathbf{u}$  and  $\mathbf{w}$  that from an acute angle if the shortest rotation of  $\mathbf{u}$  into  $\mathbf{w}$  passes over  $\mathbf{v}$ . A region of vectors is linearly/radially *continuous* iff for all  $\mathbf{u}, \mathbf{v} \in R$ , if  $\mathbf{v}$  is linearly/radially between  $\mathbf{u}$  and  $\mathbf{w}$ , when  $\mathbf{v} \in R$  (Zwarts 1997).

<sup>30</sup> The idea to relate the meaning of locative prepositions with convexity was explicitly mentioned in later work of Zwarts, and also discussed in Gärdenfors (2000).

<sup>31</sup> Notice that if we can already determine the length of an object, it is easy to determine whether one object is longer than another. To determine the meaning of

property of being *reflexive*, *symmetric*, *transitive*, etc. It is obvious that once we know that a relation has certain of these ‘natural’ higher order properties, it becomes much easier for agents to learn and remember the extension of this relation. If you know, for example, that a relation  $R$  is reflexive, you don’t need to check any object to know that this object bears the relation  $R$  to itself, and if you know that a relation  $R$  is symmetric, learning that  $x$  stands in relation  $R$  to  $y$  suffices to know that  $y$  also stands in relation  $R$  to  $x$ . From this point of view one would expect that those relations that are expressed a lot by simple natural language expressions are such that they have many of such natural ‘higher order’ properties. I don’t really know, to be honest, whether this is the case, but I *do* know that some simple relation-denoting expressions that we seem to find in all languages do have many such properties. Many relation-denoting expressions, for instance, denote *symmetric* and *irreflexive* relations, e.g. ‘opposite to’, ‘near to’, ‘be married to’, ‘similar to’.<sup>32</sup> Many other expressions denote ordering relations, relations that are *asymmetric*, *irreflexive*, and *transitive*. Examples are ‘above’ and ‘below’, ‘before’ and ‘after’, ‘in(side)’, and all comparative relations. A *linear relation* is any ordering relation that has the additional property of being *connected*: for any  $x$  and  $y$ , either  $xRy$ , or  $yRx$  (or  $x=y$ ). In a very interesting article, the economist A. Rubinstein (1996) shows that linear orderings are optimal with respect to learning, in the sense that the minimal number of observations is required in order to learn the extension of the relation. Moreover, he shows that linear orderings are optimal in terms of *expressibility*: if you know that objects in a set stand in a particular relation  $R$  to each other, the best relation that this could be is a linear relation, because then we can denote any element of the set in terms of  $R$  (plus the logical expressions) alone.

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the adjective ‘long’, however, more seems to be needed: to compute whether an object  $x$  is ‘long’ we have to compare the set of objects that are longer than  $x$  with the set of objects that are not longer than  $x$ , which involves many more computational recourses than determining the comparative relation. This might be the reason that adjectives evolved later than the comparative relation, if that is, in fact, the case.

<sup>32</sup> On the assumption, at least, that one cannot be near to, or similar to, oneself.

## 4. Conclusion

In this paper we suggested some evolutionary motivations for some proposed semantic universals using game theory. Our motivations made use of notions like *utility*, *learnability*, and *complexity*: we expect those meanings to be universally expressed in simple terms that are useful, easy to learn and remember and easy to use. We suggested some ways in which these notions have evolutionary bite, and how they might have given rise to semantic universals. In the future we would like to see how our work on linguistic universals is connected with work done in Edinburgh (e.g. Kirby 2000) by modeling biases for learning that have semantic influence.

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# **Back to nature or nurture: Using computer models in creole genesis**

*Teresa Satterfield*

“Artificial societies” are constructed to examine L2-based creole development. ‘Virtual’ slaves and slave-owners interact based on socio-historical conditions. Linguistic transmissions and developments are tracked. This study takes no position on whether a theory of ‘imperfect L2 acquisition of the superstrate language’ can be viewed as final truth with respect to creole genesis. However, a Complex Adaptive System approach sheds light on underlying dynamics of the canonical plantation which produce large-scale effects. Our findings suggest that patterns of adult L2 acquisition presumed under standard L2 creole accounts are not sufficient to explain the emergence of these creoles. Adults in our model created an incipient African-based pidgin which continued with each generation of L2 speakers. Notably, in a previous study we found that ‘prototypical creole’ structures emerged when a very small percentage of older bilingual children were present in the population.

## **1. Introduction**

Various hypotheses offer explanations for how creole languages arose in language contact settings, such as a plantation scenario. Accounts such as the Language Bioprogram Hypothesis (LBH) (Bickerton 1981, 1984, and others) argue that these types of creoles arise within one generation when children create a novel first language (L1) due to exposure to their immigrant parents’ pidgin input. A contrasting view supports the notion that these creoles are manifested by adults when their ‘unsuccessful’ or imperfect attempts at second language (L2) acquisition converge to a new code (Chaudenson 1992, 1995; Arends 1995; Mufwene 1996; among many others). A long-standing challenge has been how to reliably test theories concerning the origin and evolution of historical creole languages, owing to imperfect records and the extinction of intermediate forms.



A central premise in our research program is that language acquisition, cognition and culture can be fruitfully analyzed as complex adaptive systems (CAS), particularly given complex sets of linguistic processes mediated by internal and external factors which extend across multiple timescales (e.g., Satterfield 1999, 2001, 2005). A CAS is a dynamic network whose emergent properties are produced bottom-up by the simple interactions of many individual elements. A CAS is complex in that it is diverse and made up of multiple interconnected units; it is adaptive in that it has the capability to evolve and to learn from experience within a changing environment (Holland 1998). While the CAS approach has been applied in several linguistic studies concerning aspects of evolution leading to human language (Steels 1997, 2005; Briscoe 2002, 2000; Culicover & Nowak 2002; Kirby 1999; etc.), the present study demonstrates how the concept of CAS is also relevant for modeling natural phenomena observed in real-world language acquisition, and providing a window into linguistic development at several levels of analysis. At the local level, the existing *I*(nternal)-*language* grammar, that is, the mentally represented linguistic knowledge within the mind of each speaker, can be presented. Dynamic interactions of individuals in turn generate collective linguistic representations at the global level, where the adoption of a particular *E*(xternal)-*language*, the body of linguistic knowledge or behavioral habits as manifested in actual instances of language performance of a community, is constructed.

Because the CAS dynamics generated in the interaction of numerous socio-cultural and cognitive variables are too complex to solve analytically, we analyze these properties within computational models, which provide methods for systematically examining the concurrent formation of social patterns and psycholinguistic structures as they develop within that history. Moreover, it becomes possible to tease apart the contributions for each respective variable. In addition to generating informative empirical data that is valuable to language investigations, this particular computer model brings to light underlying assumptions concerning formal language acquisition hypotheses, thereby helping to assess the plausibility of the proposals being advanced.

The objectives framing this study are entirely practical. First, we seek to model basic demographic and socio-cultural information associated with a well-known creole language scenario. For this goal we adopt Sranan Tongo (Arends 1995; Bruyn 1995; Price & Price 1992; Van den Berg 2000; Winford 2003; among others) as our base case for calibration of the computational model. Secondly, we use this experimental environment to explore the

L2 hypothesis on the formation of plantation (Atlantic) creoles in greater detail. We utilize local-level artificial agents (computer-simulated robots) (e.g., Epstein & Axtell 1996; Ferber 1998; Fox-Keller 2002) with diverse social and linguistic repertoires to inhabit our artificial society.

In terms of demographic and socio-cultural features, children undergoing L1 acquisition are not included in the current experiment, even to the degree that they would presumably interact with and affect adult L2 acquisition. In this vein, the present work diverges from Bickerton (1981, 1984), who makes an intriguing case for a nativist perspective of language acquisition in terms of the LBH, which places primary importance on a child's inherent predisposition to acquire language (crucially, the L1).<sup>1</sup> Bickerton's view has not been widely adopted in the creole studies literature; moreover, Arends (1995: 268), among others, assumes that the presence of children was statistically inconsequential (consistently less than 20 percent) in early Surinam, and therefore L1 child-language is argued to have no enduring structural effect on emerging creoles such as Sranan.

Perhaps more thought-provoking is Bickerton's analysis spanning a range of creole languages and the structural properties that they are postulated to share. According to the guiding premise of the LBH, the similarities can best be explained by the existence of a species-particular and universal biological "blueprint" for language acquisition. Strong empirical support for the LBH with respect to the specific properties predicted to emerge in creoles due to LBH effects has not materialized. However, the general framework of the LBH in which the child's capacities in (L1) acquisition differ markedly from those of the adult in (L2) acquisition has proven to be sound and is largely compatible with the views of most language acquisition specialists. In this spirit, general cognitive and psycholinguistic specifications, adult L2 features such as bounded processing (e.g., limits in working memory, slower processing rates, reduction of inhibitory mechanisms, etc. per Johnson & Newport 1991; Strozer 1994; Miyake & Shah 1999), and limited acquisition of L2 inflectional morphology (Hudson-Kam & Newport 2005; Lardiere 2000; etc.), are integrated into the model.

Our central objective is to determine, given the conditions outlined, whether the interaction between multiple agents in this specific communicative context indirectly results in the formation of new linguistic (creole) structures. As the CAS organization is emergent rather than pre-determined,

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<sup>1</sup> For a critical overview of the Language Bioprogram Hypothesis, consult Veenstra (in press).

the agents in this scenario might end up with quite different linguistic repertoires than the historically documented human case, yet it is extremely thought-provoking to investigate whether the agents can arrive at the attested creole solution, based precisely on the conditions advanced under a specific L2 account of creole formation.

## 2. Background

We adhere to ‘standard’ definitions of a *creole* as a contact language that emerges when speakers of mutually unintelligible languages must communicate with each other (Arends, Muysken & Smith 1995; Holm 2000; Mühlhäusler 1997; Sebba 1997; Thomason 1997, 2001; and Winford 2003). In this context, the adults learn a small number of lexical items of the dominant language (the superstrate); however, they do not completely master the superstrate’s grammar. Frequently a new language comes into being, and in its earliest stages is known as a *jargon* or *pidgin*. Pidginization is a process involving (linguistic and functional-communicative) reduction, often characterized as a continuum ranging from periods of highly unstable L2 jargons to extremely stable phases with expanded pidgins. If a pidgin becomes widely accepted in the community and its use more stabilized and diffused, the resultant language is known as a *creole*. In contrast to pidgin formation, the emergence of a creole is prototypically a process of expansion that generates quite a different grammar than previously exhibited by speakers in those linguistic surroundings. While plantation-based slave communities tended to lead to creole genesis, a creole grammar was not an inevitable outcome of the plantation system. Nor is it known if these contact languages necessarily originated from pidgins.

From our assessment of the creolist literature, L2 acquisition is often depicted along a continuum, where ‘absolute’ attainment of the L2 target is regarded as largely indistinguishable from the monolingual adult native speaker. When absolute attainment is not achieved, the ‘imperfect’ L2 outcomes can produce a creole, either in the form of a new L2 grammar or an interlanguage variety of the target (e.g., Chaudenson 1995; Gross 2000; Singler 1996; Thomason & Kaufman 1988). Chaudenson’s (1995) account describes impoverished linguistic access as a catalyst for L2 creole development. With time, newly arrived adult slaves were no longer exposed to the original superstrate variety of French, but instead learned ‘approximations’ of L2 target grammars from the already acculturated adult slaves and L2

varieties of other recently imported slaves. For the most part, the L2 creole hypothesis operates along three overarching tenets:

- The formation of the prototypical creole is viewed as gradual (e.g., spanning multiple generations) and non-nativized (e.g., based on E-language formation largely in L2 speakers of the community).<sup>2,3</sup>
- There is “...broad consensus that the role of UG is to constrain the processes of restructuring by which superstrate and substrate inputs (intakes) are shaped into a viable grammar – one that conforms to universal principles of language design (Winford 2003: 347).” Thus, the idea of L2 acquisition as UG-constrained may be compatible with a view of (imperfect) input-driven learning, insofar as UG does not eclipse critical external or social elements in shaping the creole grammar.
- “...Creolists generally accept that creole formation was primarily a process of L2 acquisition in rather unusual circumstances (Winford 2003: 356).” We understand ‘L2 acquisition’ to refer to mainly adult speakers, and ‘unusual circumstances’ to be due to atypical external (social) factors such as slavery.

A general consensus in L2 acquisition references is that adults display varying degrees of L2 attainment (by monolingual native standards), even when possessing considerable motivation and resources. That linguistic capacities diminish or are not fully attainable over time is richly attested in the psycholinguistic literature, particularly with respect to specific domains of linguistic knowledge: phonemic contrasts (Werker & Desjardins 1995), sentence-processing (Weber-Fox & Neville 1996), syntax and the mastery of inflectional morphology (O’Grady 1999, Sorace 2003, Ganger et al. (in press); Johnson & Newport 1989, 1991; Lardiere 1998a, 1998b, 2000;

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<sup>2</sup> There is a trend in the gradualist-oriented theories to argue against an abrupt model in the formation of Caribbean plantation creoles, even though these were not previously assumed to have arisen from stable pidgins (Singer 1996, among others).

<sup>3</sup> Due to scope constraints, abrupt creole formation theories based on adult L2 acquisition (e.g., Relexification Hypothesis (Lefebvre 1993) and Crosslinguistic Compromise/Negotiation Hypothesis (Thomason and Kaufman 1988; Thomason 2001)) will not be covered in detail in the current study. Also, consult for example, McWhorter (1998, 2000) for counter-evidence and commentaries against the gradualist hypothesis.

Lightbown 1983; Sorace 2003; etc.), and semantic contrasts (Larsen-Freeman & Long 1991).<sup>4</sup> Generative L2 research has found considerable similarity with L1 acquisition and evidence of Universal Grammar (UG) availability (Flynn 1987; White 1989, 2003; etc.), but there is also significant counter evidence to UG availability claims (Bley-Vroman et al. 1988; Clahsen & Muysken 1989; etc.).

We hasten to add that no study reports that adults cannot learn the L2 in any capacity whatsoever. Rather, the adult norm is a high level of mastery in areas such as lexical acquisition and idiomatic expressions, with much lower proficiencies in L2 pronunciation, details in phrase structure and in certain inflectional aspects. Following a CAS approach, it is possible to argue that since L2 acquisition entails complex interactions of multiple external and internal factors varying for each individual, an endpoint in language learning abilities cannot be isolated strictly by age. Singleton & Ryan 2004 (and authors within) find that in naturalistic learning situations, those whose exposure to the L2 begins in adulthood show initial advantages in areas such as syntax and language processing over those whose exposure begins in childhood. Nevertheless, research findings generally demonstrate that younger learners come to surpass adults in L2 proficiency. Singleton and Ryan et al. (*ibid.* cit.) concede this point, but assert that the apparent lack of overall attainment for adults in the L2 has nothing to do with maturational constraints, and everything to do with language input and usage within the context of sociolinguistic and socialization norms. These authors conclude that early exposure to the L2 is important, not necessarily due to maturational factors, but simply because the earlier one starts in L2 acquisition, the more contact one gets with the target. Since the L2 access premise is central to many L2 acquisition and L2 creole formation approaches, it will be a critical experimental variable in the current work.

### 3. Case Study

#### 3.1. Descriptions

Our aim is to model the historical conditions that presumably led to the development of Sranan over time, and to determine whether the inhabitants of the artificial society form L2 grammars that reflect any or all of the

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<sup>4</sup> Also see Long (1990), O'Grady (1997, 2003) and Bhatia and Ritchie (1999) for in-depth overviews on maturational effects in language acquisition.

properties found in the stable Sranan creole. The demographic and ethnolinguistic criteria utilized in the current work derive from a compilation of historical archives and linguistic accounts on Sranan, an English-based creole (Arends 1992, 1995; Braun & Plag 2003; Bruyn 1995; Migge 1998, 2000; Seuren 2001; van den Berg 2000; and Winford 2000, 2001, 2003).<sup>5</sup>

### 3.2. Linguistic Features

Sranan exhibits several properties prototypical of other creoles: highly impoverished grammatical (inflectional) morphology, as the verb form is the same for all tenses, moods, and persons; the marking of tense, mood, aspect (TMA) and negation by means of pre-verbal particles, reduplication, and canonical SVO word order in declarative and interrogative constructions, among other features. Given the current model's parameters (to be specified in detail in the upcoming sections), a possible linguistic outcome of adult speakers' interactions over time might yield strings such as those below:

- (1) a. *Mi no ben si en.*  
 1<sup>st</sup>SING not PAST see 3<sup>rd</sup>SING  
 'I did not see him.'
- b. *Psa te unu kaba nanga skoro dan wi o meki pikin*  
 only when we finish with schooling then we FUT have baby  
*nanga den sani dati*  
 and PLU.DEF.ART thing DEM.PRON  
 'Only when we finish with school, then we'll have kids and all those things.'
- c. *Ma yu nelde yu mama dati wi e go prei bal?*  
 but 2<sup>nd</sup>SING prog 2<sup>nd</sup>SING mama that we PROG/HAB go play ball  
 'But have you told your mom that we're going to play basketball?'
- d. *Dan te mi miti en mi sa aksi en.*  
 then when 1<sup>st</sup>SING meet him 1<sup>st</sup>SING FUT ask him  
 'Then when I see him I will ask him.' Winford (2001)<sup>6</sup>

<sup>5</sup> These studies have been further informed by data in Siegel (1987).

<sup>6</sup> Interlinear glosses are my own.

Sranan's lexifier language is essentially Early Modern English.<sup>7</sup> Concerning the African substrate influences, Arends (1995) links the formation of Sranan (and perhaps additional Surinam creoles) to a variety of language families on the western African coast, providing the TMA elements and other grammatical information. A primary substrate likely formed stemming from early arrivals (1650–1720) with 50% of imported slaves speaking SVO languages of the Gbe-cluster (Fon, Ewe), and 40% from particular SVO (and options for SOV in certain contexts) in Bantu languages (e.g., Kikongo) which figured as strong secondary influences. From 1720–1740, imported slaves spoke mainly Gbe and Kwa from the Nyo-branch. The relevant African languages most likely contributing to Sranan post-1740 are Fon (along with the other closely related Gbe-languages), Kikongo, and, to a lesser degree, Twi (belonging to the Kwa group). Based on Arends' (1995) data, the substrate influences were subject to frequent fluctuations, depending on the quantity and regional origins of slaves in the population at any given time. However, the linguistic environment gradually moved to a more homogeneous state. Within a 75-to-90-year time period, the post-1740 African languages emerged as the most prominent in Surinam.

We posit that an emergent grammar resembling modern Sranan should exhibit similar characteristics, which we simplistically term '*prototypical creole effects*.' If such properties are observable as the output of the computational model, they should minimally include the following: SVO word order paralleling both English and most of the substrates, a largely European (English) lexical superstrate, and African language substrates in the form of (limited) inflectional morphology.<sup>8</sup>

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<sup>7</sup> As Mühlhäusler (1997) aptly notes, the nature and actual amount of mixing found in creoles is often underestimated (see note 12). So while the Sranan lexicon is primarily English-based, Portuguese, and a plethora of Dutch words also contribute to the vocabulary. A number of lexical items are also taken from Javanese and American Indian.

<sup>8</sup> The admittedly simplified "prototypical creole" characterization adopted for the present study is based on a general description found across the creolist literature; however, we duly acknowledge Mühlhäusler (1997: 5) among others, who note that the classification/definition of pidgins (and creoles) based on a principal lexifier language is to be avoided, since a) the highly mixed nature of pidgin (and later, creole) lexicons is often overlooked in the literature; and b) it has never been established why the lexicon should be considered as the base of the language, rather than the semantic-syntactic system.

### 3.3. Social Features and Demographics

There are many unanswered questions concerning historical aspects of (the Atlantic) creoles (a reason in itself to carry out computer modeling). As Arends is generally acknowledged as an authority on the linguistic history of Surinam creoles due to his painstaking historical research and careful tabulations, we closely adhere to his projections. Based on Arends (1995), Sranan's earliest history arose from the language contact situation with English, Dutch, and a few French settlers who set up small farms utilizing African slave labor from 1600–1650. At this time, roughly half of the European settlers were British. By 1665, there were approximately 3,000 African-born slaves laboring in the region, and close to 1500 permanently installed European planters. This early 2:1 ratio of blacks to whites may have set the stage, as Chaudenson (1995) asserts for comparable eastern Atlantic settlements, for a preliminarily successful absolute L2 acquisition model. However, a more heterogeneous European population consisting largely of the Dutch and English must be taken into account in the case of Surinam. A Dutch coup occurred in 1667 and within 13 years, a mass exodus of the majority of English colonists and, perhaps the slaves acquired by them, took place in Surinam. From 1680–1700, 18,000 slaves arrived in Surinam, with similar rates of importation each year after until the 19<sup>th</sup> century. The proportion of Africans to Europeans rose from 2:1 in 1665 to 20:1 in 1744. Plantation numbers were routinely decimated from the slaves' short life expectancy, low birth rates, and escape. Healthy males were the preferred commodity of slave-traders (Kay 1967), with males outnumbering female slaves about 2:1. Due to the small numbers of females in the population and high mortality among slaves, the slave populace was sustained through the constant influx of new African labor, rather than from natural growth.<sup>9</sup>

Arends (1995: 21) predicts that, "the model for the acquisition of the creole as a second language by the African-born slaves would be a second, not a first language version of that creole," yet this claim of L2 approximation may not fully take into account certain general social and communicative factors no doubt operative in the plantation setting. Owing to the fact that over time the Atlantic plantations functioned as increasingly hierarchical organizations, some social distance existed from early on between various

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<sup>9</sup> As Arends (1995: 264) states, very little is known about birthplace and rate of nativization of slave populations during the periods when Sranan was being formed.



groups on the plantation, causing restriction in social networks. In terms of language contact in 17<sup>th</sup> and 18<sup>th</sup> century Surinam, the possibility of gaining access to the linguistic variety with the highest prestige would have been distributed differentially, instead of across the board approximation of L2 models for all slaves. Social stratification likely occurred along the following boundaries in the plantation: European versus African; older versus younger; elite slaves, including overseers and house slaves, versus field hands; and to a lesser extent, slave elite with diminished manual tasks versus highly skilled African craftsmen (Valdman 2000). These divisions invariably produced linguistic consequences, since they created a clear incongruence in social status within the general population and more critically, within the slave community.

Only if the language of prestige is taken by all speakers to be that of the slave owners, does it become possible to obtain Arends' (1992) and Chaudenson's (1995) adult L2 acquisition scenario where directly or indirectly, the European language is the target. In this case, one must accept the presupposition that field slaves intrinsically wished to acquire the L2, and their limited access to the target would presumably lead them to preserve elements of the L1 African languages. Accordingly, low-status adult speakers such as the field workers would create a *basilect*, a creole typologically removed from the English superstrate. Adult house slaves, may attain near-native L2 proficiency, but were more likely to acquire a creole *acrolect*, which increasingly conformed to the prestigious European superstrate.<sup>10</sup> However, implications for adult L2 acquisition must also be considered in terms of the contact between speakers of the numerous African languages as well. This hypothesis becomes more critical, if as Valdman (2000) suggests, certain African languages were purposely maintained for particular slave tasks.<sup>11</sup> Insofar as a primary substrate stemmed from early slave im-

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<sup>10</sup> If such strict linguistic divisions were in place, they would have easily been instantiated by the social practice of *seasoning*; entrusting older, acculturated slaves in a plantation to acquaint recently arrived slaves with life in the colony, in a shared African language, as outlined in Valdman (2000: 156).

<sup>11</sup> Valdman (2000:155) points out from historical records that plantation owners were instructed by colonial officials to separate slaves from the same ethnic group in order to avoid uprisings. However, this policy was not strictly enforced by plantation managers, since laborers who worked in teams in the sugar mills or in the cultivation of crops needed to be able to communicate efficiently. It was likely that work forces consisted of homogeneous slave groups from the same "nation."

portation (1650–1720), approximately 90% of these arrivals spoke Gbe-cluster and Bantu family languages. Due to sheer numbers these language groups may have had considerable linguistic prestige among the slaves, further lessening the need to develop a new lingua franca when access to the superstrate was already limited.

## **4. Experimental system overview**

### **4.1. Components of the model**

The language contact scenario is constructed with a generic software platform known as SWARM. The basic properties of CAS are encoded via the following computational components: environment/space conditions, agent specifications, and local rules both for the environment and for agent actions. Each element is briefly described below.

#### *4.1.1. Environmental specifications*

Features specified for this environment include the landscape, or search space, as the spatial boundaries of the model. The space is made up of a  $50 \times 50$  square lattice that holds 2500 slots. Agents appear as color-coded squares representing five different African and European populations, shown in Figure 1:

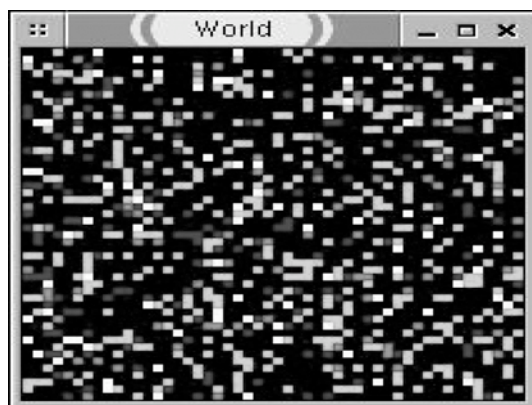


*Figure 1.* The World (Population of Agents): colors denote different groups of speakers in the population

The total population has a 2500-person carrying capacity (specified as the *population limit rule*). Parameters implemented to inform population make-up are based on historical statistics of Surinam social affiliations (Arends 1994, 1995). Demographic features and E-language aspects implemented in the model, along with their respective values, are in Figure 2:

| Parameters                |      | KEY |
|---------------------------|------|-----|
| worldSize                 | 50   | A.  |
| numberOfAgents            | 750  | B.  |
| lexiconSize               | 1000 | C.  |
| lengthOfAYear             | 12   | D.  |
| masterNewbornSurvivalRate | 0    | E.  |
| slaveNewbornSurvivalRate  | 0    | E.  |
| eWordFlowForOverseers     | 20   | F.  |
| aWordFlowForOverseers     | 20   | F.  |
| eWordFlowForLowestSlaves  | 2    | G.  |
| aWordFlowForLowestSlaves  | 20   | G.  |
| wordFlowForChildren       | NA   |     |
| numberOfSlaveIndices      | 3    | H.  |
| slaveHighLowRatio         | 0.5  | I.  |
| masterSlaveRatio          | 0.05 | J.  |
| femaleMaleRatio           | 0.5  | K.  |
| delayYears                | NA   |     |
| childRatio                | NA   |     |
| populationLimit           | 1.3  | L.  |
| eWordMorphemeRatio        | 2    | M.  |
| aWordMorphemeRatio        | 3.33 | M.  |
| eMorphemeLearningRate     | 2    | N.  |
| aMorphemeLearningRate     | 2    | N.  |
| numberOfEMorpheme         | 50   | O.  |
| numberOfAMorpheme         | 100  | O.  |
| adultFertilePercentage    | 0.58 | P.  |
| ratio1                    | 0.5  | Q.  |
| ratio2                    | 0.3  | R.  |
| ratio3                    | 0.15 | S.  |
| ratio4                    | 0.5  | T.  |
| dropAllowed               | 75   | U.  |

Figure 2. World Parameters (External and Internal Environment Profiles)

KEY: **A**=physical dimensions of landscape; **B**=current population; **C**=current maximum capacity of lexicon; **D**=number of iterations representing one year; **E**=infant mortality; **F**=maximum number of European/African forms constituting a sentence in Overseer speech; **G**=maximum number of European/African forms constituting a sentence in Infirm (lowest status) Slave speech; **H**=number of cultural tracking “tags” assigned to a Slave Agent; **I**=maximum proportion of high-status to low-status Slaves; **J**=maximum proportion of SlaveOwners to Slaves; **K**=maximum proportion of all males to females; **L**=maximum population capacity; **M**=maximum number of European/African affixes possible with a word/stem; **N**=maximum number of European/African affixes depositable in working memory during any given exchange; **O**=maximum number of European/African affixes available for acquisition; **P**=proportion of adults with reproductive capacity; **Q**=maximum proportion of Gbe-cluster speakers in the population; **R**=maximum proportion of Bantu language speakers; **S**=maximum proportion of Kwa-group speakers; **T**=maximum proportion of other African languages; **U**=maximum number of Agents that can be “eliminated” each iteration (10% of current population).

#### 4.1.2. Theoretical and cognitive variables of the model

Bounded resources are encoded as part of the agent's internal (cognitive) 'environment'. We assume the existence of a language faculty (FL) realized as the speaker's capacity for generating and analyzing linguistic structure (e.g., Chomsky 1995, 2000, 2002; Jackendoff 1997, 2002; Sharwood-Smith & Truscott 2005)<sup>12</sup>. In its most simple instantiation (per Chomsky 1995), the FL houses a performance system and a cognitive system. The cognitive system constructs linguistic derivations based on morphological, phonetic-phonological, semantic and syntactic information. The derivations are used as output that is made available for interpretation at the sound and meaning interfaces of the performance system. More specifically, the cognitive system contains a lexicon and a computational system ( $C_{HL}$ ). The lexicon generates the items utilized by the  $C_{HL}$  to build linguistic derivations according to the grammar specified. The agent's lexicon is a repository of both word-size and smaller units (stems and affixes). We further delimit the lexicon into two storage compartments: a morphology unit housing inflectional morphemes and/or preverbal tense, mood, and aspect (TMA) markers, as a grammatical function of the learner's L1; and a second unit for storage of word stems.<sup>13</sup> Figure 2 lists *eWordmorpheme* and *aWordmorpheme* ratios constituting word formation rules, in the form of the maximum number of affixes, European or African, permissible with the transmitted word stem. For this model, lexical rules apply to derive regularized African and European forms only. African languages are mapped three grammatical (TMA) preverbal markers to every one lexical item, based on general typological properties of Gbe dialects, such as Fon (Fabb 1992; DeGraff 2002, 2005). Insofar as the preverbal particles display agreement and Case information, and rigidly precede the verb with no intervention from other items, they constitute inflectional morphemes. European languages are represented as two bound inflectional affixes per stem, based on morphosyntactic properties assumed for Early Modern English.

We posit that newly encountered items are first stored in working memory (temporary memory buffer). The *Morpheme Learning Rates* regulate the maximum number of African or European morphemes deposited in

<sup>12</sup> We limit the discussion to a basic outline of the components that will be represented in the current model. The reader should consult the references listed for more detailed argumentation motivating these concepts.

<sup>13</sup> Due to space limitations, discussion of derivational morphology rules are not treated in this paper.

working memory during any given exchange, whereas the *number of Morphemes* provides the upper-bounds on the array of respective European and African morphemes available and learnable for the (input) grammars. The *lexiconSize* specifies the size of the new stems and affixes housed in storage.<sup>14</sup>

*WordFlow* exemplifies the  $C_{HL}$ , representing the maximum number of forms that can be concatenated in European (*eWordFlow*) or African (*aWordFlow*) languages, according to the surface structure of each grammar. These items are subsequently transmitted as an output string to other speakers during an exchange. An exchange is defined as the transmission and receipt of an ‘utterance (merged string of words)’. The quantity of forms that make up an utterance is parameterizable, for this model between one and twenty concatenated items (containing stems and affixes) are randomly generated to make up a given string. The recipient of the utterance acquires (i.e., analyzes as an input string whose units may, over time, be permanently stored in the lexicon) information from a given speaker. On analogy with the resources posited in the L2 language processing literature, the agents in the model store information as long-term in the lexicon only after a specific number of exchanges, when an item has been encountered repetitively. We do not distinguish the length of utterances that adult speakers direct to L2 adults. Furthermore, we make no allowances in the model for language acquisition based on overheard conversation or indirect input; all exchanges are agent-to-agent.

Crucially for this work, we follow the assumption that child language learners are substantively different from adult L2 learners. The claim that adults do not enjoy similar access to UG is founded on empirical support from numerous psycholinguistic accounts (Hyltenstam & Abrahamsson 2003; Hudson & Newport 1999; Hudson-Kam & Newport 2005; Ionin & Wexler 2002; among others), and creolist studies (DeGraff 1999, 2005; Lumsden 1999; and comments in Winford 2003).<sup>15</sup> We further assume that

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<sup>14</sup> The model presupposes a simplified language contact scenario that is activated only in this new plantation environment. Each adult brings as part of his linguistic history only one developed L1 and no other lexical influences to the language contact setting; however only the developing L2 knowledge will be tracked, as opposed to both L1 and L2.

<sup>15</sup> Where UG may include the following: larger working memory, faster processing rates, more inhibitory mechanisms, etc. (per Johnson and Newport 1991; Strozer 1994; Miyake & Shah 1999), and a higher capacity for acquisition of L2 inflectional morphology (Hudson-Kam & Newport 2005; Lardiere 2000; etc.).

the lexicon provides long-term compartmentalized storage for lexical stems and inflectional affixes/markers. For adult speakers, the model represents the lexicon as a list classifying each native (L1) lexical stem and affix/marker, plus any non-native (L2) words (minimally, stems) acquired. That is, adults not exposed to two languages from birth store new L2 lexical items separately from L1 forms (Bialystok 2001), therefore L1 and L2 items are potentially differentiated within our model's adult lexicon. In light of the characterization for adults, it remains to be seen whether the outcome of the model will be affected in decisive ways. For instance, given the presumed limited availability of UG in adults, this constraint alone may sufficiently bias the adult-only model towards producing prototypical creole effects.

Chronology is an important variable which is approximated in the model. A unit of time is represented by each cycle of the computational model that elapses. The *Length of a Year* parameter is set at 12 iterations of the computer program. While these time steps are abstractions, they play a fundamental role in identifying historical language contact benchmarks over a specified period. Figures 3 and 4 illustrate monitoring of population and age dynamics over time:

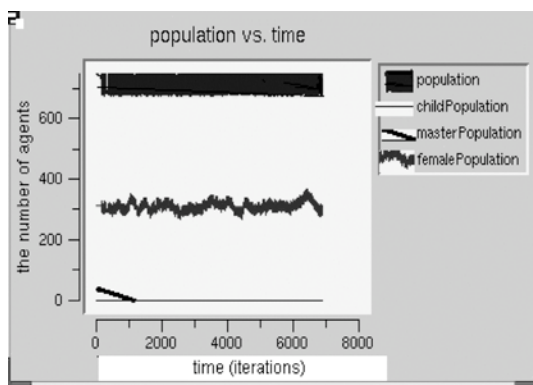


Figure 3. Population Distribution: number of agents in population over time

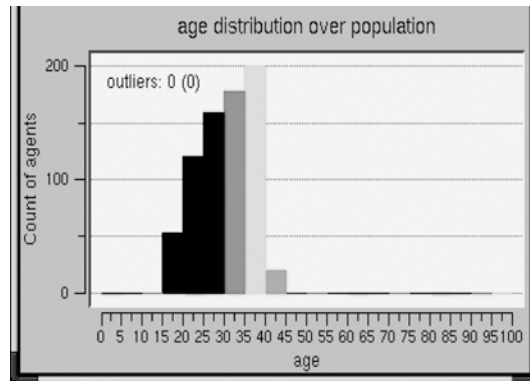


Figure 4. Age Distribution: ages of agents in the population

#### 4.1.3. Specifications of agents

Agent-level variables in SWARM endow agents with individual features and behavioral rules. Fixed attributes based on gender, racial group, age of death, etc., are designated as unique profiles for each agent. Each profile contributes to the demographic makeup of the plantation. Many states are encoded in binary (0,1) alphabets, based on specifications such as *sex*, where 0 = male, 1 = female; *dead* 0 =no, 1 = yes; *social class*, where 0= slave owner, 1 = slave; *fertile*, where 0=no, 1 = yes; and so on). Cultural identity and social status are flexible parameters which may vary over time, again in keeping with the documentation presented in conjunction with Sranan. Agents are monitored via the *slaveIndex*, which provides cultural information in the form of a ‘tag’ similar to Axelrod’s (1997) cultural chromosome. The tags link the agent in a more fine-grained manner with agents who share certain traits that we wish to track. Adult slaves are assigned 0–4 based on their occupational roles in the plantation society. Overseers (index =1) have high occupational status among slaves, whereas house-slaves have an index of 2. Field hands and infirm slaves receive progressively lower indices.

## 4.2. Local Rules for Agents

### 4.2.1. Movement rules

Movement functions of SWARM provide the basis for language contact in the plantation. The society's inhabitants are dynamic, and movement through the environment allows them to engage in potential linguistic interactions with a range of individuals, under complex conditions. Agents are always located in a specific slot, identifiable by ordered pairs of x-y coordinates. Inhabitants never overlap or occupy the same position in the world. To interact, agents seek interlocutors, and thus indirectly 'compete' to move through the search space. The movement rule incorporates frequently overlooked socio-historical factors such as power and domination within a social context (cf. Siegel 2003: 190), such that agents of higher status have priority for movement over lower status agents. The rule first applies to slave owners (Europeans), then applies sequentially to overseers, house slaves, field slaves, etc. Movement is further based on the intuition that an agent interacting with a larger number of individuals will make more impact on the language contact setting. Execution of movement rules takes place when an agent moves to the closest unoccupied cell that also has neighboring agents. A neighbor is identified as the individual in the slot situated adjacent to the agent. The number of neighbors for potential exchanges is parameterizable in the simulation. The model currently allows for a maximum of four surrounding neighbors at the north, south, east, and west slots, as based on the formal conception of von Neumann neighborhoods. Since access in the present model is restricted to linguistic information from surrounding neighbors of the appropriate status, agents are typically limited to exchanges with one acceptable neighbor per learning cycle.<sup>16</sup>

The application of this particular movement rule may be grounded on certain contentious assumptions. First, our criteria imply that physical distance is directly related to social interaction. That is, the rule sets the stage for interaction only between bordering neighbors, as we view adjacency as a logical, though simplistic, prerequisite for linguistic encounters. Neighbors in the model do not blindly interact; rather, the social factors that constrain contact will be discussed shortly. Secondly, to the extent that identification

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<sup>16</sup> The "world" landscape implemented here is actually toroidal; for instance, the right edge of the graphic wraps around to the left edge. Agents situated on the periphery thus can have the same number of neighbors as agents more centrally located.



of the “closest unoccupied cell” is a dynamical process in the model, an agent may randomly cover both large and small distances of the plantation. With each movement, the agent potentially encounters a broad diversity of new neighbors whose distribution in the space may not necessarily be predictable in a historical sense. Our hypothesis is that while initial generations of inhabitants in the model operate under ‘broader’ conditions, successive generations will not be as diverse, in ethnic, sociocultural or linguistic terms, due to the nature of later interactions. This projected development parallels real-world social contexts in various Atlantic language contact settings (Arends 1995; Chaudenson 1992; McWhorter 1998).

#### 4.2.2. *Agent social interactions*

Linguistic exchange, as constrained by cognitive resources and social factors, is the mode for acquiring and processing the linguistic input to which agents are exposed. Due to the range of possible linguistic encounters, the speaker may form novel linguistic patterns that she subsequently transmits to others; alternatively, she may transmit existing structures. The dynamic nature of the CAS is such that neither the selections made by the speakers, nor the overall results are necessarily predictable. In short, the agent’s developing I-language can drastically change based on the outcomes of her random exchanges.<sup>17</sup> The micro-grammar(s) formed may eventually be adopted globally as E-languages across the total population, but this macro-grammar only emerges via multiple, autonomous interactions of individual agents. In the present model, language learning occurs based on a loop of simple instructions executed sequentially.

In the model, adults (agents age 12 and older) are assigned their specific agent profiles and distributed into the artificial society. Contact occurs through the movement rule. Regardless of adjacency, the neighbors must be ‘socially eligible’ for linguistic exchange. The algorithm is implemented in the following manner: during the initial encounter, suppose that agent1 transmits an utterance to her adult neighbor, agent2. If during the interaction with agent2, an item in agent1’s utterance is not found in the current lexicon of this neighbor, agent2 will then add the new element to her work-

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<sup>17</sup> Importantly, agents do not intentionally move in order to have a complete view of all the other agents, nor can they consciously move to selectively develop particular languages. Likewise, no single agent completely controls all possible movement behaviors of others.

ing memory, contingent on the social and linguistic constraints outlined throughout this section. If agent2 then receives an utterance from adult agent3 and encounters the new lexical item again, agent2 will add this new item to her lexicon (long-term memory). Thus, another critical assumption in this model is that statistical frequency plays a role in the language learning process (Saffran et al. 1996; Hudson-Kam & Newport 2005).<sup>18</sup> Logically, agents may output a larger quantity of their L1 items to other speakers, if the adult L1 lexicon (permanent storage) is relatively larger than the L2 lexicon. Beyond the respective African- and European-language *Word-flow* surface structure parameters, we do not intentionally control the length of utterances generated. That is, per Ochs & Schieffelin (1994), no compensations are made in the model for L1 “foreigner talk” or simplified speech directed at L2 learning agents. The exchanges occur in parallel for various agents throughout the plantation. As the first cycle of the model is completed, the algorithm can be repeated for any number of iterations.

## 5. Experiment

### 5.1. Methodology

The objective is to now integrate the various components into the SWARM program and to observe whether linguistic structures develop. It is vital that the reader understand that while there are desired objectives, they have not been designed into the system a priori. That is, the possible outcomes cannot be predicted, due to the dynamic nature of the rules and the emergent CAS behaviors at each point in the model. Based on our assumptions underlying prototypical creole effects (e.g., SVO word order, European superstrate with African language substrates), this particular experiment may provide optimal conditions for the emergence of adult speakers with an L2 knowledge state consisting of European lexical inventories while maintaining (reduced) L1 inflectional markers resembling the African-language paradigms.

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<sup>18</sup> The number of encounters necessary for addition to the lexicon is a parameterizable value. In the current simulation two instances of exposures were employed, due to the small number of neighbors involved.

## 6. Results

The general outcome of the experiment is averaged from numerous trials, each spanning 7000 iterations (approximately 580 ‘years’).

### 6.1. Testing the ‘Imperfect L2 Acquisition’ hypothesis

Contrary to the ‘imperfect L2 acquisition’ hypothesis, our findings indicate that neither a basilect nor acrolect variety of a creole materializes such that all creole effects are present over time. However, as will be discussed below, a new L2 form spontaneously surfaces as the E-language in the plantation society.

Figures 5 and 6 represent the micro-level view, the I-language of an average adult inhabitant in the population. These are ‘snapshots’ of the average lexicon (the average quantity of words and affixes) at any given point in the simulation, where the average speaker in this setting is statistically an adult male field slave. Figure 5 illustrates the average composite of European- and African-language words (and lexical stems) across time.<sup>19</sup> Lines that are jagged and thick in appearance indicate a noisy yet stable pattern of activity within a particular language group. After the initial 1–2 generations of individuals, the average adult speaker possesses a small amount of L2 lexical knowledge. This state is represented nearly exclusively by African languages, with little influence from European elements, where the resulting sequence is: Fon, Kikongo, Twi, and other African words, respectively. The category of ‘*other African-words*’ are representative of minority African languages. This class persists in the average speaker’s L2 lexicon with typically fewer than 100 words. The average number of European words steadily declines from approximately 50 words/stems in an average lifetime (e.g., after 42 years or  $t_0$  to  $t_{500}$  iterations).

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<sup>19</sup> Due to space considerations, extensive data details in *Wordflow* are not presented in this paper. Nevertheless, the strings produced by the agents were consistently of SVO word order.

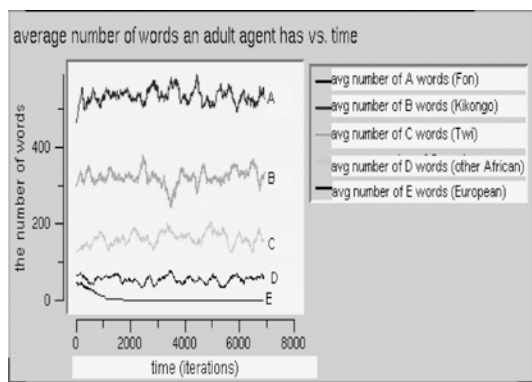


Figure 5. L2 Lexical Inventory of Average Adult: represents I-language L2 lexicon over time

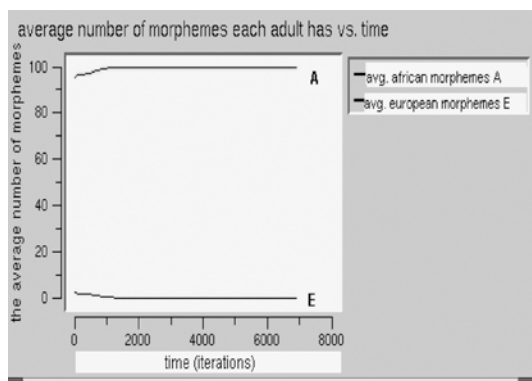


Figure 6. Inventory of Inflectional Morphology of Average L2 Adult: represents I-language grammatical morphemes over time

Figure 6 highlights the average adult's store of inflectional (grammatical) morphemes and TMA markers. The topmost line signals the L2 inventory of African-languages markers/particles, shown initially to be slightly less than 100 items across all African languages. There is evidence that the adult has access to inflectional morphemes in that after the first 500–1000 cycles, the knowledge of African-based markers reaches its maximum, signaling the adult acquisition of L2 of morphological affixes. This value then remains constant at 100 markers. In the same time span, the number of European inflectional affixes in the I-language decreases to zero. The thin, straight lines indicate sparse learning activity.

In preliminary conclusion, the average adult possesses knowledge of a variety of African-language lexical items which can be merged with the African-languages inflectional markers of the speaker’s lexicon to produce words and utterances. The resulting structures may correspond to a type of African-internal pidgin/creole, especially given the quantity of lexical items available in an average adult’s grammar. However, even in this respect there does not appear to be a significant language shift either in terms of lexical or inflectional aspects of the lexicon from one primary African language to another. We will return to discussion of these points shortly.

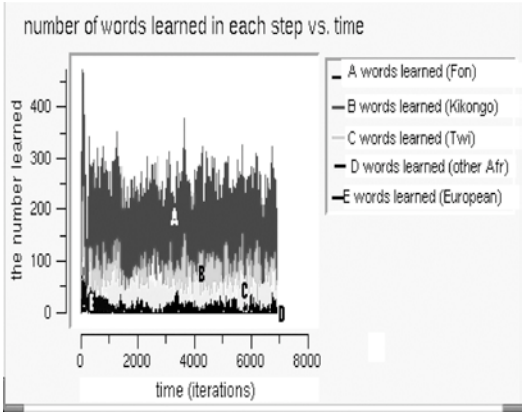


*Figure 7.* L2 Lexical Acquisition of Population: represents E-language L2 lexicon over time

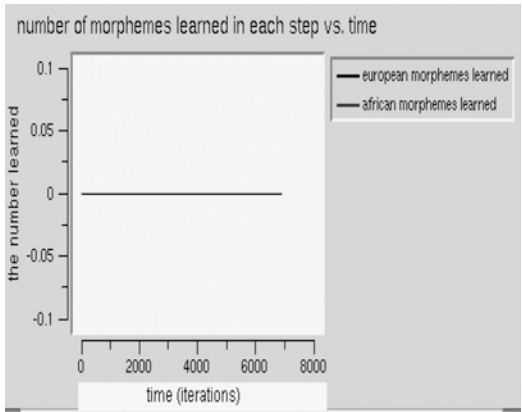


*Figure 8.* Acquisition of Inflectional Morphology for Population: represents grammatical morphemes acquired over time as E-language

Figures 7 and 8 represent E-language within the adult L2 context, signaling structures acquired across the general population. These graphs reflect the input to which L2 adults in the environment would be potentially exposed at any given time. Figure 7 shows the quantities of lexical items acquired across the total population. Although small amounts of items are acquired on a population-wide basis, the graph is extremely rugged, demonstrating the dynamic nature of the learning taking place. In the initial time steps, Fon is acquired at the highest rate across the population. This global rate is reported at 250–350 words learned per cycle in this experiment, as compared with the average individual (Figure 5) with 400–500 lexical items in Fon.<sup>20</sup> The emergent L2 lexicon is smaller, yet all languages in the contact setting initially contribute, including Kikongo, Twi, European words, and words of minority African-languages, respectively. After 500 time steps, Fon and Kikongo words/stems are learned at nearly equivalent rates by the general population. At  $t_{1000}$ , the frequency of Fon words acquired shows minimal gains, then drops substantially between  $t_{1500}$  and  $t_{3000}$  while Kikongo remains stable at 300 words/stems across all speakers. Fon eventually regains its dominant presence (likely owing to the periodical influx of slaves), maintaining approximately 350 words learned in the plantation population for the remainder of the simulation. European words/stems emerge early within the population, but have disappeared as early as cycle 1500. Words in Twi and the class of “other African-languages” are for the most part stable during the simulation posting 100 words and 50 words learned in the population, respectively, at each time step.

Figure 8 charts the population-wide acquisition of English and African language inflectional morphology over time. In reality there are two thin and rigid lines which are superimposed, however they visually appear in the graph as a single line given their equivalent values designating absence of activity. This graph indicates what may be considered predictable in the L2 acquisition of inflectional morphology for pidgin grammars: there is no emergence of new African or European inflectional items into the E-language of the plantation. We elaborate upon these outcomes in the following section.

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<sup>20</sup> We juxtapose this situation with an average of 1250 items per time step that emerged in a related creole genesis model in which the L2 adults interact with a limited number of children as well (Satterfield 2005a).

## 7. Discussion

Comparing I-language to E-language results, the average individual L2 lexicon is relatively larger and more 'complex' (i.e., containing more lexical items and inflections) than that of the general population. For several reasons the I-language falls considerably short of being an emergent creole: a) the number of L2 words stored as I-language on average is scant; b) no pronounced restructuring in the general pattern of lexical storage arises; and c) feature expansion does not occur. As noted above, we preliminarily interpret the resulting I-language structures as an African-based pidgin, based on the reduced quantity of L2 lexical items present. However, the availability of L1 inflection may qualify more as a type of stable expanded pidgin or 'foreigner talk' capacity that has emerged as the 'L2' grammar of the average plantation inhabitant (male field slave).

The E-language exhibits prototypical pidgin characteristics, since the lexical inventory is quite limited and concurrently no inflection exists. The lexical items stored consist of the common subset of shared items across the population. While this pidgin – as most pidgins – emerged abruptly (within 24 cycles or 2 'years'), it is quite stable. The scenario lends indirect support to related notions of McWhorter (1998, 2005) in the claim of a pre-existing West African Pidgin prior to the emergence of Sranan, although in the current case the pidgin emerges with the plantation setting as the initial contact point. All told, an interesting dynamic unfolds between the relatively 'sophisticated' internal L2 knowledge of the average speaker which includes a scant acquisition of L2 inflectional morphemes and the pared down external L2 repertoire arising in the overall population. Of particular note is the rapid diffusion and stability of the pidgin variety throughout the general population.

Some final issues merit attention: first, the intent to remain faithful to as many demographic points as possible and to endow adult learners with realistic resources does ultimately lead to an invariant bias in the model. It is tempting to argue that populations of elite house slaves and overseers would acquire a (higher) portion of European language features, due to their necessary interactions directly with the slave owners and the high social value of that they would attain for acquiring (aspects of) the L2. Repeatedly, the absence of genetic reproduction in the model produced a situation of early 'extinction' for European inhabitants. Based on the historical records, new influxes of Europeans were not periodically introduced (Arends 1995) as occurred for the continually replenished African population. This general paucity of Europeans and diminishing contact with them

over time, coupled with stable and growing African groups, is precisely the intuitive conclusion that creolist scholars advance for the emergence of Sranan, Haitian Creole, and other historical plantation colonies. The present model shows that these conditions may have more naturally resulted in the widespread maintenance of an L2 African language variety across the society for an extended period of time.

Secondly, we must be cautious with interpretations of this model that attempt to link the lack of L2 mastery or creole formation with the fact that no child agents were included. Our study specifically examines the notion of a population of adult language learners as the most plausible originators of a creole variety, in keeping with the L2 adult creole genesis hypothesis advanced in the literature. However, we submit that the number of children and the interactions necessary for their presence to have linguistic significance are open questions that can be effectively addressed in part by computational modeling. There are some principled estimates of the numbers of children populating the plantations at the time of Sranan's emergence, even while historical records remain scanty. Preliminarily, computational models in Satterfield (2001, 2005a) reconstruct the situation in which children are 'rare' and have limited social interactions throughout the plantation setting, based on a simplification of Arends' (1995) projections. Satterfield's studies illustrate that when children between the ages of 5–11 years old make up between 10–15% of the total population, plantation outcomes towards creole formation are significantly increased. Unlike the Language Bioprogram Hypothesis (Bickerton 1984), the computational models do not demonstrate that children (without intervention from adults) play a central role in (Sranan) creole formation, nor do the simulations uphold any claim that children must have existed in large numbers in the population. Moreover, Satterfield's (2001, 2005a) computational models suggest that the children emerging with 'prototypical creole effects' were likely to exhibit bilingual linguistic knowledge, but only when allowed broad contact with adult speakers in the plantation setting. Following Sebba (1997: 179) and Roberts (2000), and as noted in Becker & Veenstra (2003), child bilingualism may well be a necessary precursor to creole formation.

## **8. Conclusion**

In terms of our initial objectives, the aim of this paper was straightforward: we examined the standard "imperfect L2" theory of creole formation to determine whether its central constructs could be faithfully applied to model a



(relatively) well-documented historical setting. We also illustrated that the psycholinguistic attributes and capacities of adult L2 learners as implemented in the model are in line with the L2 creolist proponents view (although the computational parameters are by necessity more explicitly outlined than most standard creolist proposals given the requirements of computer programming algorithms). Overall, this study has demonstrated that it is possible to reconstruct historical contexts for examining cognitive and cultural issues in light of language emergence and change. Large-scale effects were produced in the plantation domain as populations of agents interacted and adapted locally in various ways within a CAS environment. Based on micro-specifications with regard to agents, the environment, and rules of behavior in the model, innovative macro-structures and collective behaviors were generated in the system. Thus, the emergent property of the CAS – in this case, of growing linguistic structures in the computer – is clearly demonstrated in the present study.

As regards creole formation, the findings of the current computational model have been found to be inconsistent with the ‘imperfect L2’ hypothesis.<sup>21</sup> ‘Prototypical creole’ effects mirroring Sranan structures were not evidenced in the present study’s I-language or E-language results for adult L2 speakers. Adults created an incipient stable pidgin which continued with each successive generation of L2 speakers; however, expansion of the lexicon or radical changes in the structures did not occur in this adult-only context.

While the fact that creoles did not emerge in the adult L2 context cannot be directly attributed to the absence of children in the population based on this single experiment, we suspect that the childless aspect may be linguistically relevant, especially when viewed in conjunction with several other social factors: a) the increasingly restrictive and hierarchical nature of plantation society rendered improbable the need for most adults to engage extensively in prolonged crosslinguistic contact, whereas young children were typically allowed to circulate relatively freely (Arends 1995); b) the wave of African-born slaves regularly introduced into the environment permitted

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<sup>21</sup> As an anonymous reviewer rightly observes, “There’s a problem with any simulation result showing a negative result to argue against a theory. If a particular result fails to emerge in the simulation it’s hard to know if this is a failure of the theory or a failure of the translation of the theory into the model....it is a tricky aspect of modelling in general...Certainly, the most useful thing that can be done is to consider modifications to the theory that could be translated into the model to give positive results.”

established adult slaves to retain an L1 base for communication; and c) sufficient prestige/social currency may actually not have been generated for the average adult substrate speaker to be inclined to acquire more than a few lexical items of the L2 target.

Possible criticisms of the current investigation could include the notion that such computational models are path-dependent, in that the results are shaped by the precise initial conditions chosen. However, this evaluation does not easily hold for experiments representing a CAS approach. As demonstrated, with the CAS it is not trivial, and sometimes it is impossible to determine what behavior a CAS will generate, even with a relatively simple CAS. Moreover, while we quite tentatively interpret the negative results (e.g., that a creole language did not emerge, in contrast to real world outcomes of Sranan) of this single experiment as an indication that certain presumptions of ‘imperfect L2’ are flawed, modifications which do not adhere so closely to this particular ‘imperfect L2’ theory could be tested in subsequent models and could conceivably yield different results.

Motivated by theoretical and practical concerns, such modifications might include phonological and semantic features that contribute to the selection/exclusion of specific grammatical and lexical elements over others. It would also be ideal to adapt the model to examine other language contact scenarios. Lastly, demographic factors must be fine-tuned to include more facts of African and European populations and to pinpoint those ‘critical mass’ conditions necessary for triggering individual and population-wide creole emergence over time.

## **Acknowledgements**

This general project was carried out between September 1999 and April 2004, and was supported by various funds and grants from the University of Michigan. I gratefully acknowledge all computational and technical assistance through several stages of this project from Wei Chen, Rick Riolo, and from the University of Michigan Center for the Study of Complex Systems. I thank the organizers and participants in the 2005 Blankensee Colloquium for their input, as well as an anonymous reviewer of this manuscript for generous feedback. All errors and omissions are my own.

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# **Forces in Language Change**



# **Economy of Merge and grammaticalization: Two steps in the evolution of language**

*Elly van Gelderen*

Recently, the introduction of the mechanism linking two elements has been claimed as the “‘Great Leap Forward’ in the evolution of humans” (Chomsky 2005: 11). A slight rewiring of the brain might have made the operation Merge possible<sup>1</sup> and, in its turn, Merge made syntax possible by combining concepts into multiple unit expressions, with in principle infinite recursion. The current paper has a relatively narrow focus in that it will examine the consequences of Merge for language evolution. I argue that there are two crucial steps to the evolution of syntax, namely Merge and, following from principles connected with it, grammaticalization. Grammaticalization is an easily observable process in the history of languages and has therefore frequently been seen as involved in the evolution of language from its earliest stage to the present. The emergence of Merge brings with it certain relations such as specifiers, heads, complements, and c-command. Heads, complements, and specifiers in turn define argument structure (e.g. as in Hale & Keyser 2002). In addition, I argue that there was a second development due to principles of Merge Economy. These were responsible for processes known as grammaticalization. For instance, even though subordination was in principle possible, it probably didn’t arise till the grammaticalization of complementizers. Merge brought about the first step of linguistic evolution but principles connected with it were responsible for further language evolution. These processes continue up to the present.

The outline is as follows. In section 1, I discuss Merge and phrase structure rules and their relevance to linguistic evolution. In section 2, I discuss grammaticalization and the Economy Principles that account for it, and provide examples of the linguistic cycle. In section 3, I propose a link between evolution and grammaticalization, and in section 4, a principle is

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<sup>1</sup> Chomsky entertains both the possibility that syntax was “inserted into already existing external systems”, namely the sensory-motor system and system of thought (Chomsky 2002: 108), as well as the one where the externalization develops after merge Chomsky (2006: 9–10).

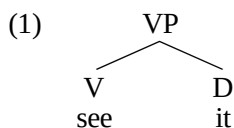
suggested that incorporates new lexical items. In section 5, I provide a conclusion and look into some criticisms of the view that grammaticalization provides us insight into language evolution.

## 1. Universal Grammar, Merge, and its implications

In this section, I discuss the generative model and in particular Universal Grammar, how Merge works in a derivation, and also what further structural relationships it is responsible for. I then comment on language evolution.

Starting in the 1950s, Chomsky and the generative model he develops present an alternative to then current behaviorist and structuralist frameworks. Chomsky focuses not on the structures present in the language/outside world but on the mind of a language learner/user. The input to language learning is seen as poor (the ‘poverty of the stimulus’ argument) since speakers know so much more than what they have evidence for in their input. How do we know so much on the basis of such impoverished data? The answer to this problem, Plato’s problem in Chomsky (1986), is Universal Grammar (hence UG), the initial state of the faculty of language, a biologically innate organ. UG helps the learner make sense of the data and build up an internal grammar. Initially, many principles were attributed to UG but currently (e.g. Chomsky 2004, 2006), there is an emphasis on principles not specific to the faculty of language, i.e. UG, but to “general properties of organic systems” (Chomsky 2004: 105). Merge is one such operation that can be seen as a UG principle (Chomsky 2006: 4) but also as one possibly “appropriated from other systems” (Chomsky 2006: 5). I’ll now turn to how a sentence is actually produced.

In the Minimalist Program, the most recent generative framework (Chomsky 1995, 2004), a Modern English derivation proceeds as follows. Merge combines two items, e.g. *see* and *it* in (1), and one of the two heads projects, in this case V, to a higher VP:

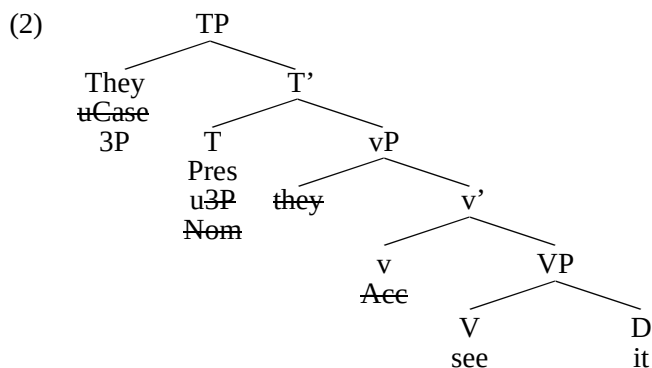


There is some debate as to the labels, which for most people are added for convenience only. I will also add them. Phrase structures are built using Merge and Move (also called external and internal Merge respectively). The

VP domain is the thematic-layer, i.e. where the argument structure is determined.

Apart from merge, there are “atomic elements, lexical items LI, each a structured array of properties (*features*)” (Chomsky 2006: 4). Each language learner selects the features compatible with the input. The main kinds involve Case, agreement (also known as phi-features), and displacement to subject position. Features come in two kinds. Interpretable ones include number on nouns and are relevant at the Conceptual-Intentional interface. Uninterpretable features include agreement features on verbs and Case features on nouns. These features need to be valued and deleted. Continuing the derivation in (1) will make the function of features clearer.

After adding a (small) *v* and subject *they* to (1), functional categories such as *T* (and *C*) are merged to VP. Agree ensures that features in TP (and CP) find a noun or verb with matching (active) features to check agreement and Case. So, *T* has interpretable tense features but uninterpretable phi-features. It probes (‘looks down the tree’) for a nominal it c-commands to agree with. It finds this goal in *they* and each element values its uninterpretable features: the noun’s Case as nominative and the verb’s phi-features as third person plural. The final structure will look like (2) where the features that are not ‘struck through’ are interpretable. The subject moves to Spec TP for language-specific reasons:



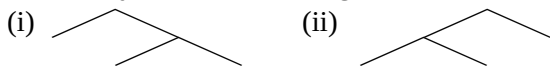
The derivation in (2) uses early lexical insertion, i.e. a lexicalist approach, as in Chomsky (1995; 2004). For the purposes of this paper, nothing hinges on this. Note that Merge and (3) below are neutral as to where lexical insertion takes place.

Structures made by Merge involve heads, complements, and specifiers. Merge, thus, automatically brings with it, the following UG principles:

**(3) Principles connected with Merge**

a. Merge involves projection, hence headedness, specifiers, and complements

b. The binary character of Merge results in either:

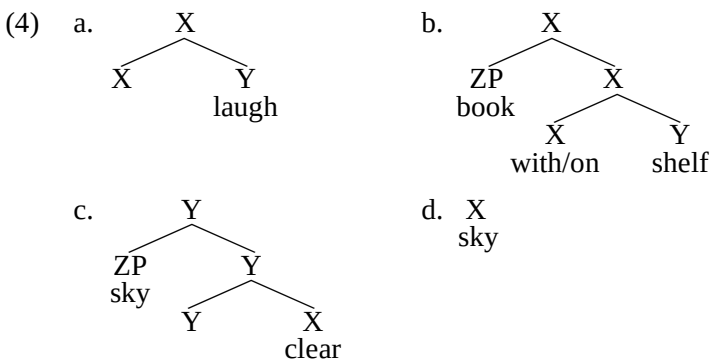


c. There is c-command of the specifier over (the head and) the complement, resulting in the special nature of the specifier.

A lot can of course be said about each of these. For instance, it has been argued that all languages are right-branching as in (3bi). This would mean there are no headedness parameters. Pidgins and creoles are typically SVO, however, i.e. (3bi), and this may also be the proto-order, though e.g. Newmeyer (2000) argues that the proto-language was SOV.

Turning to language evolution, languages closer to the proto-language will have Merge but there is no reason they would have Move and Agree as in (2) (though Newmeyer 2000: 385, note 4 suggests that proto-languages may have been inflectional). My approach assumes that agreement and Case arise later.

If language was mono-categorial, as Gil (2006) calls it, a root can be made into a noun or a verb depending on its syntactic environment. This assumption is not surprising given work by e.g. Jelinek (1998) and Hale & Keyser (2002). Here, the lexical categories are defined by the four possible relations that Merge provides. In (4a), there is a head and complement, resulting in an unergative structure; in (4b), there is a head, a complement, and a specifier, resulting in a locative structure; in (4c), there is a head and a specifier, the traditional adjectival predicate, and in (4d), there is just a head (examples are from Hale & Keyser 2002, with XP, YP, and ZP arbitrary phrases):



So, the first step in the evolution of syntax is Merge. It brings with it notions of headedness (once you merge two elements, one determines the resulting label) and binarity. These notions also determine possible argument structures. The next step is for grammatical heads, such as auxiliaries and prepositions, to appear, as we will discuss in section 3. First, however, we need to look at the mechanisms of adding grammatical elements. I will argue that Economy of Merge is responsible for that.

## **2. Grammaticalization as Economy**

As is well-known, grammaticalization is a process whereby lexical items lose phonological weight and semantic specificity and gain grammatical functions.

Grammaticalization has frequently been investigated in a functionalist framework. Recently, however, structural accounts have started to appear (e.g. Abraham 1993; Roberts & Roussou 2003; van Gelderen 2004) accounting for the cyclicity of the changes involved. Van Gelderen, for instance, uses Economy Principles that help the learner acquire a grammar that is more economical, and as a side-effect more grammaticalized.

Two Economy Principles, provided as (5) and (13) below, are formulated in van Gelderen (2004). They are part of UG and help learners construct a grammar. They are similar to principles such as c-command, in that they remain active in the internalized grammar and therefore also aid speakers in constructing sentences. They are different from absolute principles such as c-command because they help the learner/user analyze the data.

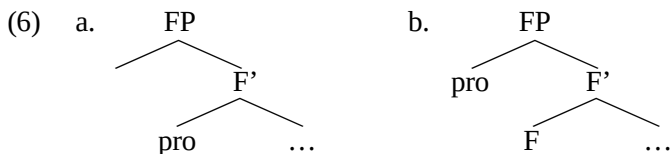
Principle (5) is a principle at work in the internalized grammar and holds for merge (projection) as well as move (checking). It is most likely not a principle specific to language but a property of organic systems:

### **(5) HEAD PREFERENCE PRINCIPLE (HPP):**

Be a head, rather than a phrase.

This means that a speaker will prefer to build structures such as (6a) rather than (6b). The FP stands for any functional category and the pro(noun) is merged in the head position in (6a), and in the specifier position in (6b). Other categories, such as adverb or preposition, work the same way:





The speaker will only use (b) for structures where a phrase is necessary, e.g. coordinates. There may also be prescriptive rules stopping this change (as there are in French, see Lambrecht 1981).

As is well-known, native speakers of English (and other languages) producing relative clauses, prefer to use the head of the CP (the complementizer *that*) rather than the specifier (the relative pronoun *who*) by a ratio of 9:1 in speech. As expected, children acquiring their language obey this same economy principle. Thus, according to Diessel (2004), young children produce only stranded constructions in English, as in (7):

- (7) those little things that you play with.  
(Adam 4:10, from Diessel 2004: 137)

Once they become (young) adults, they are taught to take the preposition along.

The Head Preference Principle is relevant to a number of historical changes: whenever possible, a word is seen as a head rather than a phrase. In early Germanic, the negative element *ne* precedes the verb (as in other Indo-European languages). In the North Germanic languages, this *ne* is phonologically very weak. As Wessén (1970: 100) puts it “[d]a die Negation schwachtonig war, machte sich das Bedürfnis nach Verstärkung stark geltend”. This strengthening comes in the form of an enclitic *-gi* that attaches to regular words. This results in *eigi* ‘not’, as in (8), *aldrigi* ‘never’, *eitgi/ekki* ‘nothing’, and numerous other forms:

- (8) *Þat mæli ek eigi* Old Norse  
that say-1S I not  
‘I am not saying that.’ (from Faarlund 2004: 225)

Faarlund (2004: 225) states that the *-gi* suffix is no longer productive in Old Norse but rather that it is part of negative words. That means that *eigi* and other negatives in Old Norse are phrasal adverbs, in specifier positions, as is obvious because they trigger V-second, as in (9):

- (9) *eigi vil ek þat* Old Norse  
 not want I that  
 'I don't want that.' (Faarlund 2004: 225)

For Modern Norwegian, Johannessen (2000) argues that *ikke* 'not' (derived from *eigi*) is a head. That means that between Old Norse and Modern Norwegian, the negative is reanalyzed from specifier to head. That it is a head is also clear from (10) since it adjoins to the verb. An expected further change is that the head weakens phonologically and this is indeed the case as is fairly obvious from sentences such as (10) taken from a novel, pretty common according to native speakers:

- (10) *Men detta æ'kke et forslag som vi har interesse av* Norwegian  
 but that is-not a proposal that we have interest in  
 'But that's not a proposal we are interested in.'

This development is similar to that in English with negative auxiliaries such as *don't*.

The next stage after (10) is when the weakened negative is reinforced. This may be occurring in certain varieties of Norwegian. Thus, Sollid (2002) argues that in the Northern Norwegian dialect of Sappen a double negative is starting to occur, as in (11):

- (11) *Eg har ikke aldri smakt sånne brød* Sappen Norwegian  
 I have not never tasted such bread  
 'I haven't ever tasted that kind of bread.' (Sollid 2002)

She argues this is under the influence of Finnish, which may well be the case. This would, however, not be possible if the grammar wasn't ready for this, i.e. if *ikke* weren't already a head.

The changes can be summarized in Figure 1, where (a) and (b) represent Old Norse, (c) is Norwegian, and (d) represents a variety such as Sappen with the verb moving through the Neg-head and the negative being reinforced by a new specifier. Traditionally, these changes are known as Jespersen's Cycle.

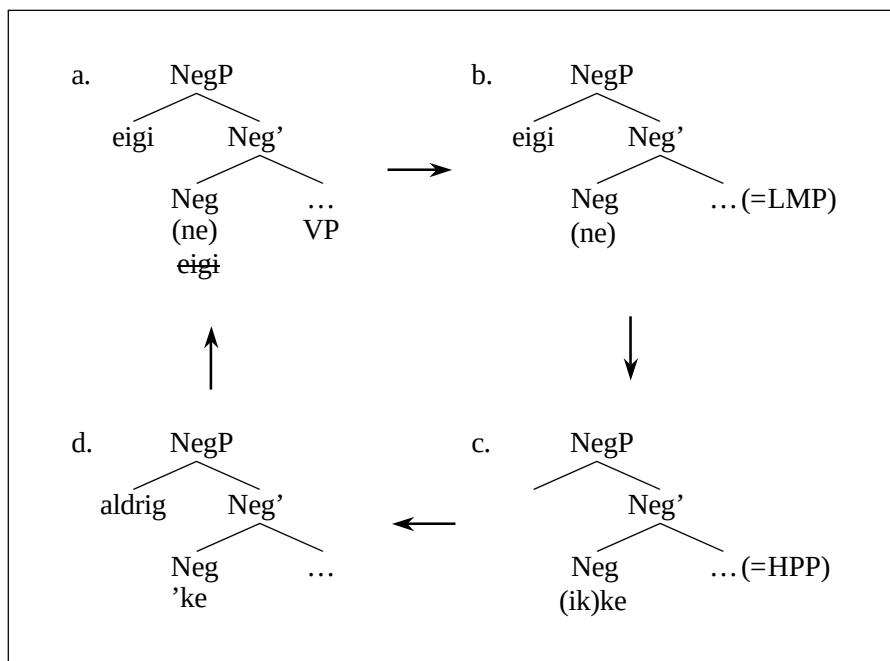


Figure 1. The Negative Cycle

Other examples of changes predicted by the HPP are given in (12):

- |                                                     |                           |
|-----------------------------------------------------|---------------------------|
| (12) Relative pronoun <i>that</i> to complementizer | Demonstrative to article  |
| Negative adverb to negation marker                  | Adverb to aspect marker   |
| Adverb to complementizer (e.g. <i>till</i> )        | Full pronoun to agreement |

Under a Minimalist view of change, syntax is inert and doesn't change; it is the lexical items that are reanalyzed. Pronouns are reanalyzed from emphatic full phrases to clitic pronouns to agreement markers, and negatives from full DPs to negative adverb phrases to heads. This change is, however, slow since a child learning the language will continue to have input of, for instance, a pronoun as both a phrase and a head. Lightfoot (1999) develops an approach as to how much input a child needs before it resets a parameter. In the case of pronouns changing to agreement markers, there will have to be a large input of structures that provide evidence to the child that the full phrase is no longer analyzed as that. This is already the case in French, where in spoken French, the pronoun is always adjacent to the Verb. The child, therefore, always produces the pronoun in that position, even

though regular subjects can precede or follow the verb (see Pierce 1992). However, the exact nature of the input needed for the change, the ‘cue’, is not explored in this paper.

Within Minimalism, there is a second economy principle that is relevant to grammaticalization. Combining lexical items to construct a sentence, i.e. Merge, “comes ‘free’ in that it is required in some form for any recursive system” (Chomsky 2004: 108) and is “inescapable” (Chomsky 1995: 316, 378). Initially, a distinction was made between merge and move and it was less economical to merge early and then move than to wait as long as possible before merging. This could be formulated as in (13):

(13) LATE MERGE PRINCIPLE (LMP):

Merge as late as possible.

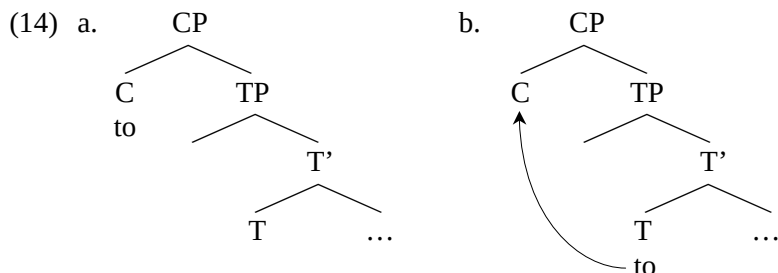
In later Minimalism, Merge is reformulated as external Merge and Move as internal Merge, with no distinction in status. One could argue that (13) is still valid since the special Merge, i.e. internal Merge, requires steps additional to the ones Merge, i.e. external Merge, requires. The extra step is the inclusion in the numeration of copies in the case of internal Merge, e.g. a copy of *they* in (2). Traces are no longer allowed, since they would introduce new material into the derivation after the initial selection, and therefore copies of elements to be moved have to be included in the lexical selection. Move/internal Merge is not just Move but ‘Copy, Merge, and Delete’. Since the numeration has to contain more copies of the lexical item to be internally merged, and since those copies have to be deleted in the case of traditional Move, (13) could still hold as an Economy Principle. In addition, Chomsky (2005: 14) suggests that a real difference between the two kinds: external merge is relevant to the argument structure, whereas internal Merge is relevant for scope and discourse phenomena. This indicates a crucial difference between the two kinds of operations that is expressed in the LMP<sup>2</sup>.

The Late Merge Principle works most clearly in the case of heads. Thus, under Late Merge, the preferred structure would be (a) with *to* base gener-

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<sup>2</sup> In other work, I have reformulated the Late Merge Principle as a Principle of Feature Economy. For the computational system, it is more economical to work with grammatical than with semantic features. Thus, a lexical verb such as Old English *willan* ‘to want’ has semantic features such as volition and future and is reanalyzed as just having grammatical future features.

ated in a higher position (here C but nothing hinges on that), rather than (b) with *to* in a lower position (here T) and moving to the higher position. See also Kayne (1999):



This is indeed what has happened in a number of changes. There is evidence that Modern English speakers prefer (a) over (b):

- (15) a. It would be unrealistic **to not** expect to pay higher royalties.  
(BNC-CSS 245)
- b. It would be unrealistic **not to** show them to be human.  
(BNC-CBF 14312)

Corpus data show this preference, e.g. the adverb *probably* splits the infinitive in 22.7% of the cases in the (mainly written) British National Corpus, but in an American spoken corpus (CSE) it does so at 100%. The prescriptive rule against split infinitives is thus alive and most obvious in British written varieties. Such external rules interfere with Economy.

The preposition *for* underwent the same change from preposition indicating space to cause to non-finite complementizer. Examples are provided in (16) to (19):

- (16) *ouþer for untrumnisse ouþer for lauerdes neode ouþer for haueleste ouþer for hwilces cinnes oþer neod he ne muge þær cumon*  
'either from infirmity or from his lord's need or from lack of means or from need of any other kind he cannot go there' (PC, anno 675).
- (17) *ac for þæm þe hie us near sint, we ... ne magon ...*  
but for that that they us close are, we ... not may ...  
'but because they are near to us, we can't...'  
(Orosius, Bately 122.18–19)

- (18) *for æuric man sone ræuede oþer þe mihte*  
 because every man soon robbed another that could  
 ‘because everyone that could robbed someone else.’ (PC, 1135, 8)

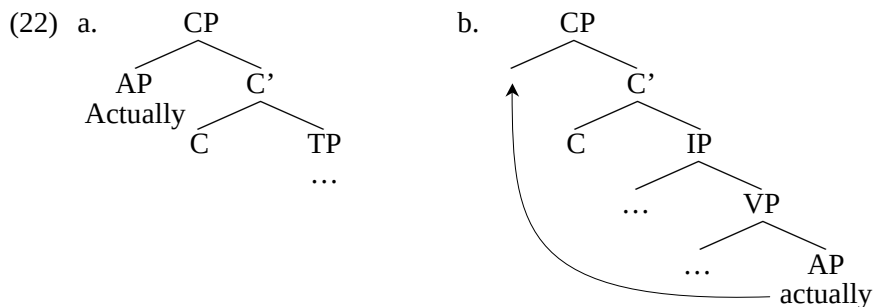
- (19) *for agenes him risen sona þa rice men*  
 ‘because against him soon rose the powerful men.’ (PC, 1135, 18)

This accounts for the change from lexical to functional **head** or from functional to higher functional head so frequently described in the grammaticalization literature (e.g. Heine & Kuteva 2002). Late Merge also accounts for lexical **phrases** becoming base generated in the functional domain. An example is *actually*. When it is first introduced into the English language from French, it is as adjective (in 1315), and is then used as a VP adverb in the 15<sup>th</sup> century, meaning ‘with deeds in actual reality’ as in (20). It then changes to a higher adverb, as in (21), in the 18th century:

- (20) Those who offend **actually**, are most grievously punished.  
 (OED 1660 example)

- (21) **Actually**, it is kind of an interesting problem. (CSE-FAC97)

Structure (22a) shows the more recent structural representation and (22b) the earlier one. The preferred one under the LMP is (22a):



Other examples of the LMP are given in (23):

- (23) *Like*, from P > C (*like I said*)      Negative objects to negative markers  
 Modals: v > ASP > T                      To: P > ASP > M > C

How exactly does Late Merge account for language change? If non-theta-marked elements can wait to merge outside the VP (Chomsky 1995: 314–

315), they will do so. I will therefore argue that if, for instance, a preposition can be analyzed as having fewer semantic features and is less relevant to the argument structure (e.g. *to*, *for*, and *of* in ModE), it will tend to merge higher (in TP or CP) rather than merge early (in VP) and then move. Like the Head Preference Principle in (2), Late Merge is argued to be a motivating force of linguistic change, accounting for the change from specifier to higher specifier and head to higher head. Roberts & Roussou (2003) and Simpson & Wu (2002) also rely on some version of Late Merge.

Concluding section 2, under the LMP as under the HPP, syntax is inert; it is the lexical items that are reanalysed. In this section, I have examined the Economy of Merge. Two principles, the HPP and the LMP, provide an insight into what speakers do when they construct a sentence. In the next section, I will apply these to a scenario for language evolution.

### 3. Grammaticalization and Language Evolution

In section 1, I have provided some background on how Merge could have been the first step in creating syntax from a stage that consisted of either words or gestures (e.g. Corballis 2000), or as Traugott (2004: 134) puts it “an exaptation of thematic role structure”. The current section provides a scenario for subsequent steps.

Once Merge applies, certain structural and thematic relationships crystallize, as in (4) above. Another head (the small *v*) can be merged to structures as in (4), to accommodate the agent or causer in its specifier. The *vP* represents the thematic level, and one that adult native speakers employ when they speak or write in ‘fragments’, as in (24). Children reach this stage too, as (25) shows, though they understand grammatical categories before they produce them:

(24) work in progress

(25) want cookie

The next evolutionary stage is highly speculative, but it could be that the lower head starts to move to a higher head to express different relationships, e.g. *clear* from ‘The sky is clear’ to ‘That clears the sky’. This is when the LMP applies as well and grammatical elements arise, just as they do in the history of English in (16) to (19) above. One feature of the lexical element is emphasized over others (hence the slight semantic loss).

This same development can be seen in child language where children first master prepositions before using complementizers. Josefsson (2000: 398) shows that Swedish “children first acquire the PP and then, directly after that the subordinate clause”. She divides the acquisition into Stage I with no prepositions, Stage II with occasional prepositions, and Stage III with first prepositions and then complementizers. “[M]ost often, the children do not start using complementizers at all until they have reached a 90% use of prepositions” in obligatory contexts, as in (26) and (27). These sentences provide a good illustration since the preposition and the complementizer have the same form *som*:

- (26) *precis som en kan / som en kanin*  
just like a rab / like a rabbit
- (27) *grisen, den som heter Ola*  
pig that who is-called Ola  
(Embla, 27 months, both from Josefsson 2000: 410)

In English, we have a similar preposition, namely *like*. In fact, *like* can be a verb as well, as in (28), from Abe (data from Kuczaj 1976 in the CHILDES database). What is interesting for a comparison with the Swedish data is that, at e.g. 3 years and 7 months old, Abe produces *like* many times as a verb and a preposition, as (28) and (29) show, but not as a complementizer. Later, e.g. at 4 years and 10 months, he uses *like* as a complementizer in (30). So the data are comparable to the Swedish, and the LMP works in child language as well:

- (28) **like** a cookie (Abe, 3.7)
- (29) no the monster crashed the planes down **like** this **like** that (Abe, 3.7)
- (30) Daddy # do you teach **like** you do [//] **like** how they do in your school?  
(Abe, 4.10)

Similar data exist for *for* in English.

In conclusion, in this section, I have given some evidence from language acquisition that the HPP and LMP are part of the internalized grammar. I am proposing that language evolution followed a similar path, but we of course lack empirical evidence.



#### 4. Renewal and the cycle

If the initial evolutionary stage of syntax is one where pragmatic relations are important (e.g. Bickerton 1990), the emergence of Merge will have the effect of incorporating the pragmatic material into a syntactic structure. This also occurs in grammaticalization – it is actually the start of the whole process. The two principles used in section 2 (HPP and LMP) take lexical material that is already part of the structure and change the position of it. There are also a number of changes where a new element comes from outside of the sentence, e.g. a special pronoun being incorporated into the CP to indicate subordination, and an emphatic topic pronoun becoming the subject (in Spec TP). This can be expressed by means of a principle that incorporates (innovative) topics and adverbials in the syntactic tree:

(31) SPECIFIER INCORPORATION PRINCIPLE (SIP)

When possible, be a specifier rather than an adjunct.

Sometimes, these ‘renewals’ are innovations from inside the language, as in the case of the English negative DP *na wiht* ‘no creature’ to mark negation but other times, these renewals are borrowed through contact with other languages. One such possible case is the introduction into English of the *wh*-relative. In Old English, there are a number of relative strategies, but by Early Middle English, the complementizers *þat* and *þe* are typical. This is predicted under the HPP since those forms are heads (see van Gelderen 2004: 83–87). By later Middle English, this form is ‘competing with the *wh*-pronoun still present in present-day English (be it mainly in written English). Mustanoja cites Latin influence for the introduction of the *wh*-pronoun. Romaine (1982) shows that the introduction of the *wh*-pronouns was stylistically influenced, and Rydén (1983) shows both Latin and French influence. The first instances of *who* occur in epistolary idioms that are very similar to those in French letters of the same period. For instance, in many of the collections of letters from the fifteenth century, the same English and French formulaic constructions occur, such as in (32) from Bekynton and (33) from the *Paston Letters*:

(32) *a laide de Dieu notre Seigneur, Qui vous douit bonne vie et longue.*

with the-help of God our lord, who us gives good life and long

‘With the help of God, our Lord, who gives us a good and long life.’

(Bekynton, from Rydén, p. 131)

- (33) *be the grace of God, who have yow in kepyng*  
 ‘by the grace of God, who keeps you.’ (Paston Letters 410)

The *wh*-pronoun is in the specifier position (since it can pied-pipe a preposition and is inflected). This shows that, for creative reasons, speakers can start to use the specifier again.

How are the three principles mentioned so far responsible for cyclical change? Let’s see what happens when we combine the effects of the HPP and the LMP, as in Figure 2. The HPP will be responsible for the reanalysis, as a head, of the element in the specifier position; the LMP will ensure that new elements appear in the specifier position:

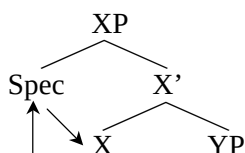


Figure 2. The Linguistic Cycle

This scenario works perfectly for changes where a negative object such as Old English *na wiht* ‘no creature’ becomes a Spec and subsequently a head *not* of a NegP, or for the Scandinavian change chronicled above, and for a locative adverb being reanalyzed as part of the higher ASP(ect)P. The SIP would enable the Specifier position to be filled from the outside.

Givón (1979) and others have talked about topics that are later reanalyzed as subjects, and call this a shift from the pragmatic to the syntactic. What this means is that speakers tend to use the Phrase Structure rules, rather than loosely adjoined structures. With (31) added, typical changes can therefore be seen as (34):

- (34) a. Head > higher Head > 0 (=LMP)  
 b. Adjunct > Spec > Head > 0 (=SIP/LMP and HPP)  
 Phrase >

The change in (a) is the one from lower head (either lexical or grammatical) to higher head, via LMP. The change in (b) shows that either an adjunct (via SIP) or a lower phrase (via LMP) can be reanalyzed as specifiers, after which the specifier is reanalyzed as head (via HPP).

In this section, I have suggested that the emergence of syntax could have followed the path that current grammaticalization also follows and one that

children take as well. In particular, Merge brings with it, a set of relations and a set of Economy Principles, from which grammaticalization and language change follow. The Economy Principles I have been discussing in section 2 are of the non absolute kind: if there is evidence for a pronoun to be both a phrase and a head, the child/adult will analyze it initially as head unless there is also evidence in the grammar (e.g. from coordination) that pronouns also function as full DPs.

## 5. Conclusion and response to critical views

I have looked at two steps that are required in the evolution from pre-syntactic language to language as we currently know it. The one is Merge and the structural and thematic relations it entails to build a basic lexical layer (the VP). The other is Economy of Merge, the HPP and LMP, the principles that enable learners to choose between different analyses. These two principles result in what is known as grammaticalization and build the non-lexical layers (the TP and CP). Lexical material is also incorporated into the syntax through a third principle, the SIP. This principle allows the speaker to creatively include new material, e.g. as negative reinforcement in special stylistic circumstances.

Newmeyer (2006) voices some criticism of the view that grammaticalization tells us anything about the origins of language. His main reasons for skepticism are the alleged uni-directionality of grammaticalization and the exclusively lexical status of categories that an initial stage would show under this scenario. Newmeyer notes that some grammaticalizations from noun/verb to affix can take as little as 1000 years, and wonders how there can be anything left to grammaticalize if this is the right scenario. The Specifier Incorporation Principle proposed in the previous section, however, provides an answer for what the source of the replenishments are, namely borrowings and creative inventions. The Economy Principles do not provide a reason why certain languages/societies are more conservative than others, e.g. why the split infinitive has encountered such opposition by prescriptivists, and has kept *to* from grammaticalizing more. The reasons for this are sociolinguistic. A similar point raised by Newmeyer is that there are some languages that are argued to have undergone very little grammaticalization, and that “grammaticalization per se cannot tell us very much about the origin and evolution of language” (2006: 1). This is a point that concerns the status of languages described in work by e.g. Gil, and on

which I cannot comment here, but see *Linguistic Typology* 9.3 (2005) for a critique of Gil.

Secondly, Newmeyer mentions that “there is no reason to assume that the earliest humans could not express concepts like ‘in’ and ‘past time’” (2006: 1). This argument only works if one assumes that all early words were arguments, and not adverbials. If early humans had words for ‘under the trees’ or ‘into a cave’, they could express location and direction, and then these could later grammaticalize into prepositions.

## Abbreviations

|     |                                                                       |
|-----|-----------------------------------------------------------------------|
| BNC | British National Corpus, see references.                              |
| CSE | Corpus of Contemporary Professional American English, see references. |
| HPP | Head Preference Principle                                             |
| LMP | Late Merge Principle                                                  |
| OED | Oxford English Dictionary                                             |
| SIP | Specifier Incorporation Principle                                     |
| UG  | Universal Grammar                                                     |

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# Prehistoric and posthistoric language in oblivion

*Wouter Kusters*

To acquire knowledge about languages of the past we rely on knowledge of properties of spoken languages of today, written sources of older languages and the reconstructions made on the basis of these sources. By definition, we can not have any direct knowledge about prehistory or prehistoric languages, since prehistory is defined as the period from which we do not have any documents. Nevertheless several attempts have been made to intrude into language of prehistory. These intrusions can be characterised as either attacks in the back, in the front or in the middle. I will briefly describe these three approaches and discuss what is missing in each of them. I will then make some suggestions about how we could fill this lacuna. For that purpose I will use evidence from what we know about the relation between language structure and culture (cf. Perkins 1992), and I will apply the newly developing framework in which social constellations and social histories are correlated to expectations about structural changes (Andersen 1988; Braunmüller 1984, 1990; Kusters 2003; McWhorter 2002; Thurston 1987, 1992; Trudgill 1992, 1996, 1997; cf. also Milroy 1982; Milroy 1992; Siegel 1985, 1993). In the last section I will discuss what this framework predicts for the future of language variation.

## 1. Approaching prehistory from the back

The prehistory of language is often discussed in the context of the *evolution* of language. However, when evolution of language is discussed, this term is usually used to refer only to the *emergence* of language. Two pre-historic stages of the homo (sapiens) are compared then; the stage in which there was no human language yet, and the stage that a human language had evolved. The focus in such studies is on the question how animal communication could evolve into human communication. The answer depends first of all on what is considered as essential to human language. This may either be the symbolic capacity of human language (cf. Deacon 1998), or some syntactic principle, like “move-alpha”, (Bickerton 1990), or the use of lan-



guage as a kind of cement to bind members of a group (Dunbar 1996). These various perspectives imply different time-scales of the emergence of language as *human* communication. In these views there is some qualitative difference between animal and human communication and after the transition (or leap) into human communication, language as we know it would have been born. Evolution of language in this sense is equal to the birth of human language.

What is seldom accounted for in such studies is the question what happened with the newly born human language *after* it had emerged long ago in prehistory. Modest estimates are that human language as we know it has existed for at least 40.000 years, although claims of an age of 200.000 or perhaps even 400.000 years are more likely (cf. Foley 1997: 70ff.). Since in the studies mentioned above focus lies on the miraculous emergence of human language, the question is seldom raised whether and how the newly arisen form of human communication changed during the thousands and thousands of years after its initial emergence.

What these approaches have in common is the assumption that once human language came into existence, it did not essentially change anymore. Of course, it may have changed in its outlook, but not in its fundamental characteristics, independent whether these are syntactic, symbolic or social. The question then still remains, how was this prehistoric outlook? Were prehistoric languages indeed roughly similar to present-day languages, or were they different? If so, how would they have been different, and how could we know? These are all questions that cannot be answered by research and speculations into the emergence of language alone. When we would like to know more about differences and similarities between modern and prehistoric languages we should perhaps start from somewhere else.

## 2. Approaching prehistory from the front

Many inquiries into the past start from the present. On the basis of knowledge of present-day languages and older documents, reconstructions are made of older stages. These have yielded many insights into genetic relations between languages, their origins and processes of language change. However, the reliability of these reconstructions decreases the further back they go in time. While for many languages and language families we have considerable knowledge about their recent pasts, when we go one or several millennia back into time our perspective becomes rather cloudy. The boldest

reconstructions go back for at most 10.000 years, which still leaves tens of thousands of years of human language unaccounted for.

When we are interested in prehistoric language the reconstructionist approach has more disadvantages. First of all, it is mainly concerned with reconstructions of lexical material, whether these are vocabulary or affixes, and it leaves other areas of the grammar unaccounted for. Even more problematic when we would want to have a sense of what prehistoric language was really like, is that it is unclear in what sense reconstructions stand for actual languages spoken at the time. Reconstructions depend on the traces older languages left in modern languages, and prehistoric language phenomena that left no trace can never be reconstructed. In fact, reconstructions are always regular extensions, back-formations, or shared common forms on the basis of what is known today. As Comrie (1992) rightly remarked, when in a language a paradigm, for instance, is regularised, the lost irregularities cannot be reconstructed. On the other hand when a paradigm becomes less regular, for instance when sound changes confuse previous regular morphology, the underlying regular order can be detected. Because of this asymmetry reconstructions tend to be less capricious and more regular than languages we have first hand knowledge of. Reconstructed languages display more regularity than they must have had in reality.

Reconstructionists face another problem if they want to develop a realistic picture of prehistoric language(s). Not only irregular parts of languages have been wiped out, but also whole languages must have died out. For historic times we know about various periods that massive language death must have taken place. When the Spaniards conquered South America in the 16th century, due to epidemic diseases, wars famine and exploitation, the indigenous population in the Andes decreased by about 70% and in the lower coastal areas of Ecuador and Peru the mortality rate is said to have been up to 96% (Spalding 1999: 932). It is highly probable that with the millions that died in that period also tens or perhaps hundreds of smaller languages died. Such cases are numerous in history, see for instance the Indo-European expansion across Europe and South Asia. How many languages and how many language families were spoken by the pre-Indo-European populations? Today the only evidence we have, are some small pockets of pre-Indo-European languages like Basque and Burushaski, and some languages and language families at the fringes of the Indo-European area, like Dravidian and Caucasian languages. When we would tentatively draw a map of the distribution of the various language families at about 5000 BCE, we could see that there are wide areas in which we do not even

know what families of languages were spoken, while the language families that are large in geographical spread, number of speakers and internal variation today, like Indo-European or Sino-Tibetan, would be strikingly small. We may conclude that reconstructions can only explore prehistory shallowly and partially, while the overwhelming majority of prehistoric language facts remain out of sight.

### **3. Typology and uniformity**

IF we would not approach prehistory either from the back or from the front, we could try to conjecture about how prehistoric languages would sound on the basis of what we know about distributions and variation in modern languages. For instance, we know that sounds like clicks, implosives, front rounded vowels, or morphemes that express dual number or gender distinctions in the first person are all quite infrequent. From what we know about the distribution in modern languages, we could extrapolate to possible and probable distributions in prehistoric language. However, this would be methodologically quite unsound; in fact many language phenomena in modern languages are not representative for all possible languages. The more the likelihood of occurrence of a particular language phenomenon is co-determined by areal location and genetic affiliation, the more difficult it is to make a claim about its likelihood in general. For instance, clicks are found only within one language family, but its absence elsewhere may be just a contingent fact and does not entail anything about the abstract probability of clicks existing in human language or about the concrete probability of clicks in prehistoric languages. In other words, when agricultural and metallurgical progress, empire buildings, and colonisations had started from South West Africa the world may could have been full of clicks. This argument does not only apply to clicks. Nichols (1992) shows that many language phenomena of which we would think that they are less susceptible for areal or genetic influences, like ergativity or (in)alienable possession are not equally distributed among language families, let alone among languages.

Of course, we could modify our typological claim and only propose that everything that is possible today, should also have been possible in prehistory. This is in fact a variant of Labov's Uniformitarian Principle, which goes back to Whitney (1867) and states: "So far back as we can trace the history of language, the forces which have been efficient in producing its changes, and the general outlines of their modes of operations, have been

the same.” I think that such a claim, based on typology and the Uniformitarian Principle is both too strong and too weak. It is too strong, because it is unclear to what extent the Uniformitarian Principle would be valid for all times. First of all, the speaker’s cognitive or even her genetic make-up may have been different in other times. Then at least one of the most important factors (or *forces*) in ‘producing (language and) its changes’ would have been different. However, I leave that possibility aside, since it belongs to the question of how human language emerged. Secondly, the communicative situation or the social and cultural environment of prehistoric man may have been quite different in prehistoric times. If so, it is well conceivable that the Uniformitarian Principle does not hold, that is, the social or cultural ‘forces efficient in producing its changes’ may have been different, and then we might expect different kinds of changes and resulting structures. In this sense the combination of the Uniformitarian Principle with typology yields predictions that are too weak if we would distinguish within the Uniformitarian Principle several social forces, which lead to different language results, we could make slightly stronger claims about (pre-historic) languages which we cannot gather information about otherwise.

#### **4. Sociolinguistics feeding typology**

I will now turn to two forces, and two types of speech community whose prevalence in prehistory may have been different and which trigger two different kinds of language change. In my dissertation (Kusters 2003) I examine changes that lead to simpler more transparent languages versus (non-) changes that conserve, foster or even promote complexities and irregularities in language. I correlated these two kinds of change to two extreme poles of speech communities. One extreme I called a Type 1 speech community, and the other a Type 2 speech community. This distinction corresponds to most received knowledge in sociolinguistics and anthropological linguistics, see, for instance 1) Trudgill (1992, 1996, 1997), who described tightly knit versus loosely knit speech communities, 2) Andersen (1988), who distinguished between central and peripheral communities, with exocentric versus endocentric attitudes, 3) Thurston (1987, 1992), who discusses esoteric versus exoteric speech communities and 4) Nichols (1992) who examines the effects of two kinds of geographical positions, that is, residual areas and spread zones on language (see also Dixon 1997; Milroy 1992; Penny 2000; Ross 1999). Linguistic literature like Siegel (1985, 1993) and Mühlhäusler

(1995) and anthropological literature like Barth (1969), Giles (1979), LeVine & Campbell (1972), Wallman (1986) and Urciuoli (1995) have also informed the distinction between compact (Type 1) and expanding (Type 2) speech communities. The two types of speech communities may be characterised as follows:

The population of an idealised Type 1 community is relatively small, and most people know each other. Most interactions take place among members of the community, and when interactions take place with outsiders, a different language is used. Outsiders not raised in the community do not learn its language. The people of Type 1 communities share a large common background. Life cycles are relatively predictable, and the concept of time is often cyclic. Originality and innovation are not appreciated very much, and neither is the transmission of new information. Language is used to keep social relations between members of the community in balance. In addition, there is a body of literature (written or oral) in the community, which has a sacrosanct status. People are proud of their language, and they have stories and myths that relate the origin and form of the language to their religion and their cultural origins. In everyday life, verbal language skills and verbal play are appreciated. Different registers exist, which are largely shared by the members of the community, while there is no dialectal variation. Ties between community members and the possession of communal values are so strong that no local centres of prestige can develop, and therefore, also no dialects. An approximation of this idealised Type 1 community would be the until quite recently highly isolated Shammar community in northern Saudi Arabia, who speak a form of Najdi Arabic (for more details, cf. Kusters 2003: 107ff.).

Type 2 communities on the other hand only form a speech community because all members use the same language. They do not necessarily form a unit in space or time, or share other social or cultural values. The number of speakers of a Type 2 language can be high. Most speakers know other languages as well, and the language in question is often not their first language, but is only used as a *lingua franca*. The members of a Type 2 speech community do not share much background knowledge, and their precise way of speaking the language in question may differ. In interactions the language is mostly used for negotiating and exchanging practical information. The language in question is not associated with any cultural or religious standard, and speakers accommodate their way of speech freely in order to be clearly understood. The language does not function as a medium in which group identities are expressed. Other languages may be more im-

portant to the speakers to express their group affinities. There are no different registers, but there may be different dialects, or at least different ways of speaking, possibly related to the original languages of the new groups of speakers. This has the effect that when speakers interact, they must accommodate and assimilate each others' ways of speech. A prototypical example of a Type 2 community is the group of pidgin Swahili speakers in the larger cities in Kenya (see Kusters 2003: 316ff.).

When we plot the measure in which a speech community is of a certain Type to the extent that a language has changed in its morphological complexity, we find that there is, at least in the cases we studied, a correlation between inflectional reduction and regularisation on the one hand, and being of Type 2 on the other. On the other hand, Type 1 communities seem to be more suitable to foster and perhaps even promote complex morphological structures. I consider inflectional morphology to be complex when words comprise many (syntactic/semantic) categories, which are expressed in a non-transparent manner (with fusion, allomorphy, homonymy and so on), and whose mutual order in the word is irregular (see Kusters 2003: 5ff. for further discussion of the concept of complexity).

Now, as I discussed above, although we cannot decide on the basis of typological linguistic data alone how prehistoric language would look, we may conjecture something about its structure – in this discussion, that is, inflectional morphological complexity – on the basis of how we conceive of prehistoric social conditions. When we analyse Type 2 speech communities we come to three necessary conditions for Type 2 communities to emerge at all, and to be able to facilitate a particular language structure. These are:

- 1) a language must spread rapidly outside its initial sphere of use to a domain where it is predominantly used in its communicative function by second language learners,
- 2) to be simplified a language must remain in use for a longer period, and be learned by a next generation,
- 3) contact between the source language and the variety spoken in the new domain must be not too extensive, otherwise simplifications may be levelled out, especially when the source language is dominant.

Condition 1) has been fulfilled quite often in modern times: the languages I studied in my dissertation (Arabic, Scandinavian, Quechua and Swahili) all had a period that they spread rapidly. In recent history many other examples

can be found. In order to expand, a language must be used as a vehicle of inter-group communication. Trade is a context in which a language may be dispersed as a communication tool. However, in order to put a mark on speech varieties of next generations – that is, condition 2) – trade alone is seldom enough. Support from above, e.g. from a religious organisation, or a political empire is favourable for expansion and consolidation of a language. Such support has existed, for the languages of my dissertation, in the Arab/Islamic Empire, the Inca Empire, the Catholic church in the Andes and under other churches in Africa as well. Condition 3) states that the ties between the original speech community (with the relatively complex language) and the new community (with the simplified variety) should not be too strong, because otherwise the simplified variety may only be a *learner variety* of individual learners, which vanishes in next generation. In other words, L2 learners must have the opportunity to become a distinct group with a distinct language, instead of a collection of individual learners each directed at the – possibly more prestigious – source language. Too close contacts between the L2 learners and native speakers of the source language may prevent such group formation. From the examples from my thesis, Nubi Arabic in Uganda and Katanga Swahili in the Kongo are fine examples of separation from the source language. Kenyan Pidgin Swahili is a variety where condition 3) may no longer hold in the future, since it has a chance to disappear by the growing influence of Standard Swahili.

There are two typical situations where all three conditions hold. First of all, situations where a speech community expands outside its core area over new territories, and where the language is used initially for trade, but later adopted as a native language. When communication between the new speech community in the peripheral zone and the original speech community is restricted, simplified varieties may arise. In my dissertation I discuss examples from the fringes of the Quechua language area, the Congo and the Arab lands. Other examples would be the expansion of Latin under the Roman Empire and the spread of English and French as languages of colonisation in Africa. In all these instances, in the center a prestigious variety is spoken, while of the periphery simplified varieties are found.

The second situation is where a language of a small speech community becomes the dominant language of a larger empire, but where in the center the language is used as a vehicle of modernisation among various groups with various language and dialect backgrounds. In such cases older, more conservative varieties are retained at the periphery, while in the center processes of *koineisation* and regularisation may simplify the older variety.

Scandinavian is an example from my dissertation of this second type. Other cases are found in modern history, where older peripheral dialects are more complex than the compromised variety of the centre area of contact, for example, Catalan (Penny 2000) and Greek (Horrocks 1997), cf. also Andersen (1988) and Klammer (to appear).

## **5. Prehistoric sociolinguistics**

Although there are many cases of simplification in more recent history, the picture for prehistoric language may have been quite different. When we examine the likelihood of the social factors discussed above for this early period of mankind, we can come to some speculations about prehistoric morphological complexity and simplifications. Although we are then still miles away from sketching prehistoric language as it really was, we are a little closer to our aim than with the three approaches above. So the question is, were the prehistoric speech communities of Type 1 or Type 2?

Domestication of plants and animals did not take place before about 8,000 BCE (Schultz & Lavenda 1998: 182ff.). Before that time, man was dependent on gathering and hunting. Man did not remain at one location, but roamed around, dependent on seasonal changes, the life cycles of the plants they gathered and the migrations of the animals they hunted. This implied that there was little material wealth or private property apart from portable tools and small weapons. Although there are few remains from this period, apart from remains of bones and small tools and weapons, on the basis of this evidence and comparisons with ape behaviour and the composition of groups of modern hunter-gatherers it is estimated that prehistoric hunter-gatherer groups had between 20–30 up to at most 100 members (cf. Bentley & Ziegler 2000: 16; Starr 1973: 25ff.; Beaken 1996: 127).

These small bands must have been Type 1 speech communities. First of all, they were highly self-sufficient, and the subsistence strategy did not depend on larger social or cultural organisations. The human population on Earth was still small, and the hunter-gatherer groups must have lived rather isolated lives. Nevertheless, there were probably some contacts; groups must have met each other when they penetrated each other's territories. Apart from warfare, arrangements to exchange marriage partners and perhaps also cultural and technological goods are thought to have occurred. (Bomhard & Kerns 1994: 147; Bentley & Ziegler 2000: 16). The wide spread of early goods away from the original location where the material



originated, suggests at least either high mobility or early forms of trade (Starr 1973: 31). Second, it is generally assumed that at least after 40,000 BCE mankind had some form of ancestor worship, magic, and other cultural complexes, in which language may have had a dominant position (cf. Schultz & Lavenda 1998: 166; Foley 1997: 58). Third, since the groups were so small they must have shared a common background, and very little dialect variation existed within a band.

Since all three factors suggest that prehistoric hunter-gatherer groups lived in Type 1 speech communities, languages were probably at least as complex as languages spoken in Type 1 communities today. Because of the more extreme isolation and very small groups, the languages may have been even more complex in their inflectional morphology (still assuming that we are within a time-period in which genetic make-up was roughly equivalent to today). Support for this view is presented by Perkins (1992), who found that the lower the top-level of organisation in a culture is, like in foraging communities, the more deictic categories are grammaticalised. Assuming that this also indicates more inflectional complexity (cf. Kusters 2003, Trudgill 1996, 2001), and assuming that hunter-gatherer communities are representative for prehistoric communities, this suggests that prehistoric language had, indeed, more complex inflectional morphology. Perkins suggests that this can be explained by the kind of discourse in such communities. Hunter-gatherers would tend to base their discourse more on concrete actions and situations in the environment, which would lead to more grammaticalisation and incorporation of deictic affixes in verbs and nouns (cf. also Fortescue 1992; Fortescue & Lennert Olsen 1992).

The question remains whether in prehistory processes of simplification also took place. Were there as many cases of *lingua francas*, simplified varieties, trade languages and simplifying changes as we know of in modern history? Probably not. At that time there were no larger empires or spheres of influence, in which languages could rapidly spread from a prestigious centre to a new population in the periphery as in modern history. Traces of language spread correspond approximately to population movements (cf. Cavalli-Sforza et al. 1994). When groups moved to a new area, or replaced an earlier population, language spread ran roughly parallel to genetic spread. These are not the circumstances in which simplification is expected. A second way of spread was through the adoption of a language that was associated with technological or cultural innovation. Although language shift triggered by such dispersion among foragers is not impossible (cf. Evans & McConvell 1998 for evidence from Australian languages), it is

unlikely that there were appropriate prehistoric circumstances which would bring about the adoption of a lingua franca and, at the same time, ensure that the language could be simplified due to massive second language learning and transmitted to future generations. As Nettle and Romaine (2000: 103) point out: “The hunter-gatherer way of life tended to preclude the emergence of dominant cultures or languages...There were no empires, no armies, and no cities. In a moving world, centres and peripheries are hard to identify, and technological change and diffusion is localized and gradual.”

It was only after the rise of the first larger state-formations around 3,000 BCE that the accumulation of wealth and the stratification in society made it possible to control wider stretches of land and populations. When transport and communication improved, people could more easily spread from one centre and dominate larger areas, and the processes of massive and imperfect second language learning would become possible. Nichols (1998) mentions that in the earliest days of Indo-European expansion, lingua francas must have been spoken in Eurasia, and that language shifts frequently took place as well. These language shifts must have been rather slow (Nichols 1998: 239), however, and spread at a speed more like that of Quechua in Bolivia than the rapid changes with simplifying effects as in e.g. Nubi (cf. Kusters 2003). In more recent times, means for rapid expansion and far-reaching control have further developed.

## **6. Posthistoric sociolinguistics**

The complicated question remains whether in the future processes of simplification are still likely to occur. As Van Oostendorp (2002: 10) writes: “If you want to predict the linguistic future, you have to predict the whole future of the globalising world.” Nevertheless, I will make some speculations about future scenarios and their ramifications for simplification and complication.

In prehistory the first of the three conditions for simplification above was problematic. In the last two hundred years, and probably also in the future, the last condition has become problematic (contact between the center of spread and the new domain must not be too intensive). In pre-industrial empires the periphery was often only loosely connected to the center. Therefore, Arabic as spoken in Morocco, or Quechua in Ecuador could diverge from the varieties spoken in the centres of spread, and other norms could develop in the new peripheral varieties. In more recent times, however, be-

cause of technological innovations like wireless broadcasting and communication, trains, cars and aeroplanes, and improvement of infrastructure, all regions on earth are tightly interlocked with each other on a scale previously unprecedented.<sup>1</sup> The interdependence of mankind has also increased and people are linked up to each other over large distances, both economically, socially, politically and culturally. This process is called *globalisation* and is usually considered to consist of 1) a tendency to shift the center of power from national states to multinational and transnational organisations, and 2) technological development that makes transnational communication and organisation of production smoother (cf. von Weizsäcker et al. 2002: 8). If globalisation proceeds in the future, simplification processes, as far as these depend on the isolation of the periphery from the center, are less likely to occur, and extreme kinds of simplification, like in Nubi, will be unlikely.<sup>2</sup>

However, it is possible that in the future language will still be used in two different modes, as a means of communication and as a symbol of identity. As a means of communication a language can undergo extensive simplification as in Kenyan Pidgin Swahili. However, we predict that a variety like Kenyan Pidgin Swahili, thanks to the extensive communication channels, will converge naturally towards Standard Swahili. Likewise, it is typical that although Standard Swahili has spread rapidly over the whole of Tanzania in the 20<sup>th</sup> century, new simplified varieties with the potential to last a long time, and develop their own characteristics, as in Katanga, have not arisen.

Nevertheless, the pressure on a language to simplify when it is used in its communicative function may remain. In some perceptions of history, people are considered to be striving towards rationality, more autonomy, and economic life improvement.<sup>3</sup> Human history should be progressing towards these values, which received their largest boost and articulation in

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<sup>1</sup> Of course, there are some outskirts like Papua New Guinea, and the Amazon basin. However, an overwhelmingly large majority of the world's population live in places that are very near to each other in distance measured in time.

<sup>2</sup> Moreover, from the perspective of globalisation, intense contact and levelling does not only prevent extreme simplification, but also pressures established smaller languages to vanish and to be replaced by major languages, especially by the one and only global language at the moment: English (cf. Crystal 1997; Van Oostendorp 2002).

<sup>3</sup> Such ideas, and the ideas in next paragraph, are both descriptive, by claiming that man and history are driven by certain forces and goals, as well as normative, since they also argue that man should act accordingly.

western Europe in the period of Enlightenment around 1800. In this perspective, traditions are held responsible for holding people back. Language and culture are viewed from an instrumental perspective: they are not part of individual identity. Instead, language and culture are used in the emancipation process towards more freedom, and when they block improvement, they should be altered. This perspective on language corresponds best to a view where people are encouraged to learn the language with which they have the highest chances to be economically successful. Languages in themselves have no special value for people, but only in combination with the non-linguistic gains they can eventually draw from them.<sup>4</sup> In this view language loss and language death are not regrettable events. In a period of globalisation when societies become more complex and interdependent, it would be advisable, according to this perspective, that people adapt to this economically beneficiary globalisation process, and learn the language(s) of progress. Simplification of these languages would be a good thing as long as it improves communication and makes human interactions easier. Complex irregularities are unnecessary in this view, and there is no reason, for example, to retain strong verbs in English as far as these make English less learnable. In the past this instrumental attitude towards language indeed simplified Norwegian in the period of the Hanseatic trade, perhaps also Quechua in Ecuador, and Swahili when it was spread by traders over East-Africa. If in the future this view becomes more popular, the dominance of simplified forms of English and other major languages can be expected.

From another perspective people also strive towards the improvement of their life conditions, but the articulation and meaning of 'improvement' is embedded in a culture and language that belongs essentially to human identity. This cultural and linguistic part of human identity should be evaluated in its own terms, and not judged from universal standards of efficiency alone. Human history would consist of transformations of and interaction between different cultures and societies, while there are no absolute standards of progress.

These two perspectives on history recur frequently in debates pertaining to history, social sciences and philosophy. With respect to linguistics, the

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<sup>4</sup> Strong proponents of language maintenance sometimes also stick to such arguments. For instance, Nettle and Romaine (2000) argue that a reason to protect languages from extinction is that threatened languages may contain valuable knowledge which humanity would lose when these languages die. In other words, it is not the language itself that should be protected, but the knowledge it contains. In fact, this also boils down to an instrumental view on language.

latter perspective is especially favoured by Herder and Von Humboldt in 19<sup>th</sup> century Romanticism. In this perspective, individual identity and well-being cannot be separated from the cultural and linguistic backgrounds of people. All languages are considered to be equal, not because of the benefits afforded to the user in other domains, but because of the plain fact that human dignity and identity find expression in one's language and culture. The influence of Romanticism on the maintenance of complex redundancies can be seen in Faroese. If this stream of thought had not existed in Faroese society, Faroese, at least in its archaic form, would probably have died and been replaced by Danish.

In the latter perspective short-term economic benefits resulting from giving up one's language do not compensate for long-term losses of parts of one's identity, and it is predicted that languages and non-communicative complexities are not so easily given up. On the contrary, as a reaction to globalisation people may stick to their language even more eagerly. Castells (1997: 52) writes: "...in a world submitted to cultural homogenization by the ideology of modernization and the power of global media, language, as the direct expression of culture, becomes the direct trench of cultural resistance, the last bastion of self-control, the refuge of identifiable meaning." We have seen examples of similar reactions in the past in the subsistence of Cuzco Quechua and Icelandic in periods of domination from outside. In addition, not only minor languages, but perhaps also dominant lingua franca's may remain appreciated for their symbolic value. In summary, from this second perspective we expect even less simplification than in the former scenario and also more language diversity.

In conclusion, simplification processes were probably unknown in pre-historic times; modern history, however, has seen many instances of simplification, and the future will probably bring only minor modifications in order to use a language as a lingua franca. More severe simplifications that arise from restricted access are less likely. To what extent larger languages like English, Arabic, and Russian will be changed in their inflectional morphology depends on the vision on the function and place of language in culture and society held by future language users.

## **7. Conclusion**

From our present-day perspective language structures of two periods in time are largely hidden from view. This concerns first of all language spoken during the huge time-span of prehistory. Prehistoric language is quite diffi-

cult to examine, first of all because it was literally spoken in *pre*-history. Secondly, prehistory is stretched along such a huge amount of time that it can hardly be examined by the reconstructionist approach. Thirdly, it is highly problematic to generalise on the basis of variation and distribution patterns of today to situations in the past.

I have proposed an approach that takes environmental, that is, sociolinguistic or 'ecological', facts about language structure into account. Then we can make better informed hypotheses about the outlook of prehistoric language. We could examine how social structures condition and restrict possible and probable language variation today and extend these analyses to what we know about prehistoric modes of living. We found that it is likely that the measure of complexity in prehistory was higher than today, and that all kinds of lingua franca's and creoles, and processes like simplification, regularisation, dialect levelling and koineisation were not found as much as today.

The same approach can be used to another period of time that is still unknown, namely the future. When extending social and economic tendencies of the last couple of centuries into the future we may predict by the same method that the more extreme cases of simplification and reduction as in pidgins and creoles will not be as common as they have been the last hundreds of years.

Remarkably, the languages of our period in time may be less representative than is usually thought. Prehistoric and posthistoric language structures may have something in common: they both tend to be more complex than what is seen in historical times and today.

For further research on pre- and post-historic language we could look for language phenomena that are most sensitive to specific social environments. For instance complex idiosyncratic ways to divide nouns into classes or redundant though obligatory agreements may correlate with quite specific modes of living. When we further examine how language phenomena are distributed historically and synchronically among various kinds of speech communities, we could further apply these findings to language structures of time periods that are still largely unknown.

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# **Grammaticalization, constructions and the incremental development of language: Suggestions from the development of Degree Modifiers in English**

*Elizabeth Closs Traugott*

## **1. Introduction**

When we consider the topic of “language evolution: cognitive and cultural factors”, one of the first questions that come to mind is what is meant by “evolution”: language change over the course of recorded history (the time-span of which is very short, some seven thousand years only), or the biological and cognitive development of the language capacity in the species (the time-span of which is geological, possibly of over two hundred thousand years). As a historical linguist I will be discussing “language evolution” in the first sense, assuming that the historical record (all written until about a century and a half ago, when live recording began) reflects modern cognitive ability and a stable stage in the evolved human language capacity.<sup>1</sup> By hypothesis, millennia ago, there were earlier stages that had not evolved as far; possibly, millennia from now, it will evolve further, but neither the very distant past nor the future are accessible, and can only be the subject of speculation. Another question is which model of the human language capacity seems likely to be plausible in the pursuit of hypotheses about the origins and development of language in the biological sense.

In this paper I explore some aspects of the relationship between linguistic constructions and grammaticalization. I assume that language is fundamentally a symbolic system that pairs form and meaning. Since constructions as theoretical objects are designed to capture systematic associations between form and meaning, I assume that constructions, conceived in recent traditions of Construction Grammar (e.g., Goldberg 1995; Kay & Fillmore 1999),

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<sup>1</sup> When this modern capacity developed is controversial; so too is the extent to which its development was rapid or slow (see Wong 2005 for discussion). I assume here that the development was relatively gradual, without major saltations.

and especially Radical Construction Grammar (e.g., Croft 2001), form part, possibly all, of the building-blocks of grammar.<sup>2</sup> I also assume that grammaticalization, understood as the output of processes of language use that lead to systematic changes in morphosyntactic form and meaning (e.g., Traugott 2002; Hopper & Traugott 2003 [1993]), is a basic type of change that may lead to the reorganization of central aspects of language, both syntagmatic and paradigmatic.

The organization of the paper is as follows. I start with overviews of earlier characterizations of the relationship between constructions and grammaticalization (section 2), and outline some of the salient features of Construction Grammar and Radical Construction Grammar (section 3). I then provide a sketch of the development in English of the degree modifiers/quantifiers *a sort/lot/shred of* from [NP1 [of NP2]] > [[NP1 of] NP2] (section 4). In Section 5 I interpret these changes from the perspective of grammaticalization, and in Section 6 from that of Construction Grammar. In Section 7 some thoughts are presented on how the material discussed might be relevant to the larger topic of this volume.

## 2. Claims about “constructions” and grammaticalization in earlier work

Although many concepts germane to grammaticalization were articulated before Meillet,<sup>3</sup> he is thought to be the first to have used the term. He saw it as the result of reanalysis (in his view, the only way to innovate new grammatical material, a point to which I will return in section 7). He considered “lexical items” to be the source of most instances of grammaticalization, but also included word order, and lexical items in context of phrases. For example he discussed the function of French *suis* ‘I am’ in the context of a phrase such as *chez moi* ‘at home’ rather than of a lexical item such as *allé* ‘went’ (1958 [1912]: 131). Such contexts have often been called “constructions” in the literature on grammaticalization, and have been seen as the source as well as the outcome of grammaticalization, e.g., Givón (1979), Bybee, Perkins & Pagliuca (1994), Heine (2003), Hopper & Traugott (2003 [1993]), Traugott (2003), and Himmelmann (2004). Representative of this

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<sup>2</sup> For overviews of major features of various traditions of Construction Grammar, see Croft & Cruse (2004), Fried & Östman (2004).

<sup>3</sup> For short histories of grammaticalization, see Lehmann (1995 [1982]), Heine (2003).

perspective are the following two quotations from Bybee, Perkins & Pagliuca's study of the development of tense, modality, and aspect in the languages of the world:

- (1) a. Reduced to its essentials, grammaticalization theory begins with the observation that grammatical morphemes develop gradually out of lexical morphemes or combinations of lexical morphemes with lexical or grammatical morphemes (Bybee, Perkins & Pagliuca 1994: 4).
- b. It is the entire construction, and not simply the lexical meaning of the stem, which is the precursor, and hence the source, of the grammatical meaning (Ibid.: 11).

However, it is not always clear that "construction" in these works means much more than "syntactic string", or "string in morphosyntactic context". For example, Lehmann's work in the development of adpositions from prepositional and postpositional phrases, and of auxiliaries from main verbs, etc., led him to conclude that lexical items alone do not grammaticalize. They do so only in specific contexts, e.g., case markers derive from nouns, classifiers from numerals only under certain specifiable linguistic conditions:

- (2) [G]rammaticalization does not merely seize a word or morpheme ... but the whole construction formed by the syntagmatic relations of the elements in question. (Lehmann 1992: 406)

More recently, Himmelmann has said:

- (3) [I]t is the grammaticizing element *in its syntagmatic context* which is grammaticized. That is, the unit to which grammaticization properly applies are [sic] *constructions*, not isolated lexical items. (Himmelmann 2004: 31; italics original)

Despite this apparent focus on syntagmatic strings, it has also been clear that grammaticalization is multilayered, and involves a number of correlated changes. In Lehmann's view these are structural (identified as six "parameters": three paradigmatic constraints, called "integrity", "paradigmaticity", and "paradigmatic variability", and three syntagmatic constraints, called "structural scope", "bondedness", and "syntagmatic variability"). But he has not limited the correlations to these structural constraints:

- (4) A number of semantic, syntactic and phonological processes interact in grammaticalization of morphemes and of whole constructions.  
(Lehmann 1995 [1982]: viii)

Bybee, Perkins & Pagliuca see this interaction as so tightly correlated that they have developed the hypothesis of “coevolution” of morphosyntax, semantics and morphophonology:

- (5) Our hypothesis is that the development of grammatical material is characterized by the dynamic coevolution of meaning and form.  
(Bybee, Perkins & Pagliuca 1994: 20)

Another approach to multi-layeredness is Himmelmann’s suggestion that grammaticalization is characterized by three types of expansion (2004: 32–33):

- (6) a. “Host-class expansion”: a grammaticalizing form will increase its range of collocations with members of the relevant part of speech (noun, adjective, verb, or adverb). This is increase in type-frequency, i.e. productivity.  
b. “Syntactic expansion”: this involves extension to larger contexts, e.g., from core argument positions (such as subject and object) to adpositions (such as directional and temporal phrases).  
c. “Semantic-pragmatic expansion”: a grammaticalizing form will develop new polysemies in pragmatic or semantic contexts.

According to Himmelmann, in grammaticalization all three contexts expand (but not necessarily together); by contrast, in lexicalization the first does not, while syntactic or semantic-pragmatic contexts may stay the same, expand, or narrow.

The importance of pragmatic and semantic environments for morpho-syntactic change are highlighted in e.g., Traugott & König (1991) and Heine (2003). As Heine has said:

- (7) [S]ince linguistic items require specific contexts and constructions to undergo grammaticalization, grammaticalization theory is also concerned with the pragmatic and morphosyntactic environment in which this process [shift from lexical to functional categories ECT] occurs.  
(Heine 2003: 575)

As I show in the next section, it is this kind of perspective on grammaticalization that underlies Croft's (2001) theory of Radical Construction Grammar.

### 3. Construction Grammar approaches

Construction Grammar is one of the few theories of grammar that builds correlations directly into the model, and so it is useful to consider its relevance to the multilayered views of grammaticalization outlined above. It is a cognitive, holistic, and (in most proponents' view) usage-based framework (Fried & Östman 2004: 23–24), in other words, no one level of grammar is autonomous, or “core”. Rather, semantics, morphosyntax, and phonology, and, in some models, pragmatics, work together in a construction:

- (8) Constructions in C[onstruction] G[rammar] are multidimensional objects that represent generalizations about speakers' linguistic knowledge. As such, they allow for both the gestalt, holistic view of linguistic patterning (unlike formal theories of language) and for keeping track of the internal properties of larger patterns (like any other grammatical theory). (Fried, forthcoming)

Accounts by Goldberg (1995) and Kay & Fillmore (1999) focus on parts of the grammar, especially those that show idiosyncracies:

- (9) Any linguistic pattern is recognized as a construction as long as some aspect of its form or function is not strictly predictable from its component parts or from other constructions recognized to exist. (Goldberg 2006: xx)

This work is for the most part conducted with the objective of showing that lexical items have relatively indeterminate meaning, and rather than being polysemous, they acquire their meaning from a construction in systematically related ways (Goldberg 1995: 159). As a consequence it is hypothesized that the construction imposes a meaning, and under the right implicit circumstances “coerces” interpretations (Goldberg 1995: 159; Francis & Michaelis 2002; Michaelis 2004). Examples are drawn extensively from “mismatches” such as *She had a beer*, where *beer* is used as a mass, not count noun.

Wiemer & Bisang (2004: 9) comment that Goldberg's and Fillmore & Kay's approaches are synchronic, concerned with idiosyncracies, and syn-



tactic in orientation (where “syntax” includes argument structure and grammatical relations), and therefore their approach may be less than optimal for diachronic work on grammaticalization. By contrast, Croft (2001) highlights the Construction Grammar perspective that ultimately all of linguistic structure is constructional. In his version, Radical Construction Grammar, constructions are symbolic units conceived as in Figure 1:

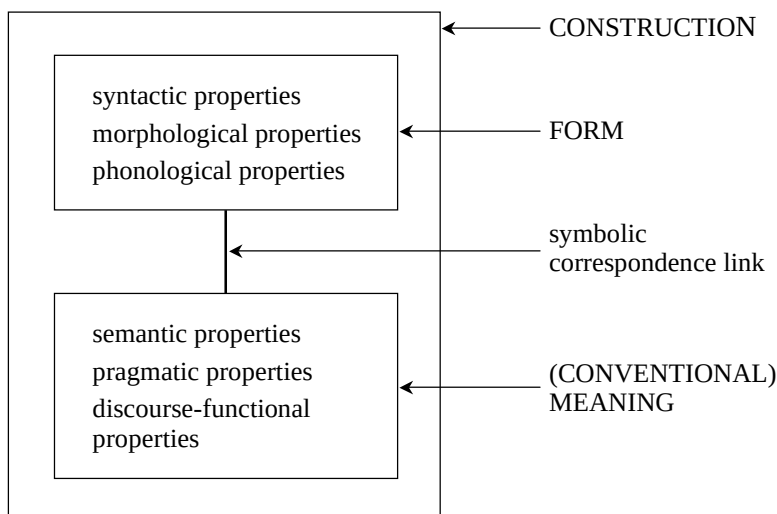


Figure 1. Model of the symbolic structure of a construction in Radical Construction Grammar (Croft 2001: 18; Croft & Cruse 2004: 258)

According to Croft, the link between form and conventional meaning is construed in semantic terms, and as internal to the construction. This model has been built to account in part for cross-linguistic typological variation, and in part for grammaticalization. It therefore seems especially well suited for developing a more articulated approach to the correlations between grammaticalization and constructions than has been developed to date.<sup>4</sup>

While Croft does not give any detailed examples of grammaticalization within his framework, he puts forward some general observations, including:

<sup>4</sup> Owing to limitations of space, I will not go into the significant differences between Radical Construction Grammar and other varieties of Construction Grammar (see Croft & Cruse 2004: 283–289). What is important here is the six-layered construction model.

- (10) a. In the grammaticalization process, the construction as a whole changes meaning. (Croft 2001: 261)
- b. [T]he [new] construction is polysemous with respect to its original meaning ... the new construction undergoes shifts in grammatical structure and behavior in keeping with its new function. (Ibid.: 127)
- c. Extension of constructions to new uses is a change in the distribution of that construction, and such changes are theorized to follow connected paths in conceptual space. (Ibid.: 130)

(10a) and (10b) are reminiscent of (5) above on coevolution of form and meaning. (10c) is a crucial reminder that grammaticalization is hypothesized to occur in small, local steps (known as the “gradualness” hypothesis).

Heine and his colleagues have interpreted plausible local steps, particularly from a conceptual point of view, as “grammaticalization chains” (see Heine 1997). The steps are fine-grained structural differences that are constrained by plausible structural shifts along a continuum via morpho-syntactic reanalysis and analogy. Very briefly, reanalysis involves change in constituency, hierarchical structure, category, grammatical relations or boundary types (Harris & Campbell 1995); it involves new structural configurations for old material.<sup>5</sup> By contrast, analogy involves attraction of extant forms to already existing structures, hence generalization. Naturally, many reanalyses result from attraction to existing structures, and evidence for reanalysis derives from generalization to new contexts, so reanalysis and analogy often work together (Hopper & Traugott 2003 [1993]: ch. 3, Andersen 2006). In some cases they may be hard to distinguish – each analogical change is after all a reanalysis of the former string (Tabor 1994, and, from a different perspective, Kiparsky forthcoming). These local steps of grammaticalization also involve equally fine-grained and constrained semantic and pragmatic shifts, sometimes with indeterminate intermediary stages, whether metonymic (e.g., Traugott & König 1991) or metaphorical (e.g., Heine, Claudi, Hünemeyer 1991). Such semantic and pragmatic shifts may themselves be conceptualized in terms of semantic and pragmatic reanalysis and analogy (Anttila 1992).

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<sup>5</sup> Andersen (in press) provides a broader view of reanalysis, including shifts in individual speaker-hearers’ competence as changes are adopted by the community. Only structural reanalysis will be considered here

#### 4. Some examples: the development of three degree modifier constructions

English and most European languages have a large number of expressions with the pattern NP1 of NP2. This syntactic string has a wide array of functions, among them locative *the back of the house*, partitive *a piece of the plate*, approximative *(a) sort of a frog*, emotionally charged epithet *an idiot of a teacher*, subjective genitive *the singing of the diva*, and objective genitive *a portrait of a hunter*. NP1 of NP2 strings therefore participate in a number of different constructions, where syntax, meaning, and pragmatic function can be construed at various degrees of granularity ranging from general classes to individual idiosyncratic combinations which differ with respect to the kind of noun or determiner that can realize either NP, or with respect to the pragmatics associated with the string.

As a generalization, we may use an abstract string of the type illustrated by *a piece of the plate*:

(11) [NP1 [of NP2]]

This construction is binominal. Here NP1 is the head, the part that is perspectivized as in the foreground. *A piece of the plate* is about the piece (where it was found, whether it can be glued back, etc.). This is a “partitive” construction, since the semantic relationship between NP1 to NP2 is that of part/member to whole (NP1 can usually be substituted by a phrase such as *part*). However, not all constructions to be discussed have the same semantics strictly speaking. For example, *sort of* (= ‘subtype’) in *a (special) sort of rose* is in a taxonomic (‘kind-of’) rather than partitive (‘part-of’) relationship to NP2 (see Croft & Cruse 2004: ch. 6).<sup>6</sup>

The string in (11) is contrasted with that in (12), where the relationship between NP1 and NP2 is different, specifically NP2 is the head, as in *a sort of a frog*:

(12) [[NP1 of] NP2]

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<sup>6</sup> In a more fine-grained analysis, partitive and taxonomic, degree modifier and quantifier uses would probably be considered separate subtypes of construction. It should be noted that the partitive discussed here involves an alienable relationship. Inalienable partitives such as body-part relationships have a very different history, typically becoming case markers (Heine 1997).

Here NP1 functions as a modifier, often a quantifier, substitutable by a degree phrase like *more or less*, *somewhat*, and NP2 is perspectivized as in the foreground, as in *A sort of a frog* (= ‘frog-like thing’) *jumped out at me*, *He is a bit of a liar*.<sup>7</sup> Pragmatically such phrases cast doubt on the accuracy of the description ‘frog’ or ‘liar’. Such doubt may be a classificatory one as in the first instance, or a social one regarding whether the interlocutor will accept the description. Criteria for distinguishing (11) and (12) include agreement patterns: in (11) the determiner in NP1 agrees with N1 only, but in (12) it can agree with N2, cf. *these sorts of skill* (11) vs. *these sort of skills* (12) (colloquial). Furthermore, in (11) NP2 can be preposed, but in (12) it cannot, cf. *Of frogs this kind/sort is becoming extinct* (partitive) but *\*Of wine this is a lot* (degree modifier) (see Denison 2002).

A small subset of such constructions have been investigated from a historical perspective, most notably (a) *kind of N*, (a) *sort of N* (e.g., Tabor 1993; Denison 2002, 2005), *a bit (of) N*, *a shred (of) N* (e.g., Traugott, forthcoming b). At some point in their histories they all participated in the Partitive Construction and at a later point also in the Degree Modifier Construction.

The change can be schematized as:

- |      |                |   |                |
|------|----------------|---|----------------|
| (13) | [NP1 [of NP2]] | > | [[NP1 of] NP2] |
|      | Head = NP1     | > | Head = NP2     |
|      | NP1 + Mod      | > | Mod + NP2      |

(13) is a standard type of grammaticalization, as shown by the rebracketing and functional shift of NP1 (in some cases accompanied by phonological reduction (*kinda*, *sorta*). *Kind of*, *sort of*, and *a lot* can be used as free adjuncts as in *I like it a lot*, but *a shred (of)* cannot. Two of the strings, *a bit of N* and notably *a shred of N* have come to be used largely in negative polarity contexts (negative, interrogative, conditional, etc., see Israel 2004).

Here I briefly outline the history of *a sort of*, *a lot of*, and *a shred of*. The changes are considerably more complex than suggested here, but must be abstracted away from for limitations of space. My purpose is two-fold. One is to illustrate aspects of grammaticalization, notably changes in the relationship between the two NPs, and the locality of structural and semantic changes. The second is to consider ways in which work on grammati-

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<sup>7</sup> Sadock (1990) and Yuasa & Francis (2002) discuss the special properties of quantificational nouns used as modifiers.

calization and Construction Grammar can together help capture the various changes involved.

#### 4.1. (a) sort of NP

The noun *sort* was initially, at Step I, borrowed from French in the fourteenth century in the meaning of ‘group’ or ‘set’, i.e. as the superordinate term (see also *kind*, *manner*, *type*, Denison 2002, 2005), as in (14):

##### (14) Step I, pre-partitive use

Well may [h]e be called valyaunte and full of proues that hath *such a sorte of noble knyghtes* unto hys kynne  
 ‘Well may he be called valiant and full of prowess that has such a group of noble knights among his kin.’  
 (a1470 Malory, *Works* 526/21 [MED, *sort* 1a])

In (14) *sorte* is a set or group consisting of knights. However, in the sixteenth century we find *sort* with meanings that suggest it denotes member of a set (i.e. partitive), rather than the superordinate set, especially when preceded by a quantifier (15a). There has been a semantic reversal from the term for a superordinate set to that for a member of the set.<sup>8</sup> This crucial step aligns the string with many other NP of NP strings; without it the subsequent changes could not have occurred, so it merits special note.

##### (15) Step II, partitive use

- a. The countrie aboundeth with *all sort of corne, flesh, and fruit*  
 (1594 Ashley tr. *Loys de Roy* 10b [OED, *sort* n2, 6d])
- b. He’s *a sort of a* prentice, but he’s not fastened (= ‘bound by contract’).  
 (1632 Lithgow *Trav.* VIII. 353 [OED, *fasten*, v.]

When both NP1 and NP2 have indefinite articles, as in (15b), the indefiniteness may trigger the inference that because the class membership is not

<sup>8</sup> A crucial factor contributing to the shift under discussion is the changing status of the preposition *of*, which in Old English meant ‘out of’, but during Middle English came to be the default preposition (Denison 2002). Another crucial factor was the development of the indefinite article, another Middle English phenomenon.

uniquely identifiable, it is not exact. In this kind of context NP2 came to be reanalyzed as the head and *a sort of* became a degree modifier conveying the speaker's assessment that the entity referred to is not an adequate or prototypical exemplar of NP2 (16a), or that NP2 is not an exactly appropriate expression (16b). In other words, it has a partially epistemic meaning. It can therefore sometimes be used as a hedge to save face if NP2 might be negatively evaluated by the addressee, as in (16b):

(16) Step III, degree modifier use

- a. I do think him but *a sort of a*, kind of a ,... *sort of a* Gentleman  
(1720 Shadwell, *Hasty Wedding* II. iv [OED, *sort* n2, 8b])
- b. I am a kind of a what d'ye call 'um *a Sort of a* Here-and-thereian;  
I am Stranger no where (1701 Cibber, *Love makes Man* IV. iv  
[OED, *here* 9d])

Over time, the use of an indefinite article with N2 declined for both the partitive and degree modifier uses. The development of the degree modifier use is particularly apparent when (*a*) *sort of* is extended (like other degree adverbials such as *very*) in the form *sort of* to verb (17a) and adjective (17b, c) contexts (early examples are dialectal):

- (17) a. It *sort o'* stirs one up to hear about old times  
(1833 Hall, *Legends West* 50 [OED, *sort* n2, 8c; Denison 2002])
- b. I bees *a sorter* courted, and *a sorter* not  
(1839 Marryat *Diary Amer.* Ser. I. II. 218 [Tabor 1993])
- c. One is *sort of* bewildered in attempting to discover ...  
(1858 Pirie, *Inq. Hum. Mind* i. 10 [Ibid.])

As (17) illustrates, in the Degree Modifier Construction *sort of* loses its compositionality as a NP: *a* is dropped before N1, and *of* loses its prepositional function, eventually becoming cliticized to *sort* as *-a* (*sorta*, see also *kinda*). It has the status of an adverb (cf. *rather*, *quite*).

A further step is the use of the degree modifier as a free adjunct that can be used without NP2 as in (18a), often in an elliptical tag, and in the twentieth century as an independent response (a downtoning agreement marker, as in (18b)):

## (18) Step IV, free degree adjunct use

- a. He chalked me down like a fool, me and Tom Staples; being old friends, or *sort of*  
(1835 Bird, *Hawks of Hawk hollow* II. viii. 78 [OED *chalk*, v.]
- b. 'Friend of this hombre?' 'Yes; *sort of*'  
(1918 Mulford *Man fr. Bar-20* viii. 79 [OED, *hombre*])

All uses of (*a*) *sort* in NP1 of NP2s strings, except those at step I, continue to be available in Present Day English.

## 4.2. A lot of NP

*A lot of* (and its congeners *a bit/bunch of*) has many similarities to *a sort/kind of* in its development, but there are also differences. Initially *lot* referred to an object (usually made of wood) by which persons were selected, e.g., for office, with appeal to God or chance (see *throw a lot, lottery*), or, by metonymy, to the share that falls to a person by chance or inheritance (19) (see *allotment, lot* 'parcel of land'). Here it is a limited partitive in the sense that it can only be used with NP2s that denote concrete inanimate objects. Like *a bunch*, but unlike *a bit*, it appears always to have been used with mass, plural, or collective NPs:

## (19) Step I, limited partitive use

- On Fearn's felda ge byrað twega manna *hlot* landes in to Sudwellan  
'In Fearn's fields you extend two men's parcel of-land in to Southwell'  
(= '... you extend a parcel/share of land large enough for two men ...')  
(958 *Grant in Birch Cartul. Sax.* III. 230 [OED, *lot* 2a])

In one early Middle English text, *Orrmulum*, there are several occurrences of *a lot of* being used to mean 'part', or 'group' of things or persons forming part of a larger whole (see OED and MED), one of which is cited in (20a). Examples in other texts occur considerably later, usually with reference to groups of items for sale (20b, c). Although the changes are minor (host-class expansion to animates and groups), they are an essential step toward the development of the degree modifier/quantifier uses, and can be considered a separate stage (still partitive, but expanded):

(20) Step II, expanded partitive use

- a. He ne wass nohht wurrpenn mann..Forr to forrwerppenn *anig lott Off Moysæsess lare*  
 'He not was not become man for to overthrow any part of Moses' teaching' (= 'He did not become incarnate to overthrow any part of Moses' teaching') (c1200 *Orm* 15186 [MED, *lot* 2c])
- b. Mrs. *Furnish* at St. *James's* has order'd *Lots of Fans*, and China, and India Pictures to be set by for her, 'till she can borrow *Mony* to pay for 'em.  
 (1708 Baker, *Fine Lady's Airs* [LION, English Prose Drama])
- c. Nothing could make amusing Mr. O'Rouke's method of setting out crocus bulbs. Mr. Bilkins had received *a lot of a* very choice variety from Boston. (after 1860, Aldrich, *Marjorie Daw* [UVa])

In this use of *a lot of* NP2 may refer to a conceptually bounded entity or group (a limited amount) as in (20a) or a group of considerable (but unbounded) size, as in (20b, c). This is particularly clear when *lot* is plural as in (20b). In such unbounded contexts, *a lot/lots of* invites the inference of large quantity.

By the nineteenth century we find examples in which *a lot of* functions as a quantifier meaning 'many/much'. At first this meaning is associated with the represented speech of servants, rustic or comic characters (21a, b), but by the mid-nineteenth century it is more widely used. However, in contemporary grammars it is still considered "informal" (Quirk et al. 1985) or "characteristic of casual speech" (Biber et al. 1999). As a quantifier it can be modified by the degree modifier *quite* (21c); this appears to be an early twentieth century development. Unlike the approximator degree modifiers *sort/kind of*, adverbial *a lot* was used in only very limited ways with adjectives: it occurs with comparative adjectives from about 1900 on (21d), but not bare adjectives (*sort of/\*lot lumpy*) or verbs (*sort of/\*a lot ran*):

(21) Step III, degree modifier use

- a. Sir, there's *a lot of* folks below axing for – are you a Manager, Sir?  
 (1818 Peake, *Amateurs and Actors* [LION, English Prose Drama])
- b. Learning at bottom, physic at top! / *Lots of* business, *lots of* fun, / Jack of all trades, master of none!  
 (1833 Daniel, *Sworn at Highgate* [Ibid.])



- c. the moon had risen and was letting *quite a lot of* light into the bank  
(1902 Hornung, *The Amateur Cracksman* [UVa])
- d. "If Marilla wasn't so stingy with her jam I believe I'd grow *a lot*  
faster." (1909 Montgomery, *Anne of Avonlea* [UVa])

Like *sort/kind of, a lot* (without *of*) came to be used as a free adjunct, at first through ellipsis (in (22a) we understand *a lot of the hillside*). This free adjunct came to be used as an independent response (22b).

(22) Step IV, free degree adjunct use

- a. My house faces east and is built up against a side-hill, or should I  
say hillside? Anyway, they had to excavate *quite a lot*.  
(1847, Stewart, *Letters of a Woman Homesteader* [UVa])
- b. "How many are there?" "Oh, *a lot*, perhaps a hundred."  
(1900 Wharton, *The Touchstone* [UVa])

All uses of *a lot of*, including 'parcel of land' at step I, continue to be available in Present Day English.

4.3. (Not) a shred of NP

My final example is *a shred of*. Again, this phrase (and its congeners *a drop/jot of*) has many similarities to those discussed above, but there are also differences. Although ultimately related to *shear* (sheep), in Old English it meant a "fragment cut or broken off from fruit, vegetable, textile, coin, vessel" (OED) and in Middle English was generalized to bodies (the religiously symbolic Host, humans and animals) (23a). From its beginning it was, like *lot*, available in a partitive construction, but in this case the NP2s with which it could be used were limited almost exclusively to concrete count nouns:

(23) Step I, limited partitive use

- a. With strengthe of his blast / The white [dragon] brent than rede, /  
That *of him* nas founden *a schrede* / Bot dust  
'With the strength of his blast, the white dragon burned the red, so  
that of him (the red) not a shred was found, only dust' (c1300  
*Arthur & Merlin* 1540 [MED, *shrede a*]); note *of him* is preposed)

- b. hee is paid for his workemanship, vnlesse by misfortune his shieres  
slipp away, and then his vailes is but *a shred of* homespunne cloth  
'He is paid for his workmanship, unless by misfortune his shears  
slip away, and then his profit is merely a shred of homespun cloth'  
(1592 Greene, *A quip for an upstart Courtier* [LION, EEBO])

In the sixteenth and seventeenth centuries *a shred of* was further generalized to NP2s including language, mankind, nature, all of them mass nouns, or used as mass nouns (24). In these contexts it came to be evaluated as a small, insufficient, inadequate part of NP2:

(24) Step II, expanded partitive use

- a. *A despis'd Shred of* mankind (1645 Daniel, Poems [OED, *shred* 6])
- b. [objecting to tyrants who] fix their talents on the poor / (As if a  
slave was not *a shred of* nature / Of the same common nature with  
his lord) (= tyrants treat slaves as if they were not part of nature)  
(1742 Blair, *Grave* 224 [OED, *shred* 6])

In the nineteenth century it was reanalyzed as a degree modifier/quantifier (not 'the (smallest) part of' but 'some/any'). The new head (NP2) is typically an abstract mass noun that is positively evaluated (*evidence, character, hope, reputation, credibility*, but not *fear, dishonor, falsehood*). By the twentieth century it is largely restricted to negative polarity contexts (25a, b), but does still occur in positive ones as well (25c):

(25) Step III, degree modifier use

- a. Loto has not *a shred of* beauty. She is a big, angular, raw-boned  
Normande, with a rough voice, and a villainous patois  
(1867 Ouida, *Under Two Flags* [LION, 19thC Fiction])
- b. You're so worthless, you can't even recognize *the shred of* petty  
virtues in others, some of which I still have  
(1965 Osborne, *A Patriot for Me*, III. v. [LION, 20thC Drama]; note  
the agreement mismatch between singular *shred* and plural *virtues*)
- c. A lot of lies and *a shred of* truth (2005 Heading, *NewsForge*  
[Google])

*A shred of* has not been extended to adjective or other syntactic contexts, nor has it become a free adjunct.

All uses of *a shred of* persist in Present Day English.

## 5. The developments from the perspective of grammaticalization

All three patterns are examples of grammaticalization in that they involve:

- a) Change from strings in which the NPs are free to occur with any determiner and in any number, to strings with significant constraints in this regard.
- b) Change from strings in which both NPs have literal, concrete meanings, to ones in which NP1 becomes far more abstract.
- c) Rebracketing (reversal of head relationships).
- d) Functional shift in which NP1 comes to serve a grammatical modifier function such as 'quite', 'somewhat'.
- e) Host-class expansion from concrete lexical heads to abstract ones.
- f) Syntactic expansion in that they become degree modifiers or quantifiers; in addition, (*a sort of* has been further expanded to pre-adjective and pre-verb contexts; *sort of* and *a lot* have been expanded to free adjunct status.
- g) Semantic-pragmatic expansion in that there is a shift to status as approximator (*a sort of* or degree modifier/quantifier *a lot (of)*, (*not*) *a shred of*.
- i) Coexistence of older and newer meanings and uses (a phenomenon commonly called "layering" in the grammaticalization literature, see Hopper 1991). In other words, there has been systematic polysemy within each of the NP of NP strings, at least for a time. Doubtless *a lot* that is for sale is not currently thought to be polysemous with *a lot* in *a lot of land* (= 'much land'), but at the time when the degree modifier meanings were coming into existence, speakers must have been aware of such polysemies.
- j) Renewal of already extant categories. From earliest Old English there have been degree modifiers (cf. *swiðe* 'very') and free degree adjuncts used as responses (cf. *gea* 'yes'). Over the history of English these categories, especially degree modifiers have been added to.
- j) Cross-linguistic replication. In so far as *a shred of* and other expressions referring to small amounts (*a drop of*, *a bit of*) share a tendency to be associated with negative polarity, they are reminiscent of similar changes in French, where *pas* 'step', *point* 'dot, point', *mie* 'crumb', *gote* 'drop', etc. came to be understood as reinforcers of the negative (after competing with the others, (*ne*) *pas* became the default negative marker in French, a phenomenon widely known as the "Jespersen Cycle"; see Eckardt 2002 for an account of the semantics involved).

The changes may be summarized in the change-schema in (26). In (26) PrePart is short for the source NP of NP pattern, with pre-partitive (*a sort of*) or limited partitive uses (*a lot of*, *a shred of*); Part for extended partitive, DegMod for degree modifier, DegAdv for degree adverb, and DegAdjct for free degree adjunct uses.

(26) PrePart > Part > DegMod > DegAdv > DegAdjct

Change schemas such as (26) are overarching macro-abstractions, “paths” of change that can be observed over considerable lengths of time and across large quantities of data. They are linguists’ extrapolations: hypotheses about the likely development of particular form-meaning pairs over time, given ways in which speakers and hearers strategize interaction (Andersen 2001, 2006, in press). The particular sets of changes that the individual strings and their congeners have undergone are themselves abstract sub-types of change, while the individual textual representations are tokens of those changes.

When we study particular subtypes of change from the perspective of macro-types or schemas, we are likely to find that any particular subtype has undergone only partial change. It may be in progress, or its development may have been arrested, because people stopped using it (e.g., *manner of* ‘method of’ came to have partitive meaning (1876 *all other manner of fools* [OED, *republican* B. n, 2a]) and even some aspects of degree modifier use (surviving in the fixed phrase *in a manner of speaking*), but ceased to be used productively in either function (Denison 2002). In other words, changes do not have to “go to completion”; they do not even have to occur (Hopper & Traugott 2003 [1993]: 131).

## 6. From the perspective of Construction Grammar

If we consider the three examples of grammaticalization from the perspective of Construction Grammar, the most striking observation is that we can no longer focus on strings, which represent form-meaning pairs, but overtly reference only one member of the pair, but rather must consciously consider form and meaning together. More specifically, from the perspective of Radical Construction Grammar, we must keep all of the six correlated factors in Figure 1 in focus as we conduct our analysis.

Another striking observation is that, despite such apparent differences, there are significant overt similarities with respect to levels of analysis. In work on grammaticalization we distinguish between:

- schemas, the macro-structures like (26) that are the overarching frame within which particular changes can be described,
- generalized change-types: sets of similarly-behaving strings, e.g., *(a) sort of*, *(a) kind of* as a subset distinct from the subset *a lot (of)*, *a bunch (of)*, *a bit (of)* as distinct from the subset *(not) a shred of*, *(not) a jot (of)*, etc.
- specific change-types, such as *(a) sort of*, *a lot (of)*, and *(not) a shred of*,
- the empirically attested tokens, which are the locus of change.

Likewise, in Radical Construction Grammar (and other types of Construction Grammar) we can distinguish between what I will call:

- macro-constructions: meaning-form pairings that are defined by structure and function, e.g., Partitive, or Degree Modifier Constructions, etc.,
- meso-constructions: sets of similarly-behaving specific constructions,
- micro-constructions: individual construction-types,
- constructs: the empirically attested tokens, which are the locus of change.

Neither in the case of grammaticalization nor of Construction Grammar in its various versions, is the hierarchy given here restricted to the first three levels. More elaborate levels of generalization may be relevant in some instances, in others, fewer.<sup>9</sup>

With these two factors in mind we can model the changes in 4.1 – 4.3 as in Figure 2.

The columns identify macro-constructions, and the rows represent individual constructions. Meso-constructions are not included. Note that for convenience the same labels are used as in (26). However, in (26) they are used for patterns that are essentially form-meaning pairs (where morpho-syntactic form and meaning are crucial), while in Figure 2 they are used for constructions in which form and meaning each have three sub-levels (for a total of six correlated levels).

Figure 2 provides concrete, visual support for the claim cited in (1b) that “It is the entire construction, and not simply the lexical meaning of the stem, which is the precursor, and hence the source, of the grammatical meaning” (Bybee, Perkins & Pagliuca 1994: 11). It also reveals at a glance

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<sup>9</sup> The abstract lower and higher levels are related by “inheritance hierarchies” (Fried & Östman 2004); these are not of direct concern here and will not be discussed.

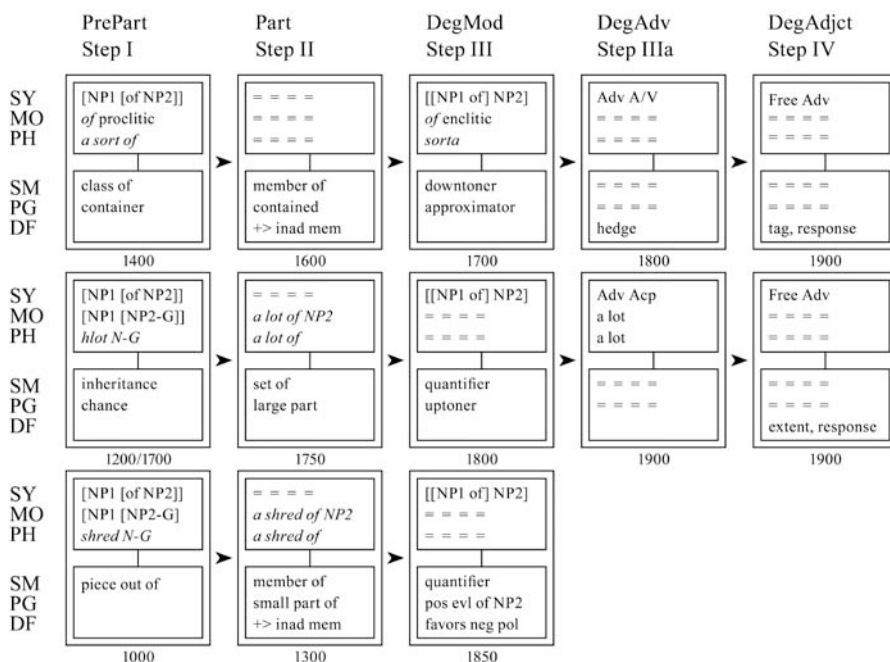


Figure 2. Model of the development of a sort of, a lot of, a shred of

|         |                    |            |                                 |
|---------|--------------------|------------|---------------------------------|
| Key: SY | syntax             | '= = ='    | 'carries over from prior stage' |
| MO      | morphology         | +>         | 'implies'                       |
| PH      | phonology          | 'inad mem' | 'inadequate member'             |
| SM      | semantics          | 'pos evl'  | 'positive evaluation'           |
| PG      | pragmatics         | 'neg pol'  | 'negative polarity'             |
| DF      | discourse function | N          | noun                            |
| G       | genitive affix     | Acp        | comparative adjective           |

that each of the constructions, despite similarities, has considerable local differences. For example, syntax may stay constant from Step I to II (*sort of*), and phonology from Steps II to III (*a shred of*). There is therefore no necessary “coevolution of form and meaning” (Bybee, Perkins & Pagliuca 1994: 4, quoted in (5)).<sup>10</sup> However, the more restrictive statement found in

<sup>10</sup> For earlier arguments against the necessity of coevolution of form and meaning, see Bisang (2004). We should also beware of statements like “grammaticalization ... seize[s] the whole construction” (Lehmann 1992: 406, quoted in (2)); Lehmann clearly did not intend “construction” in the sense developed by Croft, but rather “string in context”.

Bybee & Dahl (1989: 68) that coevolution occurs in the development of affixation may still be valid.

One of the aspects of Construction Grammar approaches that has drawn much attention is the hypothesis that a construction imposes meaning (see (10b)). As we have seen, the macro-constructions are highly abstract schemas, and it is not here that such attraction operates, except in a very general, largely morphosyntactic, way (e.g., allowing agreement mismatches of the type found in *these sort of skills*). Rather, to the extent that semantic attraction occurs, it does so at the meso-level, but even here each individual construction maintains not only its own idiosyncracies with respect to structure (e.g., constraints on the use of indefinite *a*), but also with respect to the particular lexical Ns that can occur as NP2 (or adjectives and verb, if these are available).<sup>11</sup>

By hypothesis, speakers and hearers match parts of constructs (tokens) based on one construction (e.g. Partitive) to parts of a different (e.g. Degree Modifier) construction, given pragmatic and other contexts that make such a match plausible. If the construct is innovatively matched to a construction of which it would not traditionally be an instance and the innovation is replicated, it may be conventionalized by other speakers as a micro-construction. Stronger integration with the multiple layers of a construction-type may occur, leading to alignment with meso-construction-types and ultimately with hierarchically high-level functions (similarly Fried, forthcoming).

## 7. Some implications

In this section I raise a few issues pertaining to grammaticalization and constructions that may be relevant for the larger theme of this volume: the relationship of language to evolution and to culture.

It seems plausible that constructions could be the building-blocks out of which language could have evolved biologically. Being form-meaning correlates, they can be seen as the manifestations of the ability associated with modern human society to store symbols outside the human brain (whether as tools, drawings, language or other symbolic expressions). Constructions

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<sup>11</sup> Geeraerts, Speelman & Tummers (2005), citing a debt to concepts in Gries & Stefanowitsch (2004), are developing a way to give an account of the measure of the strength of association between a schematic slot in a construction and the lexical fillers of that slot ("collostructions").

are abstract schemas and do not spell everything out in an efficiently computational way but rather leave room for generalizations, realignments, and negotiated interaction, hence change (see Trousdale, forthcoming, on emergent and grammaticalizing constructions). In Andor (2004) Chomsky suggested that if language were designed for communication, then everything would be spelled out and nothing would be hinted at. This assumes that communication is transfer of information. But if instead we view communication as negotiated interaction, then hints are optimal. Each participant in the communicative event is given some leeway that allows for saving face (one's own or others'), and creative thinking beyond the immediate situation.

In Construction Grammar analogy has been an important factor. Attractor sets can be construed as analogical templates. It may be that "Humans are simply analogical animals" (Anttila 2003: 438), and that much language change involves the emergence of family resemblances, but where the families come from is an important issue, as is the way in which new membership in a family comes about. This is where change becomes relevant, including the kinds of changes discussed here, in which strings are reanalyzed (e.g., partitive > degree modifier uses), and analogized to other strings within a construction (e.g., *sort of* to *kind of*).

While work on Construction Grammar has been focused on analogy, work on grammaticalization has been focused extensively on reanalysis. As indicated at the beginning, Meillet conceived of reanalysis as the unique way by which new structures could be developed; he contrasted it with analogy, which he understood as simply expanding the instances of an already extant structure. Since then grammaticalization has been widely associated with reanalysis, although to what extent reanalysis is coterminous with grammaticalization is a matter of significant debate (see Haspelmath 1998, and a summary of issues in Hopper & Traugott 2003 [1993]: ch. 3). In the early days of work on generative historical syntax, Lightfoot (1979) sought to focus attention on "catastrophic" reanalyses (one of his examples was the development of syntactic auxiliaries in English, along with special rules for placement of *not*, inversion in interrogatives, and presence or absence of "dummy *do*"). In constructional terms, one might think of such changes as occurring at the level of macro-constructions. However, it soon became clear that an alternative way of thinking about this change was that rather than being a new syntactic category, auxiliaries were in fact relics of older word orders, and a series of very small, local steps, along with the spread of verb-object word order which had always existed in English, led to a significant reorganization of the verb system in English (Kroch 1989).



One could say, then, that in this case the changes emerge out of use of constructs (tokens) patterned on particular constructions. This would be exemplar-based analogy (the occurrence of which requires very local reanalysis).

From this perspective, Construction Grammar and work on grammaticalization complement each other, and provide ways to understand “local reanalysis”. As Andersen points out, if we can shift from thinking of change, especially reanalysis, as error or mis-analysis, to thinking of it as a manifestation of our creative ability to symbolize, then

- (27) [T]he formation of grammar (and other cultural systems) demonstrates an overperformance of human minds, a capacity for forming new symbols for immediate use that surpasses any need to acquire precisely all the details of extant patterns of usage. (Andersen 2006: 81–82)

Analogizing to an extant construction is essentially a case of renewal. Meillet (1958 [1915–16]) noted that expressions of clause connectives and of negation are especially open to renewal. But in fact every functional category is likely to be renewed many times, and therefore to have many alternative forms. What may once have been a functional category with a relatively small number of expressions may be expanded exponentially.<sup>12</sup> It appears that having several forms that are very close in meaning for one functional category is useful in negotiating meaning. The category of degree modifiers is one that has expanded; so too has membership of various types of adverbial categories. The fact that in English large numbers of expressions were semantically reanalyzed as having degree modifier meanings in the sixteenth century (Peters 1994; see also Borst 1902) must be cultural. So presumably is the favoring of heads with positive meanings such as *honor*, *dignity*, *evidence* in the negative polarity context of *shred of*. That such a restriction is a social tradition and not a strictly cognitive phenomenon is suggested by the fact that *a bit of*, which is also favored in negative polarity contexts, is not constrained to occurring with the positive member of a pair of antonyms. We may consider also the division of labor in recent developments in Dutch of the items *bar* and *bijster* ‘very, all that’ reported in Hoeksema (1996–97). *Bar* has come to be used in positive polarity contexts, but with heads that are negative members of an antonym set, while *bijster* has come to be used in negative polarity contexts with heads that are positive members of antonym sets.

<sup>12</sup> Reductions are also possible, e.g., the reduction of a large inventory of subordinators in eighteenth century French (Schlieben-Lange 1992).

One of the theoretical contributions of research in grammaticalization has been the hypothesis of unidirectionality. The change-schema in (26) is unidirectional in the sense that degree adjuncts are not expected to become degree modifiers, nor are degree modifiers expected to become partitives, whereas the reverse order is widely attested. Although the reasons for directionality are a matter of considerable debate, it seems likely that they result from speaker production strategies, including two much-discussed competing motivations (e.g., Langacker 1977; Slobin 1977; DuBois 1985).<sup>13</sup> One is “Be quick and easy” (“Zipf’s Law”: Zipf 1929). Economy in speaker production leads to reuse of old material for new means (hence analogy), and routinization. The second motivation is “Be clear”. While this appears to be hearer-oriented, it presupposes speakers having hearers in mind, and leads to greater explicitness.<sup>14</sup> A less widely-acknowledged motivation is the tendency for speakers to recruit material to the purposes of text-making, i.e. giving symbolic expression to rhetorical strategies (Traugott & Dasher 2002). A particularly obvious example is the recruitment of descriptive speech act verbs (e.g., *promise*) and mental verbs (e.g., *recognize*) for illocutionary purposes. The development of *sort of* and *a lot* as free responses is another example: such responses express the speaker’s assessment of someone else’s linguistic expression.

The hypothesis of unidirectionality is often greeted with skepticism, as to some it suggests that there was a stage of language in which an item toward the right in a change-schema may not have been available at an earlier stage of the language. However, since the change-schemas are generalizations about how classes of form-meaning pairs (symbols) may develop once they have come into being, nothing is claimed about the development of the concepts themselves. A more extreme form of concern has been that unidirectional schemas might lead to the reconstruction of a language stage that violates the uniformitarian principle because it would have features that are not evidenced by extant languages (e.g., Lass 2000). If we assume

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<sup>13</sup> Competing motivations are usually based on the assumption that change is change in use (Croft 2001). By contrast, Kiparsky (forthcoming), assuming that change is change in grammars, proposes that unidirectionality arises out of universal optimization principles.

<sup>14</sup> Recent work on syntactic parsing suggests that speakers are not as altruistic as usually thought. There is growing evidence from processing studies that production is not always motivated by perceived needs of the addressee. Speakers use ambiguities, and Addressees resolve ambiguity from contextual information (e.g., Roland, Elman & Ferreira 2006; also Wasow 1997).

that development of the language capacity was incremental and adaptive, this is not an issue, since we can have no way of knowing what language was like prior to modern humans. It presumably had characteristics that were unlike those of modern human languages, e.g., it may not have had recursion, but we do not know for sure what they were. By hypothesis whatever communicative system preceded the modern language capacity was retained and built upon, not discarded. Furthermore, as Comrie (2003) has stressed, the uniformitarian principle as used in relation to geology (where it was first developed), concerns not states but processes.<sup>15</sup> It seems probable that concepts did at some point precede certain symbolic expressions of them, probably prior to the development of modern languages, and that the processes that have led to the expression of concepts have been uniform over time. What has changed is the pairing of concepts with forms.

## 8. Conclusion

Categorization into classes and members of classes, or evaluation of entities on a scale are presumably part of general cognition and experience. We may assume that they developed as human beings evolved. Croft's hypothesis in his theory of Radical Construction Grammar is that there is a "universal conceptual space" (2001: 9; see also Langacker 1987), and that:

- (28) universals of language are found in the patterned variation in distribution of constructions and the categories they define. (Croft 2001: 5)

These constructions are said to be primitives (Ibid.), but also language-specific (Ibid.: 6). A solution to the apparent contradiction between "primitive" and "language-specific" status might be to develop a theory in which macro-constructions are primitive, and presumably universal, while lower-level micro- and meso-constructions are language-specific. Grammaticalization, which concerns the dynamic realignments of form-meaning pairings, can then be seen as a theory of the relationships between such pairings, and their likely directionality over time. But not everything can be accounted

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<sup>15</sup> See Dahl (2004), Andersen (2006) for extensive discussion of ways in which terminology from the physical sciences, especially evolutionary studies has been used in linguistics, often in metaphorical ways that may produce profound conceptual problems.

for in terms of cognition and experience. Dahl (2004: 114–1115) has suggested that certain properties of languages may evolve over time as a direct result of grammaticalization,<sup>16</sup> including inflectional morphology, grammatical gender, agreement, word order that violate adjacency (e.g. V2), specific marking of subordinate clauses, and tone (exactly where these fit in constructional hierarchies remains to be determined). And the fact that certain innovations take hold in a community, i.e. are transmitted and then may spread to more communities, or become entrenched in constructions, is a social and cultural fact.

The kinds of developments discussed in this paper are relatively recent, having occurred only over the last six hundred years or so. Framed in terms of the intersection of grammaticalization and constructions, they appear to be consistent with the view that:

- (29) The complex content of human cultures has been built incrementally, with cognitive equipment present at least 250 ka, in a process that continues today. (McBrearty & Brooks 2000: 532)

## Acknowledgements

Thanks to members of the Blankensee Colloquium and to members of the Linguistics group, University of Copenhagen, for comments and suggestions on earlier versions of this paper. Also to members of the Workshop on Constructions and Language Change organized by Alexander Bergs and Gabriele Diewald at ICHL XVII in Madison, Wisconsin, August 2005 for comments on a related paper, in which some of the same issues are discussed (Traugott, forthcoming a).

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<sup>16</sup> Dahl (2004: 119) says that a lexical item comes to serve a grammatical function “by virtue of becoming a fixed part of a larger pattern – a grammatical construction”. Since in Radical Construction Grammar, at least, a single word may be a construction, I assume that Dahl intends the term “construction” to be used in a less theoretical sense than is proposed in Construction Grammar.

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# **Cognitive Foundations**



# The two faces of creole grammar and their implications for the origin of complex language

Alain Kihm

## 1. Introduction

The initial idea for this paper grew out from an observation and a reading. The observation is, I think, one that everybody can make, although strangely it has seldom, perhaps never, been seriously pointed out and used for theoretical elaboration. It is that creole grammars, when compared with one another, show a sharp division. On the one hand there is the verb phrase (VP), which is the whole syntactic field whose leftmost boundary is the negation or, if it is missing, a tense-aspect particle, and whose rightmost boundary includes the selected complement(s) of the main verb. On the other hand there are noun phrases (NPs). The inner structure of VPs turns out to be remarkably uniform across creole languages, no matter what their lexifier languages, possible substrate influences or type as fort vs. plantation.<sup>1</sup> The inner structure of NPs, in contrast, appears much more diverse and more obviously under lexifier and/or substrate influence.

The reading that put me on the track that led to the present paper was Andrew Carstairs-McCarthy's 1999 book called *The Origins of Complex Language: An Inquiry into the Evolutionary Beginnings of Sentences, Syllables, and Truth* (henceforth C-MC). A thought-provoking title indeed! C-MC's basic hypothesis is that natural language syntax is rooted in the distinction between noun phrases and sentences, it is arbitrary in the sense that it could have been organized in quite different ways without depriving our ancestors of the evolutionary edge language is supposed to have given them, and it ultimately originates in phonological, more precisely syllabic structure.

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<sup>1</sup> Plantation Creoles arose from displacement of the creole-speaking population because of slave trade or indentured labour, whereas fort Creoles did not. The Caribbean Creoles and the West African Creoles are typical examples of the former and latter types respectively.

The aim of this paper is to show that the intriguing property of creole languages pointed out in the first paragraph is a direct consequence of the phonological foundation of natural language syntax mentioned in the second paragraph. Because of the special conditions of their genesis, this foundation, I will argue, shows up in young creole languages more perspicuously than it does in 'old' non-creole languages.

The latter claim requires some words of caution. That the living languages we call 'Creoles' are young languages is a certain fact (see section 4). The equation of 'old' with 'non-creole', in contrast, is in no way warranted. One cannot dismiss the possibility that old or even dead languages arose from processes akin to those that produced the Creoles of today. So much has been argued for, e.g., English, Marathi, Proto-Germanic, and the Romance languages (see Southworth 1971; Bailey & Maroldt 1977; Hall & al. 1977; Schlieben-Lange 1977; Goyette 2000). One cannot dismiss this possibility, to be sure, and yet one cannot do much with it. For one thing, the formative events in question are so remote that their specific import, insofar as it is distinct from 'ordinary' language change, has necessarily been overlaid to a large extent by the effects of the latter. Then, the phenomena the authors put forward in order to support a creole origin quite often seem to resort to language contact more than to creolization in the (perhaps too radical) sense I wish to keep for this term, namely that of a catastrophic event giving rise to a *new* language, rather than to a continuation of some previous idiom, no matter how severely changed.<sup>2</sup> I will therefore ignore the possibility that there may be more creole languages than the ones that appeared four or five centuries ago and are (most of them) still with us today. The argument of the present paper is based entirely on these languages.

In the following section I illustrate the creole NP/VP divide. Then I present C-MC's theses and my own interpretation of them. In section 4 I explain why creole languages constitute the best testing ground for such theses, and I point out in section 5 that regular presence of an overt copula (or one of several overt copulas) in sentences having NPs or prepositional phrases (PPs) as predicates is diagnostic evidence. In section 6 I then show how phonology-based syntax is able to account for a whole array of facts concerning nouns predicates in closely related Cape-Verde and Guinea-Bissau

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<sup>2</sup> I agree that the dividing line between 'new' and 'deeply modified' may be hard to draw. I think it can be drawn, however, given precise comparative analyses, which may still exceed our present state of knowledge.

Portuguese-based Creoles, the empirical field in which I have first-hand expertise. Finally I conclude that creole languages do indeed bear out CM-C's hypothesis, *modulo* my reinterpretation, thus casting light onto the constituency of Universal Grammar (UG).

## 2. Noun phrases vs. verb phrases in creole languages

The uniformity of VP structure across creole languages as opposed to the variety of NP structures is readily illustrated with a few examples, purposefully including 'exotic' Creoles, i.e. non-Caribbean Creoles or Creoles with non-Indo-European lexifier languages:<sup>3</sup>

- (1) *Iyal marid de ma ge akulu laham.* (Juba Arabic)  
 child sick D NEG T eat meat  
 'The sick child does not eat meat.'
- (2) *Dat kid nomo bin kilim walabi.* (Ngukurr Kriol)  
 D child NEG T kill wallaby  
 'This child did not kill wallabies.'
- (3) *Kil mininu dwenti di ña amigu ka ta kume karni.* (Guinea-Bissau Kriyol)  
 D child sick of my friend NEG T eat meat  
 'That/the sick child of my friend's does not eat meat.'
- (4) *tifi malad la pa ka manje vyann.* (Martiniquais)  
 girl sick D NEG T eat meat  
 'The sick girl does not eat meat.'

Juba Arabic is an Arabic-based Creole spoken in Southern Sudan (see Miller 1985); English-based Ngukurr Kriol is spoken in the Northern Territory of Australia (see Adone 2003); Portuguese-based Kriyol is the main vehicular language of Guinea-Bissau (see Kihm 1994); Martiniquais, a French-based creole, is spoken in Martinique in the Lesser Antilles (see Bernabé 1983). In all four languages we find the same elements in the verb phrase, identically ordered, namely:

- (5) Negation > Tense-Aspect > Verb > Complement(s)

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<sup>3</sup> D = determiner ; NEG = negation ; T = tense-aspect.



The grammars of the lexifier languages do not seem to play a significant role in accounting for these elements and their ordering. Note that Arabic, Portuguese, and French are highly inflected languages which do not possess exact counterparts of tense-aspect particles such as *ge*, *ta*, or *ka*.<sup>4</sup> However, the latter can be historically accounted for as resulting from the grammaticalization of auxiliary verb forms in periphrastic constructions in the lexifier languages. For instance, Juba Arabic *ge* is related to the dialectal (e.g. Chadic) participle *gaaʔid* ‘remaining’ used in constructions such as (6) (see Pommerol 1999: 208).<sup>5</sup>

- (6) *Al-wileed gaaʔid yabki*.<sup>6</sup>  
 D-child remaining 3SG.M.IPF-cry  
 ‘The child is crying.’

Grammaticalization implies restructuring, though. In (6), *gaaʔid* is an obvious member of the paradigm of *gaʔad* ‘to remain’. There are no morphological clues to the lexical category of *ge* in (1), in contrast, nor any compelling empirical reasons to allot it to one of the major categories, so calling it a ‘particle’ – i.e. the self-standing exponent of a functional meaning, the phonologically free (or freer) equivalent of a bound morpheme – may well be the best we can do. Further note that *yabki* ‘he cries’ is inflected for tense, person, number, and gender whereas *akulu* in (1) is uninflected in the creole fashion.

The same can be said about Kriyol *ta* and its relation to Portuguese *estar*, or Martiniquais *ka*, the origin of which is unsure. English is less inflected than the other lexifier languages in the sample, but again Ngukurr Kriol *bin* clearly involves restructuring when compared with its etymon *been*.

I want it to be quite clear that when pointing out similarities in the verb phrases of these four Creoles, I am only referring to **formal** similarities. In particular, I am not implying anything about the number or meanings of the tense-aspect particles in the spirit of, say, Bickerton (1981). It is quite enough

<sup>4</sup> There is no consensus concerning the syntactic category, if any, of these elements. ‘Particle’ seems to me to be the most neutral term.

<sup>5</sup> Chadic Arabic is not the lexifier of Juba Arabic, which is more likely to be some variant of Sudanese Arabic. Yet, it is sufficiently close to it to make the comparison worthwhile. Moreover Chadic Arabic is itself partly creolized, it may even be a semi-creole in the sense of Holm (2004).

<sup>6</sup> 3SG.M.IPF = 3<sup>rd</sup> person singular masculine imperfective.

for my purpose that these particles, whatever their array and precise interpretations may be – and they certainly are distinct *pace* Bickerton – are present in all four Creoles (and more) and in the same location, between the negation and the verb.

The lack or extreme scarcity of affixal verb inflections in the four Creoles exemplified as well as in all Creoles generally is also something that hardly need to be emphasized.

Concerning the negation, notice that it appears in the same preverbal position as in the lexifier language in only two languages, Juba Arabic and Kriyol, as shown by the following examples:<sup>7</sup>

- (7) *Al-wileed maa akal /yaakul.* (Chadic Arabic)  
 D-child NEG eat.3SG.M.PF / 3SG.M.IPF-eat  
 'The child didn't/doesn't eat.'
- (8) *O menino não come.* (Portuguese)  
 D child NEG eat.3SG.PRES.INDIC  
 'The child doesn't eat.'

But also note that Portuguese *não* serves both as a sentential negation meaning 'no' and as a predicate negation, while Kriyol *ka* is only a predicate negation. Whenever the negation is not preverbal in the lexifier language, however, it becomes so in the Creole: English post-auxiliary *not* is replaced by Ngukurr Kriol preverbal or, more accurately, VP-initial *nomo* (see (2)); French postverbal *pas* becomes Martiniquais VP-initial *pa* (see (4)).

The shift, more than the position itself, is what I find significant. Why didn't Martiniquais, for instance, keep the ordering of (colloquial) French *La petite fille mange pas de viande*?<sup>8</sup> A quite plausible answer is that (4) does not actually derive from such a structure, but either from a French construction involving an auxiliary where *pas* precedes the main verb (cf. *La petite a pas mangé...*), or from a pidgin-foreigner's talk utterance using the infinitive such as *Petite fille pas manger...*<sup>9</sup> The former hypothesis is

<sup>7</sup> 3SG.M.PF = 3<sup>rd</sup> person singular masculine perfective; 3SG.PRES.INDIC = 3<sup>rd</sup> person present indicative.

<sup>8</sup> I assume, rather uncontroversially I think, that *pas* is the real negator in spoken French, with *ne* an optional scope marker that has no reflex in the Creoles.

<sup>9</sup> The phonological identity of *mangé* and *manger* allied to the inconspicuousness of the auxiliary may be the reason that both geneeses are in fact indistinguishable.

supported by the contrast in Louisiana Creole between perfective *Mo pa mōzhe* ‘I haven’t eaten’ and *Mo mōzh pa* ‘I don’t eat’ (see Rottet 1992: 268; Kihm 2003a). It accounts as well for Haitian *M pa ap manje* ‘I’m not eating’, probably derived (directly or through a pidgin phase) from *Je suis pas après manger*.

In the case of Martiniquais, however, the particle *ka* has no obvious French etymon, so the preceding negation cannot be explained away so easily. Moreover, we have to reckon with the fact that *pa* precedes the Past particle *te* (cf. *Tifi la pa te manje* ‘The girl did not eat’) despite its probable origin in French *était* ‘was’ as in *La petite fille était pas à/pour manger*.<sup>10</sup> And also with the fact that it precedes stative verbs such as *konen* ‘to know’ (cf. *Man pa konen* ‘I don’t know’) which are unlikely to have been used in the Present Perfect in this context – not to mention that the past participle of French *connaître* is *connu*, whose creole reflex would presumably be \*/koni/, not *konen*, which probably comes from the Present (*je/tu/il*) /kone/.

Be that as it may, no such historical explanation seems available for the VP-initial negation of Ngukurr Kriol (see (2)) and all English-based pidgins and creoles, unless one assumes *do*-support deletion before uncontracted *not*: *I do not eat* > *\*I not eat* > *Mi no iit* (basilectal Jamaican – see Bailey 1966). Unlikely – although not impossible – as the process may be, it cannot account for Negerhollands *Mie no weet* ‘I don’t know’ compared to Dutch *Ik weet niet*.

This discussion about the place of the negation is especially relevant, I think, for it brings up a crucial point: it is nearly always possible to think up a distinct explanation for the shift to VP-initial position in each particular Creole, and there is no question that such accounts may be factually right. Yet, the fact that, independent as they were, all these events conspired to bring about identical results cannot help but strike one as intriguing. The issue, in other words, is not whether such events may have happened –

<sup>10</sup> To be sure, there is also the possibility that *te* comes from the past participle *été*, which would entail a different position of the negation in the French model (cf. *La petite fille a pas été...*). The probability this might be the correct account, however, is lessened by the fact that *passé composé* (Present Perfect) was much rarer in all dialects of 17<sup>th</sup> century French than it is in contemporary standard French. In the context of a remote past, the Simple Past *fut* for punctual events or the Imperfect *était* for durative events would have been the norm. Since the former left no trace whatsoever in the Creoles, we have to bet for the latter. The Reunion Creole Past particle *lete* < *il était* strongly suggests this is a winning bet (see Corne 1999).

clearly they may – but that they did happen similarly in so many, often unrelated Creoles (say Martiniquais and Tok Pisin) rather than occurring differently or not at all in each particular language. As a matter of fact, they did not occur precisely when negation was already preverbal in the lexifiers, namely Arabic and Portuguese in our sample.

Counterexamples are interesting as well. In Berbice Dutch (see Kouwenberg 1994, 1995) the negation is not VP-initial. But neither does it follow the verb as in Dutch: it is clause final as in the following example (Kouwenberg 1995: 237):

- (9) *Ek suk mu lasan eni ka.* (Berbice Dutch)  
 1SG want go leave 3PL NEG  
 ‘I didn’t want to leave them.’

In the Gulf of Guinea Portuguese-based Creoles (see Günther 1973; Ferraz 1979; Post 1995), the negation is at the same time VP-initial and clause final as shown in (10) (Post 1995’s (21)):<sup>11</sup>

- (10) *Odyai amu na be mem -bo xama-kumu -f.* (Fa d’Ambu)  
 today 1SG NEG see mother-2SG place-food -NEG  
 ‘Today I haven’t seen your mother at the market.’

The VP-initial negation *na* obviously comes from Portuguese *não*, whereas clause final *fa* (São Tomé and Príncipe) or *f* (Annobon) is of dubious origin. Synchronically it may well be that *na* is a scope marker analogous to French *ne*, and the ‘real’ negator is *f(a)*. (In Principense only the latter is found – see Günther 1973: 77–78.)

Berbice Dutch and the Gulf of Guinea Creoles are not really at odds with the VP-initial trend, in the first place because the negative items *ka* and *f(a)* were probably borrowed from the respective substrates (Eastern Ijo in the former, perhaps Bini in the latter – see Rougé 2004), a factor that cannot be demonstrated to have played any role in this area in the other Creoles. Secondly, it is certainly significant that they occupy a peripheral position, meaning they are not linearized with respect to the VP to begin with, but with respect to the whole clause.

The reality of the VP-initial trend cannot therefore be denied, it seems. Where does it come from? To claim that prior pidginization – not attested in every case – is the cause begs the question. Why should VP-initial nega-

<sup>11</sup> This is also a possibility in Berbice Dutch (see Kouwenberg 1995: 237).

tion be an essential pidgin feature? There is no answer to this question, unless we accept, as will be argued later on, that pidginization, or some similar process, has something to do with increased access to structures directly determined by the principles of Universal Grammar.

Meanwhile, compare the noun phrases in examples (1)–(4). Juba Arabic exhibits the order Noun > Adjective > Determiner (see (1)). Comparing it with its Chadic Arabic equivalent in (11) shows it differs from the latter only in having further grammaticalized the demonstrative *da* ‘this’, which became a mere definiteness marker with concomitant loss of the prefixed definite article (*a*)/-, and in having lost gender/number/definiteness agreement with the head noun:

- (11) *al-wileed al-mardaan da* (Chadic Arabic)  
       D-child D-sick DEM  
       ‘this sick child’

A more complex example can be given from Turku, a now extinct Arabic-based pidgin/creole formerly spoken in Chad (see Tosco & Owens 1993: 203):

- (12) *Shíli rangáye laam wadín anapokter anína.*  
       take basket meat other of porter we  
       ‘Take some other baskets of meat of our porters.’

The main structural difference between Turku and Arabic is in the use of full instead of suffixal pronouns in possessive function: compare *pokter anína* with Chadic Arabic *kitaab-na* ‘our book’. Otherwise, *rangáye laam wadín* is a *bona fide* Construct State Nominal forcing extraposition of the adjective (*wadín* ‘other’) modifying the head *rangáye* ‘basket’. Prepositional possessive constructions are also a feature of all varieties of Arabic, with a rather wide range of prepositions. Turku *ana* is related to *hanaa*, a shibboleth of Chadic Arabic, itself the grammaticalization of a noun meaning ‘property’ in Classical Arabic.

In Ngukurr Kriol, in contrast, the determiner precedes the noun, but again in conformity with the lexifier (cf. *that kid*). The following Jamaican example from Bailey (1966: 99) illustrates a noun phrase with an attributive adjective:

- (13) *di sik biebi*  
       D sick baby  
       ‘the sick baby’

A comparison of the subject noun phrase in Kriyol (3) with its Portuguese translation, *Aquele menino doente do meu amigo*, should suffice to demonstrate the near-perfect identity of the two languages in this area. The main divergence is the absence of a definite determiner in Kriyol (compare *di ña amigu* and *do meu amigo*, in which *do* = /de o/ 'of the') partially compensated by the possibly anaphoric reading ('the N in question') of the distal demonstrative *kil* 'that'. In closely related Cape Verdean, *kel* seems to have gone all the way towards being a definite article (see Baptista 2002), an evolution that fully parallels that of *da* in Juba Arabic.

Martiniquais seems to have gone a different route, since the definite determiner comes at the end of the noun phrase (*tifi malad la* 'the sick girl'), but it precedes it in French (*la petite fille malade*). Yet, we know that Martiniquais NP-final *la* has nothing to do with the French article; it comes from the deictic locative adverb *là* 'there', commonly postposed to noun phrases including a demonstrative or the definite article: *cette fille-là* 'that girl', *la fille là* 'the girl [already in discussion]'. The proclitic French definite article *le/la/les* fell victim to the creolization process, vanishing entirely or being reanalysed as a meaningless first syllable: cf. Haitian *lajan(-an)* '(the) money' < *l'argent*, *labu(-a)* '(the) mud' < *la boue*, etc. Taking this into account, the noun phrase in Martiniquais (and French-based Creoles generally) turns out to be structurally hardly distinct from the French noun phrase.

The point, then, is not that creole NPs, as compared to VPs, more closely resemble the lexifiers. Sometimes they do, sometimes they don't. To put it metaphorically, each Creole seems to have found its own solution to the problem of expressing the nominal functional categories of definiteness, number, etc., or of building complex noun phrases, and these solutions depended very much on what the particular lexifiers and substrate languages had to offer. For instance, the NP-final definite determiner *la* of nearly all French-based Creoles was made possible by the availability of the phonetically salient French deictic item *là*.<sup>12</sup> To this, we may add the fact that the probable substrate languages of the Caribbean Creoles, namely the Gbe languages of Benin, also have postposed determiners which may even fortuitously sound like *là* (cf. Ewe *-la* – see Kihm 1989; Lefebvre 1998). In contrast, there is nothing like French *là* in Portuguese, and the article *o/a* is phonetically quite elusive, so the Portuguese-based Creoles either made it

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<sup>12</sup> The only exception is Seselwa which has preposed *se* from the French demonstrative *ce* (see Corne 1999).

with no definite determiner at all (e.g., Kriyol), or had recourse to Portuguese demonstratives such as *kel* < *aquele* in Cape-Verdean or *se* < *esse* in the Gulf of Guinea.

Additional examples could be given, for instance concerning number marking or the structure of possessive noun phrases. They would all point in the same direction, namely the dissimilarity of creole languages in the domain of NP structure as opposed to their structural likeness in the VP domain.

It would be very difficult, therefore, not to ask the question: why do we observe such a division in creole grammars? Perhaps it is only an accident, and the question is irrelevant. But it may also reflect something deep. To show this we now turn to C-MC's theory of the origin of the properties of natural language.

### **3. From syllables to sentences**

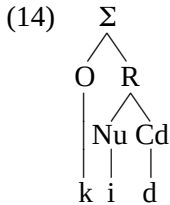
To demonstrate the arbitrariness of natural language syntax, C-MC makes up some possible languages that do not make use of the basic distinction of NP vs. sentence, according to him typical of 'Human Language'. One of these languages, for instance, he calls Asyntactic and it looks a little like some real pidgins we know of such as early (19<sup>th</sup> century) Tok Pisin or Russenorsk. He shows, rather convincingly, that such languages would have done quite well to satisfy the communicative needs of hunter-gatherer herds (also see Uriagereka 2001). Perhaps they wouldn't have allowed humankind to progress to the glory of post-industrial society, but that is a different story.

Since (if) it is arbitrary, natural language syntax 'as-it-is' must result from some accident in the course of evolution. According to C-MC this accident occurred about 200 000 years ago, when archaic *Homo Sapiens* started to give way to *Homo Sapiens Sapiens*, and it was the descent of the larynx due to the erect posture and the alignment of the head along the body axis (also see Lieberman & Crelin 1971). Rather than being flat as in apes or older humans, the larynx – mouth cavity space became right-angled. This anatomical change notably enlarged the supralaryngeal space, which in turn allowed our ancestors to do two things more and better. First, they could increase the sheer number of distinct sounds. Secondly, they became able to distinguish consonant-like from vowel-like sounds more neatly than their predecessors (or perhaps their Neandertalian cousins) did.

This means they could discriminate them accurately enough that these sounds took on the quality of phonological segments or phonemes, which gravitated together to form syllables.

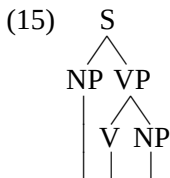
### 3.1. Syllables and sentences: C-MC's view

C-MC uses what may be called the standard model of the syllable, whereby syllable structure rests on the hierarchical contrast of a vocalic nucleus and a consonantal margin consisting of an initial onset and a final coda. This is schematized below, taking *kid*, English or Ngukurr Kriol, as an example:



The components of the syllable ( $\Sigma$ ) are the consonantal onset (O) and the rhyme (R). The rhyme consists in a vocalic nucleus (Nu) and a consonantal coda (Cd). Given that Nu is the only obligatory component (cf. French *eau* [o] ‘water’),  $\Sigma$  may be considered to be the projection of R, itself the projection of Nu.

As already mentioned, syntax ‘as-it-is’ is built on the distinction of noun phrases (NPs) vs. sentences (Ss). (I ignore recent and possibly short-lived refinements that led to rename Ss as IPs and CPs.) It thus reflects syllabic structure as represented in (14): NPs (arguments) correspond to the margins; S corresponds to  $\Sigma$ , and it is the projection of the verb phrase (VP). VP is analogous to the rhyme and it is the projection of the verbal nucleus V. The following schema shows the elementary sentence structure stripped of all accretions useless for our purpose:





This structure aptly describes, e.g., Kriyol *Kil mininu kume karni* ‘This child ate meat’, where *Kil mininu* / *This child* realizes the ‘onset’ or subject NP immediately dominated by S, *kume* is the V ‘nucleus’, and *karni* / *meat* is the ‘coda’ or complement NP. In another, but fully equivalent terminology, (15) represents the so-called X-bar schema, with S the maximal projection, NP under S the specifier, V the head, and NP under VP the complement.

The structural homology between (14) and (15) is both unmistakable and real. That is to say, it is not a representational artifact: any other representational style – e.g., using autosegmental linkings instead of trees – would reveal the same parallelism. Similarities actually go further than structural parallelism. Just as only the vocalic nucleus is obligatorily realized, sentences may, at least apparently, consist in nothing but their verbal nuclei (cf. Portuguese *Canta* ‘S/he sings’). The same asymmetry shows up with the two marginal components: although the onset and the coda are dispensable parts of the syllable, only the latter may be systematically absent. Numerous languages only have consonant-vowel (CV) syllables, but there does not seem to exist any language with only vowel-consonant (VC) syllables. Moreover, many languages that seem to tolerate V(C) syllables actually require a glottal stop in front of the apparently initial vowel (cf. German). In the same way, although there are sentences effectively lacking a complement NP at all analytical levels (cf. *She’s coming*, *She’s sleeping*, etc.), one can doubt the reality of entirely subjectless sentences.<sup>13</sup> There are still other similarities between the syllabic and syntactic structures, which C-MC points out and I leave aside.

There are differences as well, of course, but they can be related to the fact that phonology and syntax, formally parallel that they are, are not made of the same stuff. For instance, NP complements are present or not depending on the meaning of the verbal nucleus: *to come* does not ‘take’ a complement, while *to put* requires one (even two). The timbre of the nuclear vowel, in contrast, obviously has no effect on whether a coda is there or not. Likewise, there does not seem to be a phonological counterpart of, say, WH-movement, which is probably related to the fact that syllabic structure of itself does not express meanings as syntactic structures do. Notice, however, that long-distance relations are not foreign to phonology – think of vowel harmony – and they may relate to syntactic movement as a kind of prefiguration.

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<sup>13</sup> See the vast literature about the so-called ‘pro-drop’ languages and topic prominent languages such as Chinese.

Similar remarks can be made concerning variation in word order as compared to the rigidity of the onset-nucleus-coda sequencing. For instance, if Kayne (1994) is right to consider specifier-head-complement (SVO) to be the universal basic ordering, and all different orderings to be derived from it, the phonology-syntax parallel comes out nearly perfect. But even if anti-symmetry is mistaken and, say, SOV is considered as basic as SVO, no dramatic consequences ensue. The phonology-syntax parallelism has only to be moved up by one notch to the onset-rhyme or NP-VP level, ignoring the inner ordering of the nucleus or verb with respect to the coda or object. To be sure, VSO – the only alternative word-order with a significant frequency – then poses a problem. I can do no more for the moment than pointing out that (a) VSO is a rare order compared to S-Predicate (conflating SVO and SOV); (b) no pidgin or creole language shows it, whatever that is supposed to mean.

Given the formal similarity of the two structures, C-MC proposes the following story. The descent of the larynx allowed the emergence of the syllable and of the phonological contrasts it structures. This emergence took place within a species (ours) that was already endowed with highly developed cognitive capacities thanks to an independently occurring increase of the brain volume.

A pause is in order at this juncture. Obviously, the concurrence of the two events, larynx lowering and reaching a critical encephalic mass, was crucial. It may provide an answer to the objection that low larynxes are also observed in non-talking species (see Finch this volume). On the other hand, there are those who maintain, *contra* Lieberman & Crelin (1971), that larynx lowering cannot be of any real relevance since complex sound production, especially of the whole range of vowels, is perfectly feasible without it (see Boë et al. to appear).

Actually, as linguists, we do not have to take sides on this issue. It is enough that we can accept it as a fact that, at some point in its evolution, the human species became endowed both with vast mental faculties including the ability to conceive and to interpret discrete signals and with the physiological means to produce and to perceive such signals. A vital issue could thus be resolved, namely how to express concepts in potentially infinite (at least unbounded) quantity through signs that must therefore also be potentially infinite in number and as distinct as possible from one another.<sup>14</sup>

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<sup>14</sup> I don't think 'vital' is too strong a word. Imagine a species with the same mental richness as ours, but deprived of the possibility to express it adequately by

Double articulation, the theoretically unlimited combination of meaningless phonological segments to form meaningful sequences usually called ‘words’, was the solution.

Potentially infinite lexicons thus began to grow. A new question then came up: how to combine the lexical signs that were now available in order to convey complex messages about who did what to whom in a real or imagined situation (see Appendix)? Apparently, humankind chose the easiest way as they are wont to do: they merely transferred the already established phonological structure to the new syntactic domain. As shown by C-MC, this was not the only possibility. Syntax could have worked on quite different principles than phonology. Using the same structure for both was simply the most sensible solution given how the human mind works (we believe).

Notice however that the foundation of the structure changed as it was thus transferred. Given physiological factors that cannot be modified, (14) is necessary: being the most sonorous elements, vowels must be nuclei; as the least sonorous elements consonants have to be onsets or codas. Such ‘hardware’ factors apparently play no role in syntax, so (15), the syntactic copy of (14), is arbitrary contrary to the latter. Actually, it is arbitrary precisely because it arose as a copy of the already established phonological structure, not for functional reasons, and it is not directly selected by survival needs (phonological structure probably is).

### 3.2. Syllables and sentences: a different model

As mentioned, C-MC’s syllable model is the standard one represented in (14). It is characterized, as we saw, by double branching, at  $\Sigma$  level and at R level, and by the fact that only the nucleus is obligatorily realized. Given the possibility of complex onsets and codas (e.g. *strict*), the set of syllables it generates may be described with the formula  $(C^+)V^+(C^+)$ .<sup>15</sup>

Other authors (e.g., Lowenstamm 1996) maintain that syllables are basically and obligatorily single-branching: there must be an onset and there is never a coda. Such a model thus only generates  $C^+V^+$  syllables, in which complex onsets ( $C^+$ ) are furthermore limited to ‘natural’ consonant clusters

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sounds or gestures, not to mention telepathy – something that should not be too hard to imagine, as it is the ordeal aphasics have to live with. Such a species, I wager, would not survive very long. (Also see the appendix to this paper.)

<sup>15</sup>  $V^+$  because the nucleus may be complex as well, meaning it may be a diphthong as in *strike*.

involving an obstruent (/k/, /t/, etc.) and one or several continuants (/s/, /r/, etc.), but not several obstruents. The word *strict*, for instance, is given the following syllabic representation



Since scanning is trivial in the CV model – every V is a syllable boundary – there is no need to show a  $\Sigma$  level, and the onset and the nucleus may be directly notated C and V. The final segments /k/ and /t/, a complex coda in the standard model, are considered onsets of CV syllables in which the V nucleus is empty, that is it is not identified by phonological material. (Strict conditions which I cannot detail warrant the licencing of empty nuclei – see Kaye, Lowenstamm & Vergnaud 1990.) Likewise, the apparent initial syllable /tka/ of the Russian word *tkanie* ‘weaving’ results from the amalgamation of two underlying syllables /tØ/ and /ka/.

(16) looks quite different from (14). In particular, (16) does not seem to present us with the asymmetry of an onset and a branching rhyme mirroring the subject-predicate asymmetry. Let us assume, nevertheless, that (16) expresses Universal Grammar in the phonological domain more adequately than does (14). Does it contradict the ‘syllabic syntax’ hypothesis schematized in (15)? I don’t think it does. Indeed, it can be demonstrated that even CV syllables are endowed with the asymmetry the hypothesis requires.

Every vowel has a consonantal counterpart traditionally called a ‘semi-vowel’ (or a ‘semi-consonant’) and now usually called a glide. It is common knowledge for the high vowels /i/ and /u/, the corresponding glides of which are /j/ and /w/; but it is also true of the low vowel /a/ that has the glottal stop as a consonantal counterpart (see Prunet 1996; Kihm 2003), and of the mid vowels /e/ and /o/ (cf. the Romanian diphthongs /ea/ and /oa/). True consonants, in contrast, i.e. obstruents, have no vowel equivalent. It follows that, e.g., (17) and (18) are possible syllables pronounced /ja/ and /’i/ respectively, whereas (19) is ill-formed.<sup>16</sup>

<sup>16</sup> Using phonetic evidence, authors have argued for the well-formedness of (19) and similar syllables in some dialects of Berber, concluding that obstruents may indeed be nuclei (see Dell & Elmedlaoui 1985). The empirical basis for their case remains very narrow, however.

(17) C V  
       | |  
       I A = /ja/

(18) C V  
       | |  
       A I = /'i/

\*(19) C V  
       | |  
       A k

In addition to vowels and true consonants, there is the intermediary class of sonorants (/l/, /r/, /m/, /n/), which seem to be able to occur in either position (cf. Czech *vlk* 'wolf'). I will come back to this.

I therefore propose the following principle:

- (20) Syllabic asymmetry principle: Every nuclear V segment may also occur in the onset where it is realized as a glide, whereas onset C segments cannot occur in the nucleus (except sonorants).

Applied to syntax, the CV model implies a basic contrast between type V elements, where V means vowel or verb, and type C/NP elements. I will therefore call this kind of syntax **CV syntax**. The following is the counterpart of (20):

- (21) Syntactic asymmetry principle: Type V elements – i.e. verbs as well as verb phrases and sentences projected from them – may appear in the same positions as type C/NP elements, but C/NP elements cannot occupy V positions.

Unlike C-MC, therefore, I assume syntax to rest on the fundamental contrast not of noun phrases and sentences, but of noun phrases and verb phrases, of which sentences are the extended projection (see Grimshaw 1990).

A crucial correlate of (21) is that V elements, as they may occur anywhere, are the unmarked member of the contrast. Therefore, if the CV syntax hypothesis is valid to some extent, CV syntax being a more specific name for Universal Grammar (UG), we may assume it provides for the inner constituency of V elements. C/NP structure, in contrast, would be allowed to vary more freely across particular grammars, not being *a priori* narrowly

specified by UG. I will return to this point, as it is obviously connected with the double face of creole grammars.

What (21) implies, then, is that nominalizations and sentences in argument (C/NP) position should be all right, but NPs cannot be predicates (a V position) unless they depend on a V element, which may remain unrealized since the model allows for empty nuclei.

The first half of the prediction is obviously correct. Nominal equivalents of VPs are commonly observed in natural languages: cf. *The Vandals destroyed Rome* vs. *Rome's destruction by the Vandals*... And there probably is no language that does not authorize sentences in subject or object position: cf. *That it will rain this Summer would surprise me; I don't think it will rain this Summer*. Obligatory *that* in the first sentence is a parochial property of English (or French, etc.). Many languages would say something like *\*It will rain this Summer would surprise me*.

The second half, on the contrary, seems just as obviously false, given the widespread attestation of so-called 'noun predicates' in languages genetically and typologically as diverse as Arabic, Hungarian, Mohawk, Russian and so forth (see Stassen 1997 for a world-wide survey).

It is important to remark, however, that using nouns phrases as predicates seems always to be dependent on quite specific conditions, as opposed to using verbs. In Croft's (2001) terminology, predication is thus the unmarked function of verbs, while it is a marked function for nouns. For instance, noun predicates in Arabic and Russian must be in the present tense; in Hungarian, they must also be in the present tense and in addition the subject must be 3<sup>rd</sup> person singular; in Mohawk, noun predicates must be 'bare', meaning devoid of any kind of determination (see Baker 2003: 7–8). Moreover, one can speculate that, because of countless historical accidents, 'old' languages like the ones just named do not perfectly realize CV syntax as it emerged some 100 000 years ago. (We know, for instance, that noun predicates in Russian are a fairly recent innovation not shared by other Slavic languages.)

#### 4. Creoles as a testing ground

Rather than ancient idioms, then, creole languages ought to be our testing ground. Indeed, as mentioned in the introduction, they are young languages – probably the only young natural languages there are on the planet – which appeared in the true sense of the word at most 500 years ago for the oldest ones, less for many. I say they appeared 'in the true sense of the word', be-

cause their emergence always followed some disruption, some catastrophe. The worst and most emblematical of these was of course the 17<sup>th</sup>–19<sup>th</sup> century slave trade that gave birth to the New World Creoles. But there were more hidden, less murderous disruptions such as indentured labour, severance of traditional links, religious conversion, etc. (see Arends 1995). Their precise relevance in the creolization process is open to debate, but their reality cannot be denied. Because of them, the time when a given Creole appeared can often be dated with sufficient precision, within say a 50 year span, which cannot be done with languages coming from a continuous, or at least less catastrophic process of change, even though the history of the overall process may be reasonably well known. Take, for instance, the evolution from Latin to French.

The fact that such a disruption occurred implies that language transmission was somehow distorted. To put it as broadly as possible, there was a time when the people inhabiting what was to become Creole-speaking territory had native competence in their ancestral languages only and were just coming into contact with the so-called lexifier or superstrate language.<sup>17</sup> Then there was a time when their offspring had full competence in the recently formed Creole and possibly other languages, including the lexifier.<sup>18</sup> (In the New World the ancestral or substrate languages went out of common use entirely, subsisting only as secret cult languages in some areas; on the West Coast of Africa and elsewhere, they remained part of the linguistic landscape.) In between, there must have been a time when a sizeable amount of people, deprived of the ancestral knowledge or having little use for it, while the Creole didn't exist yet, had (not necessarily unique) competence in a system for which 'pidgin' has become the current technical term of designation.<sup>19</sup>

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<sup>17</sup> They came into contact with the lexifier language either because they were, so to speak, brought to it (New World, Indian Ocean) or because it was brought to them (West African coast).

<sup>18</sup> Since the whole process spans three generations at least, such offspring were at least grandchildren. I say 'full' rather than 'native' competence in the Creole in order to reckon with so-called 'expanded' pidgins such as Tok Pisin (see the discussion in Mühlhäusler 1986: ch.1).

<sup>19</sup> By 'little use' I mean that these people, second or later generation, still knew the old tongues, could speak them within small circles, but could not use them for communication at large. This must have been a typical situation among slaves in plantations before the Creoles stabilized.

Given this, two things are open to discussion. First, the duration of this time lapse: was it very short, from one generation to the next as argued by Bickerton (e.g., 1981, 1999)? Or was it protracted over several generations? Both may well be the case depending on circumstances. For instance, there is a convincing argument in Arends (1986) to the effect that the full formation of Sranan must have taken more than 100 years given the demographic conditions of Surinam in the 17<sup>th</sup>–18<sup>th</sup> centuries. Secondly, what was the in-between system exactly? Was it a pidgin in Bickerton's (1998) sense of a non-language proceeding from the protolanguage faculty and having the aspect of a 'macaronic jargon' (see Bickerton 1999)? Or was it a basic variety of the incompletely acquired lexifier language, that is something that qualifies as a language, but with perhaps a number of unnatural features due to imperfect learning (see Becker & Veenstra 2001)? Or again one or the other in different locations, with additional variation in how far removed from natural language the jargon was, or how basic the basic variety?<sup>20</sup>

All these questions delineate what I call the 'relativized universal grammar hypothesis' or RUGH (see Kihm 2000), the modest proposal of which is that creolizers had to avail themselves of the resources of UG to a significantly larger extent than is normally the case in language transmission, but not exclusively. In this hypothesis, UG is conceived of as providing at least a set of specifications for grammatical (i.e. lexical and morphosyntactic) form. In 'normal' situations, these specifications are overlaid by the particular features of the grammar the child is exposed to. The latter will twist the former in all sorts of ways. In exceptional situations conducive to creole formation, however, universal specifications are more likely to make it to the surface relatively unadulterated.

Here I want to dispel a possible misunderstanding. It might look as if I am arguing for the position upheld by, e.g., Bickerton (1984) and Roberts (1999), namely that Creole grammars realize UG's syntactic unmarked options. We have just seen this cannot be true for NP structure, which cannot be said to be unmarked with regard to the lexifier languages.<sup>21</sup> More generally, I am not sure that notions such as markedness or default, whose usefulness in phonology and morphology is beyond dispute, have any relevance

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<sup>20</sup> In a number of situations there is clearly no evidence that a distinct pidgin ever antedated the creole (see, e.g., Bollée 1977). Even there, however, it is hard to imagine how the creole could have geared smoothly to the lexifier, without a restructuring gap due to the very process of untutored language learning.

<sup>21</sup> To be fair, Roberts makes his case on VP structure only. Even there, however, markedness may not be the proper notion as I will argue presently.



to syntax (see Kihm to appear for theoretical and empirical arguments that they do not). Instead I will use the notion of canonicity which I borrow from Corbett (2005).

A canonical structure is one that fully obeys the logical principles according to which such a formal object ought to be constructed given the functions it is supposed to serve. For instance, a canonical paradigm – say for an inflected verb – is a paradigm where each cell is filled by a form distinct from all other forms in the same paradigm.<sup>22</sup> CV syllables are canonical because they include all and no more than what is required to make up a syllable. If recounting real or virtual events is primarily what syntax is about, a canonical syntactic structure might be one in which all relevant participants (recast as arguments) are explicitly mentioned and their mutual relationships and implication in the event being expressed are clearly spelled out.

Canonicity's strong point is that it makes no reference to frequency. Canonical structures are not more frequent or widespread than noncanonical ones – actually they are rather **less** frequent! Neither does it imply anything about relative ease of processing. Canonical structures **are** easy to process since they banish all logical ambiguities. It does not follow, however, that noncanonical structures raise difficulties, as the human mind appears to be very well equipped to deal with formal complexity and all kinds of illogicalities.<sup>23</sup> Canonicity is not an all-or-nothing matter: departures from the canon may be more or less severe. Finally, it does not commit us to any specific hypothesis about innateness. The mental capacity that leads to the production of canonical structures as an option that comes to the fore in particular circumstances is most certainly part of the genetic endowment or genome of the human species (singular or plural). Calling this capacity UG is legitimate as long as one is aware it is shorthand for something whose existence we have good reasons to suppose, which we may tentatively formalize on the basis of available evidence, but the effective representation of which in the mind-brain is so far unknown to us. Canonicity, as distinct from markedness or bioprogramming, thus allows one to remain agnostic about this issue.

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<sup>22</sup> An example of a canonical paradigm is the Present Indicative of 1<sup>st</sup> conjugation verbs in Spanish.

<sup>23</sup> In that way, canonicity might not be terribly different from the notion of 'perfection' put forward in Chomsky (1995) (also see Kihm 2000).

In the cautious way I formulate it the RUGH seems to me to be reasonable, even plausible. On the other hand, C-MC's hypothesis provides us with a picture of what the universal specifications might look like, what I called CV-syntax. Therefore, if it turned out that creole languages respond to the predictions of CV syntax in a more uniform fashion than do non (obviously) creole languages, that would give support both to the reality of CV syntax and to the RUGH, that is a theory of creole formation that makes room for universal factors, although not all the room as a matter of principle.

We will now examine creole languages from this perspective. In particular we want to see how they fare with respect to the second half of the Syntactic Asymmetry Principle (21), which the preceding discussion shows us should be tuned down to read "C/NP elements are not canonical in V position". Our examination will then allow us to assess the meaning of the two-facedness of creole grammars as presented in the introduction.

## 5. Creoles and copulas

A cursory observation reveals that most creole languages show an overt copula in sentences of the type 'X is Y', in which Y denotes a set X belongs to, a property X is said to possess, or a place where X is located. See, for instance, the following examples from a variety of Creoles:<sup>24</sup>

- (22) *Jan se yon bon chapantye.*  
 John be a good carpenter  
 'John is a good carpenter.' (Haitian, Déprez 2003: 141)
- (23) *Samwel a tiela.*  
 Samuel be tailor  
 'Samuel is a tailor.' (Jamaican, Bailey 1966: 65)
- (24) *Di kaafi a kuol.*  
 the coffee be cold  
 'The coffee is becoming cold.' (Jamaican, Bailey 1966: 65)
- (25) *Di tob de ina kichin.*  
 the tub be in kitchen  
 'The tub is in the kitchen.' (Jamaican, Bailey 1966: 65)

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<sup>24</sup> PROG = progressive aspect ; PM = predicate marker (see below) ; 1PL.EXCL = 1<sup>st</sup> person plural exclusive ('we but not you').

- (26) *Am bi pubu.*  
3SG be poor  
‘S/he is poor.’ (Negerhollands, Sabino 1988: 201)
- (27) *Mi a di kining fa kren.*  
1SG be the king of crane  
‘I am the king of cranes.’ (Negerhollands, Sabino 1988: 201)
- (28) *Bumba bin da loo wak.*  
driver be.PAST there PROG wait  
‘The driver was waiting there.’ (Negerhollands, Sabino 1988: 201)
- (29) *Zot le zoli.*  
2PL be nice  
‘You are nice.’ (Reunionese, Caïd-Capron 1996: 180)
- (30) *Uwo be maksut.*  
3SG be satisfied  
‘He is satisfied.’ (Juba Arabic, Miller 2001)
- (31) *Ita kan bineya batal.*  
2SG be.PAST girl bad  
‘You were a bad girl.’ (Juba Arabic, Miller 2001)
- (32) *Bineya takun fi ini.*  
girl of-2SG be here  
‘Your daughter is here.’ (Juba Arabic, Miller 2001)
- (33) *Masta Sak i bos bilong mipela.*  
Mister Jack PM boss of 1PL.EXCL  
‘Mr Jack is our boss.’ (Tok Pisin, Mühlhäusler 1985: 362)
- (34) *Wanpela niuspepa i stap long tebol.*  
one newspaper PM be on table  
‘A newspaper is on the table.’ (Tok Pisin, Mühlhäusler 1985: 362)

The copula is said to be *overt* in these examples because they all include a phonologically realized element the function of which is to make a predicate of what follows it. This element may express more specific meanings such as stability or transitoriness of the property, location, etc., either by itself – e.g., Jamaican *de* and Tok Pisin *stap* only have the locative meaning – or depending on the context. Its lexical category as verb, ‘particle’, pronoun or whatever cannot be determined on a priori grounds, as we shall see. Sen-

tences (22)–(34) are thus non-verbal predicates since none of them includes an actual verb, but they are not noun predicates either as they require the presence of an element which is clearly non-nominal or not fully nominal. This will be explained later on.

This observation is significant, because *absence* or *covertness* of the copula is what we would rather expect given the pidginization or basic variety episode that antecedes creole formation, no matter for how short a time. Lack of overt copula, in other words generalized noun predicate, has repeatedly been reported to characterize pidgins and basic varieties (see Ferguson 1971; Klein & Perdue 1997). Yet, it didn't leave traces in the Creoles.

Not only do Creoles show overt copulas, they may even be richer than their lexifier languages in the number of copulas they have available. Compare, for instance, Jamaican and Tok Pisin with English, Negerhollands with Dutch. The copula in Juba Arabic – predication *be* or locative *fi* – seems to be mandatory, whereas noun predicates are grammatical in the relevant Arabic dialects. Creole languages thus seem to conform to CV syntax not only quite well, but better than do their lexifier languages.

In order to substantiate the observation, I will now focus on the domain I know best, namely West African Portuguese Creoles, in particular Guinea-Bissau Creole or Kriyol (K) and the Sotavento dialect of Capeverdean (KV). The former is especially revealing, as it still shows something of the stages through which CV syntax proceeded to establish its dominion.

## 6. Copular sentences in Kriyol and Capeverdean

### 6.1. Overview

In KV, all non-verbal predicates involving noun or adjective phrases require an overt copula: *e* for permanent ('individual-level') states of affairs, *sta* for temporary ('stage-levels') states of affairs (see Kratzer 1989), as illustrated in the following examples from Baptista (2002: 102) for the first two, and from Gonçalves & De Andrade (n.d.) for the following two:<sup>25</sup>

- (35) *Vieira e diretor di skola.* (KV)  
 V. be<sub>IL</sub> director of school  
 'Vieira is a school director.'

<sup>25</sup> IL = individual-level; SL = stage-level.

- (36) *Gosi Vieira sta diretor di skola.* (KV)  
 now V. be<sub>SL</sub> director of school  
 'Presently, Vieira is the director of the school.'
- (37) *Nha pai e altu.* (KV)  
 my father be<sub>IL</sub> tall  
 'My father is tall.'
- (38) *Djon sta duenti.* (KV)  
 John be<sub>SL</sub> sick  
 'John is sick.'

In the past tense, the IL copula *e* has the suppletive form *era* (cf. *Nha pai era altu* 'My father was tall'), while the SL copula *sta*, which also serves as the locative copula, appears under the inflected form *staba* (cf. *Djon staba duenti* 'John was sick'). The future shows yet another suppletive form of the IL copula, namely *ser*, following the imperfective aspect particle *ta* (see Baptista 2002: 106):

- (39) *Djon ta ser profesor.* (KV)  
 John IPF be<sub>IL,NF</sub> professor  
 John will be a professor.

I gloss *ser* as 'non-finite' (NF) since it must occur in the scope of a tense-aspect particle supplying finiteness. The same construction gives its future tense to the temporary state copula (cf. *Djon ta sta duenti* 'John will be sick').

KV nouns and adjectives therefore belong to the C/NP type mentioned in (21) just as they do in the lexifier language Portuguese (P). The morphological similarity of the two languages is also striking: KV *e* < P *é* 'is', KV *era* < P *era* 'was', KV *sta(ba)* < P *esta(va)* 'is / was', KV *ser* < P *ser* 'be'. I take no stand about whether the similarity is original, meaning that KV is not a 'radical' Creole and never strayed very far from its lexifier language, or whether it results from recent influence. As we shall see later on, *e*, contrary to *sta*, has properties that make it different from ordinary verbs.

Things are more complex in K where more than one stage in the evolution of the language is still apparent. Contrary to KV, basic adjectives in predicate position have type V.<sup>26</sup> This is shown by the fact that they do not

<sup>26</sup> Basic are those adjectives which have surely been in the language since the beginning. Adjectives more recently borrowed from P such as *demokrátiku* 'democratic', *ambiental* 'environmental', etc., are non-basic or learned.

require an overt copula whereas noun phrases and non-basic adjectives do, and they can be in the scope of the same aspect particles as regular verbs, with special semantic effects as shown in the following examples (cf. Kihm 1994):<sup>27</sup>

(40) *Kil lubu branku.*  
that hyena white  
'That hyena is white.'

(41) *Kil lubu fusi.*  
that hyena run.away  
'That hyena ran away.'

(42) *Lubu ta branku.*  
hyena IPF white  
'Hyenas are white.'

(43) *Lubu ta kume karni.*  
hyena IPF eat meat  
'Hyenas eat meat.'

Basic adjectives not in the scope of a tense-aspect particle – or in the scope of a phonologically null perfective particle, depending on your preferred analysis – denote individual, possibly (but not necessarily) temporary properties (cf. (40)). Under the same conditions, activity or accomplishment verbs denote past events (cf. (41)). In contrast, with the imperfective particle *ta* basic adjectives denote essential properties (cf. (42)), whereas verbs take on habitual or iterative meanings, depending on the particular verbs and context (cf. (43) and Kihm 1994 for more details). Past and future will be dealt with later on.

There clearly is an etymological connection between K *ta* and P *estar*/KV *sta*. Yet, *sta* in P and KV goes with temporary properties (cf. (36) and (38)), whereas essential and permanent properties are introduced with *ser/e* (cf. (35) and (37)) which left no trace in K adjectival predicates – although it did elsewhere as we shall see presently. On the other hand, *sta* is only locative in K. Add to this that KV *ta* does not mean the same in adjectival predicates as does K *ta*. The two languages thus present us with rather differently organized predicate systems.

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<sup>27</sup> IPF = Imperfective

Adjectives are usually analysed as a mixed category, both verbal and nominal, [+V +N]. Baker (2003) argues against this analysis. According to him, adjectives constitute an entirely negative category: they lack specifiers, which makes them [–V], and they have no referential index either, so they are [–N]. From the perspective of CV syntax, this means that adjectives, being neither nouns nor verbs, occupy the place of sonorants, which are neither true consonants nor true vowels. Some languages will treat them as V elements, permitting syllabic sonorants (cf. Czech *vlk* ‘wolf’) and/or adjectival predicates syntactically non distinct from verbal predicates; other languages will treat them as C/NP elements, excluding the former and/or the latter. Both treatments occur in creole languages as predicted by the hypothesis.

Actually, to give more consistency to the story, we probably ought to posit that adjectives are not [–N/V], but rather [uN/V], that is unspecified for the features N and V, thus allowing particular grammars to range over all four a priori possible combinations of <+> and <–>.

A quite different approach to the issue, but leading to the same conclusion, is Croft’s (2001) theory of parts of speech as prototypes. He proposes the following table, which I slightly revise:

(44) Semantic properties of prototypical parts of speech (Croft 2001: 87)

|                                            | Relationality | Stativity | Transitoriness            | Gradability |
|--------------------------------------------|---------------|-----------|---------------------------|-------------|
| <b>Objects</b><br>(unmarked nouns)         | nonrelational | state     | permanent                 | nongradable |
| <b>Properties</b><br>(unmarked adjectives) | relational    | state     | permanent /<br>transitory | gradable    |
| <b>Actions</b><br>(unmarked verbs)         | relational    | process   | transitory                | nongradable |

My contribution to Croft’s table is to notate that properties expressed by unmarked or, in my terms, canonical adjectives can be permanent (IL) or transitory (SL). Given this, adjectives share two properties with nouns, and they share two properties with verbs. We thus predict that, if morphosyntactic conflation is to occur, it may proceed in either direction: A > N or A > V. We also predict that conflation may remain partial since adjectives possess

a property they do not share with nouns and verbs, namely gradability.<sup>28</sup> This is indeed what we observe in K: although basic adjectives pattern with stative verbs when predicative, they are still able to directly modify nouns, which verbs, stative or not, can only do via a relative clause construction. See (45):

- (45) *lubu branku ku fusi/sibi...*  
 hyena white that run.away / know  
 ‘the/a white hyena that ran away / knows...’

Noun and non-basic adjective phrases, in contrast, cannot form predicates by themselves. This means that \*/*Jon (un) diretor/* (cf. Hungarian *Péter szabó* ‘Peter is a tailor’) is ungrammatical. I give the well-formed expression in (46):

- (46) *Jon i un bon diretor.*  
 John I a good director  
 ‘John is a good director.’

What is this *i* which I gloss ‘I’ for the time being? Three analyses are possible *a priori*. First, *i* may be the 3SG subject pronoun referring back to *Jon*. It then follows that *Jon* is not the subject of (46), but it occupies a left-dislocated, topic position. (47) is an example in which *i* must indisputably be analysed in this way:

- (47) *I kay.*  
 3SG.SU fall  
 ‘S/he/it fell.’

Notice that *Jon i kay* ‘John, he fell’ is structurally and semantically distinct from *Jon kay* ‘John fell’. Analysing *i* as a pronoun thus implies that, while every nominal expression may be the subject of a verbal predicate, only *i* may be the subject of a noun predicate. This is a surprising constraint. Copula-like 3<sup>rd</sup> person pronouns are indeed found cross-linguistically, but

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<sup>28</sup> This claim must be qualified. Verbs such as *love* are obviously gradable, but they do not denote actions. Likewise, nouns may be gradable, either inherently like *success* or due to special usage as in *His accent is very Oxbridge* (see Huddleston & Pullum 2002: 1643). Again such nouns or noun usages are not canonical.



their occurrence is usually dependent on properties of the noun predicate (such as it being definite), which implies that NP-NP sentences are also available.

A second possibility is for *i* to be a predicate marker (PM) analogous to Tok Pisin or Seselwa *i*, the former probably from English *he*, the latter certainly from French *i(l)* (see Mühlhäusler 1985; Corne 1977), with one difference, though: Tok Pisin and Seselwa PMs must always occur, whereas K *i* only introduces noun predicates. The question then is: what is the difference between a PM and a copula? I will come back to this. Finally, *i* might also be a copula similar and related to KV *e*, both from P *é* 'is'.

## 6.2. I as a pronoun

Given this assumption, we do have a C/NP type element in a V-type, predicative position. The conclusion is supported by the fact that there is a subgrammar of K (call it A for reasons to be made clear shortly) in which past in nonverbal predicates, whether they are basic adjectives or noun phrases or nonbasic adjectives preceded by *i*, is expressed with a postposed morpheme *ba*:

- (48) *Kil lubu branku ba.* (Compare (38))  
       that hyena white PAST  
       'That hyena was white.'

- (49) *Jon i un bon diretor ba.*  
       John I a good director PAST  
       'John was a good director.'

Clearly, *ba* in (48) and (49) has scope over the predicates *branku* and *un bon diretor*, of which it is said they represent a foregone truth when applied to that hyena and to John, both existing in the present. Despite its obvious kinship with the P and KV suffixes *-va* / *-ba*, *ba*'s position in (49) makes it impossible to view it as an inflectional morpheme. We shall rather consider it to be a kind of clitic adverbial particle that may attach to any V-type expression, including verbs as in (50):

- (50) *I kay ba.*  
       3SG.SU fall PAST  
       'S/he/it had fallen.'

Analysing *i* as a 3SG pronoun then turns out to tally with grammar A evidence. Obviously, grammar A runs afoul of CV syntax predictions.

### 6.3. I as a copula

This analysis, in contrast, accords well with another grammar we shall call M, in which one finds an inflected suppletive form of the copula in the past, as in KV:

- (51) *Kil lubu yera branku.*  
 that hyena be.PAST white  
 'That hyena was white.'
- (52) *Jon yera un bon diretor.*  
 John be.PAST a good director  
 'John was a good director.'

Notice that K has only *yera* (sometimes pronounced *era*) in the past for the non-locative copula, since *sta(ba)* is only used for location. Therefore, K cannot distinguish permanent from transitory properties in the past, unlike KV which contrasts *era* with *staba* (also locative) for this feature, as we saw.

Moreover, there is a variant of dialect M, say M', in which *yera* must be considered not an inflected suppletive form, but an allomorph of *i* in a past context, to which *-ba*, no longer an adverbial clitic, attaches as an inflectional suffix. See (53):

- (53) *Kil lubu / Jon yeraba branku / un bon diretor.*  
 that hyena / John be-PAST white / a good director  
 'That hyena / John was white / a good director.'

Correspondingly, the counterpart of (50) in M or M' K ought to be transcribed as below:

- (54) *I kayba.*  
 3SG.SU fall-PAST  
 'S/he/it had fallen.'

The M variety of K is thus identical to KV in this area.

The occurrence of *yera(ba)* in the past leads us to postulate a ‘CV-conform’ grammar in which the *i* of (46) has to be seen as a copula, fully analogous to KV *e*. In other words, (46) is analysed in two different ways according to whether it is generated by grammar A or M. Likewise, comparing (51) with (40) and (48), we are led to assume that two distinct structures underlie the same overt form *Kil lubu branku*.<sup>29</sup> With one structure, *branku* is type V and no copula is present at any level. With the other structure, *branku* is type C/NP and there is an underlying copula that is not phonologically realized for reasons that should be brought to light, but are certainly connected with the coexistence of two grammars. Non-realization may be deemed optional, moreover, if we assume that *Kil lubu i branku*, also well-formed, can be assigned two analyses and two readings according to the grammar that generates it:

- (55) *Kil lubu i branku.*  
       that hyena 3SG.SU white  
       ‘That hyena, it is white.’ (A)

- (56) *Kil lubu i branku.*  
       that hyena be white  
       ‘That hyena is white.’ (M)

Both analyses, *i* pronoun plus noun predicate (A) and *i* copula (M), thus apply equally well to the data, which appear to be partly distinct and partly mixed up on the surface. Speakers having the, usually passive, competence of grammar A describe it as ‘Ancient Kriyol’ (*kriyol antigu*, hence A); the same speakers have a parallel active competence in M which represents Modern Kriyol (hence M). It is therefore a reasonable assumption that the side-by-side existence of two grammars, one CV-conform, the other not, might be a reflection of K’s evolution since it was a pidgin.

The origin of K is indeed quite probably a Portuguese pidgin in use along the Southern coast of Senegal, Casamance, and present-day Guinea-Bissau. This pidgin creolized slowly and never completely. When Guinea-Bissau became independent in 1975, only a minority of the population used K as a native language. For the majority it was something like an expanded pidgin, with various degrees of proficiency in it. This state of affairs has

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<sup>29</sup> Putting aside the irrelevant case where *kil lubu branku* is not a sentence, but an NP meaning ‘that white hyena’.

not changed radically since then. We shouldn't be surprised, therefore, to find remnants of pidgin grammar intermingled with creole grammar.

That pidgin grammar is not CV-conform is also unsurprising if we accept Bickerton's (1998) assumption that true pidgins exploit a pre-language faculty, protolanguage, which appeared before CV syntax finally settled and looks rather more like Asyntactic, one of the alternative grammars devised by C-MC. This faculty is not erased when language emerges: it remains available for situations in which communication through language is impossible because the participants do not share a common tongue.

#### 6.4. I as a predicate marker (PM)

According to Baker (2003) every nonverbal predicate is in the scope of a functional category PRED that may either directly spell out, or not be realized, or incorporate into the nonverbal head. Being a functional category spell-out is what distinguishes a PM from a copula. Strictly defined, a copula is a verb with inflectional and/or syntactic properties which make it a member of at least a subclass of clearly verbal lexical items. A PM, in contrast, is a morpheme which shares with verbs the semantic property of allowing predication, but which differs from verbs in terms of morphological or syntactic features. Now *i* analysed as a verbal element, the *i* of grammar M, shows features which make it different from morphologically canonical verbs. And this is also the case, remarkably, with KV *e*. I shall now review these features.

The first feature, the least crucial, has to do with inflection. In Modern K and in KV, as we know, verbs suffix *-ba* in the past. But K *i* and KV *e*, as we also know, show suppletive (y)era in the past, and *\*i/eba* is totally excluded. There is of course a historical explanation to this fact. Yet, as it is quite unique in K and KV, to the difference of P, it sets *i/e* apart from all other verb forms.

The second special feature of *i/e* is its position vis-à-vis the predicate negation, which is *ka* – probably from P *nunca* 'never' – in both languages and precedes morphologically canonical verbs, including *sta* and (y)era. The negative counterpart of (54) is thus (57), of (37), (58), and of (52), (59):

- (57) *I ka kayba.* (K)  
 3SG.SU NEG fall-PAST  
 'S/he/it had not fallen.'

- (58) *Djon ka sta duenti.* (KV)  
 John NEG be<sub>SL</sub> sick  
 'John is not sick.'

- (59) *Jon ka yera un bon diretor.* (K)  
 John NEG be.PAST a good director  
 'John was not a good director.'

*Ka* follows *i/e*, in contrast, so the negative counterpart of (46) generated by grammar A or M, is (60) and of (35), (61):

- (60) *Jon i ka un bon diretor.* (K)  
 John I NEG a good director  
 'John is not a good director.'

- (61) *Vieira e ka diretor di skola.* (KV)  
 V. be<sub>IL</sub> NEG director of school  
 'Vieira is not a school director.'

Finally, *i/e* differs from other verbal items in the type of subject pronouns it accepts. K and KV pronouns belong to two paradigms: a clitic, weak paradigm and a nonclitic, strong paradigm. Clitic pronouns show subject, object and oblique (post-prepositional) forms (not distinct for all persons). Subject clitics immediately precede the first element of the predicate domain, i.e. the negation, aspect auxiliaries, modals, or the main verb. Nonclitics appear in left- or (rarely) right-dislocated position. They do not distinguish subject, object, or oblique forms – which suggests they bear inherent case or no case at all in contrast to weak pronouns – and their reference constitutes the informational centre of the sentence. (I call 'informational center' that about which the sentence is uttered.) They therefore require a coreferential clitic inside the sentence. All these properties are illustrated in the example below:<sup>30</sup>

- (62) *El i ka yera un bon diretor.*  
 3SG<sub>ST</sub> 3SG.SU<sub>w</sub> NEG be.PAST a good director  
 'As for her/him, s/he wasn't a good director.'

The fact that the non-past copula and the 3<sup>rd</sup> person subject pronoun are homophonous in Modern K is not without consequences. Indeed we expect

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<sup>30</sup> ST = strong; w = weak.

sentences like ‘She is a good director’ to translate as *\*I i un bon diretor*. Such sequences are badly ill-formed, however. (So is *\*E e un bon diretor* in KV.) Instead, Modern K exhibits what looks like a null subject construction:

- (63) *I un bon diretor.* (K)  
 be a good director  
 ‘S/he is a good director.’

In the same context KV uses nonclitic *el*, stripped of its special informational value, i.e. apparently functioning as an ordinary subject:

- (64) *El e un bon diretor.* (KV)  
 3SG<sub>ST</sub> be a good director  
 ‘S/he is a good director.’

K also has this construction (*El i un bon diretor*) as a variant of (63). It is moreover the only available strategy in both languages whenever the subject is 1<sup>st</sup> or 2<sup>nd</sup> person:

- (65) *Amí/Abó i un bon diretor.* (K)  
 1/2SG<sub>ST</sub> be a good director  
 ‘I/you am/are a good director.’
- (66) *Mi/Bo e un bon diretor.* (KV)  
 1/2SG<sub>ST</sub> be a good director  
 ‘I/you am/are a good director.’

The weak subject forms *N* ‘I’ and *bu* ‘you’ (likewise *no* ‘we’ and *bo* ‘you-all’) are strictly excluded in this context. Compare (67):

- (67) *Amí/Abó N/bu yera un bon diretor.* (K)  
 1/2SG<sub>ST</sub> 1/2SG.SU<sub>w</sub> be.PAST a good director  
 ‘As for me/you, I/you was/were a good director.’

Without *amí/abó* (67) simply means ‘I/you was/were a good director’ (*N/Bu yera un bon diretor*). These special features of K *i* / KV *e*, especially pre-NEG position and the inacceptability of weak clitic subjects, clearly require some explanation. Two may be thought of.

One is radical, and it consists in assuming that in all examples (60)–(61) and (63)–(66), *i/e* is not a copula after all, but rather the 3SG subject clitic pronoun. The default, ‘nonperson’ character of 3<sup>rd</sup> person could then be in-

voked to explain why *i/e* in (65)–(66) is able to refer to 1<sup>st</sup> or 2<sup>nd</sup> person, meaning that (65) is somehow parallel to English *Me, it's different*, in which *it* does not refer back to *me*, but to some proposition about 'me'. This would amount to assuming that K and KV actually remained at the 'pre-CV' noun predicate stage when no overt tense-aspect value bears on the predicate. Such an account, in addition to being semantically unfounded – (65) and (66) **are** about 'me' or 'you' – does not work from a morphosyntactic viewpoint either. If weak, clitic *i/e* 's/he/it' can be the subject of a 'bare' noun predicate, why can't *N* 'I' or *bu* 'you'?

Moreover, why should the strong pronouns lose their informational force before *i/e* just when the latter happens to be the subject of a bare noun predicate, but not when it is the subject of a verbal predicate (see (64))? And why are K and KV not like Hungarian, and why cannot one say \**Jon bon diretor*? What conceivable syntactic or semantic principle could force the insertion of a 3<sup>rd</sup> person pronoun before bare noun predicates rather than simply concatenating them after the subject? Finally, the paradigmatic logic in favour of viewing *i/e* as a member of the copula paradigm in Modern K and KV seems compelling enough.

These pitfalls suggest we should look for a different explanation, such as that proposed by Baptista (2002: 104–110) for KV *e*, which she also compares with K *i* using data from Ichinose (1993) and Kihm (1994). According to her, *e* is verbal in Modern KV, yet it inherited nominal features from the time when it was a pronoun as in Grammar A of K. In other words, KV *e* constitutes a kind of asymmetrical mixed category, primarily verbal and secondarily nominal, which can be represented by the formula [V/n].

I agree with the spirit of Baptista's analysis. However, I think she oversimplifies the course of evolution. My proposal is that the transition from the pre-CV, noun predicate stage in which *i/e* is a pronoun to the final CV stage in which *i/e* is a copular verb may not be immediate, but it may proceed through a stage in which *i/e* is a PM, thus explaining its peculiarities.

## 6.5. From pronoun to PM to copula in K and KV

Above I gave Baker's definition of PMs couched in terms of the Principle and Parameters or Minimalist framework involving hierarchically arranged functional heads. Here I propose a linear definition in the spirit of Kathol's (2000) Linear Syntax, according to which PMs are morphemes the sole function of which is to delineate the predicate domain from the subject domain in the surface, 'flat' representation of the sentence, the only syntactic

level of representation there is in such models. This directly implies that (i) PMs do not have the morphosyntactic properties of verbs and (ii) PMs always occupy a position at the margin of the predicate domain. It may be the right margin as in the following Wolof example, where morpheme boundaries are indicated with hyphens (not present in the official orthography):<sup>31</sup>

- (68) *Góor ñ-ii jaaykat la-ñu.*  
 man CL.PL[+HUMAN]-DEM merchant PM-3PL  
 ‘These men they are merchants.’

*Góor ñii* ‘these men’ may be considered a hanging topic, so the minimal well-formed sentence is *jaaykat lañu* ‘they are merchants’, where the PM *la* stands on the right margin of the basic predicate *jaaykat*, which it thus separates from the enclitic subject pronoun *-ñu* ‘they’. (68) can therefore be represented as in (69), using angled brackets to show the sentence to be an ordered sequence:

- (69)  $\langle_S \langle_{Pred} jaaykat \rangle, la \rangle, \langle_{SU} ñu \rangle$

That *la* does not have the properties of verbs is clearly established by comparing (68) with the sentence below, in which the delineation of the subject *góor ñi* ‘the men’ and the predicate is ensured by the fact that the verb head *gis* ‘see’ and the verbal complex *gis nañu* ‘they saw’ appear at the beginning of the latter, as Wolof is mostly an SVO language:<sup>32</sup>

- (68) *Góor ñ-ii gis na-ñu jaaykat b-i.*  
 man CL.PL[+HUMAN]-DEM see AC-3PL marchand CL-D  
 ‘These men saw the merchant.’

(Notice that *-ñu* is no longer the subject, but it is an agreement marker on the enclitic aspect auxiliary *na*.)

If *i* is a PM in K, then it occupies the predicate’s left margin in accordance with the consistent S-PRED typology of the language (whereas Wolof is only partially S-PRED). As in Wolof, but unlike other Creoles such as Seselwa or Tok Pisin, PM *i* is only inserted when the predicate is headed by a noun, i.e. when syntactic category identification does not suffice to articulate the sentence according to the subject-predicate pattern, since predication

<sup>31</sup> CL.PL = plural noun class; DEM = demonstrative.

<sup>32</sup> AC = accomplished aspect.



is ensured by verbs in the canonical case (see Croft's typology above). Note that present-day Tok Pisin seems to be changing in the direction of K (see Sankoff 1980: 260–270).

This property of *i* being a PM directly accounts for all the special characteristics it presents. Absence of inflection (*\*iba*) is expected since *i* is not a verb. So is position to the left of negation (*i ka...*, not *\*ka i...*) since negation, when present, is always the first member of the predicate domain, preceding even tense-aspect particles.

Incompatibility with weak subject pronouns (*Amí i...*, *El i...*, not *\*N i...*, *\*I i...*) also follows from *i* not being verbal: assume these pronouns include a syntactically assigned nominative case feature for which *i* is not a possible assigner. Strong pronouns, in contrast, contain inherent case as already suggested. Or they contain no case feature at all if we assume a surfacist theory of case in which case is only present if it is morphologically overt in at least some forms of the paradigm. This allows us to extend the account for strong pronouns to full noun phrases, which never show case in K. At the same time the problem with person mismatch (*amí/abó... i...*) vanishes since *i*, not a verb, is not a pronoun either.

Finally, the best account for (63) may be to regard it as a 'bare' predicate, marked as such by PM *i*, in which the absence of an overt subject is interpreted by default as meaning 3SG. As I said, the lacuna may be filled up (*El i...*). The ungrammaticality of the sequence *\*[i<sub>3SG.SU</sub> i<sub>PM...</sub>]* – like that of *\*[i<sub>3SG.SU</sub> i<sub>COP...</sub>]* – might then be due to a phonological constraint barring haplogy (see Arends 1986 on a similar phenomenon in Sranan). And note that 3PL pronominal subjects are obligatorily overt: *Elis i bon diretor* 'They are good directors' with nonclitic *elis* 'they' contrasting with clitic *e* – cf. *E kayba* 'They had fallen'.

Substantial revisions follow from this analysis. Indeed, Grammar A, which I previously assumed to contain only pronominal *i* is actually rendered much simpler and more consistent if one provides it with PM *i* as well. This means that all Grammar A analyses involving *i* and a non-verbal predicate are improved if we take *i* to be a PM rather than a pronoun. I will therefore assume that Grammar A never allowed for bare noun predicates, i.e. noun predicates not delineated from the subject domain by PM *i*.

The question now is whether the identification of K *i* as a PM – I leave KV aside for the moment – is only valid for Grammar A, or whether it also adequately describes Grammar M or a variant of Grammar M. I wish to suggest that both assertions are true. That is, *i* always was a PM in Grammar A, and Grammar M is in a state of flux from an early stage in which *i*

is still a PM to a contemporaneous stage where it is the Present tense form of the verbal copula – where Present tense means nonpast, collapsing actual and general present. Assume then the following paradigmatic statement for the latest state of grammatical affairs:

- (71) In Modern Kriyol (Grammar M) the paradigm of the copula includes three verbal items: *yera* [PAST], *sedu* [NONFINITE], and *i* [PRESENT]

This allows us to characterize the change from Grammar A to Grammar M as consisting primarily in the *integration* of PM *i* to the copula paradigm. An underlying supposition is then that in Grammar A this paradigm only included one item, the ancestor of *sedu* < P *ser* ‘to be’ (compare KV *ser*), and PM *i* was disjoint from it.<sup>33</sup> Then the paradigm was enriched with past *yera*. *Yera*’s adoption progressively expelled noun predicates with adverbial *ba* (see (49)) from the language – they are still understood but very rarely produced – with the result that past noun predicates became formally parallel (homomorphous) with non-past noun predicates: compare [yera NP] with [i NP] on the one hand and [i NP] with [i NP *ba*] on the other hand.<sup>34</sup> Structural parallelism then led to PM *i*’s integration into the paradigm that already contained *sedu* and *yera*. I suppose this integration took place at some point between A and M, producing what may be called ‘Early M’.

Is a paradigm including verb forms and a PM as in Early M canonical? I think it is. Paradigms must not contain opposite values for major class features, that is you don’t make paradigms mixing nouns ([–V +N]) and verbs ([+V –N]), for instance. Now PMs are not specified as nouns or verbs, meaning they are neither <+> nor <–> for these features. On the other hand, they may be said to include a [+PRED] feature that verbs also include. PMs can thus be characterized as [ØV, ØN, +PRED] (Ø = unspecified), verbs as [+V, –N, +PRED]. There is no opposition between <Ø> and <+/->. Therefore, nothing prevents a PM from integrating a verbal paradigm such as the following, where *i* is also unspecified for tense:

<sup>33</sup> Examination of the Casamance dialect of K, which preserves archaic features, suggest that this ancestor had the form *sa* at some point.

<sup>34</sup> *Yeraba* probably represents an intermediate stage in *yera*’s adoption.

(72) The copula paradigm in Early M:

|             | V | N | PRED | TENSE |
|-------------|---|---|------|-------|
| <i>i</i>    | Ø | Ø | +    | Ø     |
| <i>yera</i> | + | – | +    | +     |
| <i>sedu</i> | + | – | +    | –     |

From this paradigm to (71) there is only one step. It consists in valuing [ØV] as [+V] and consequently [ØN] as [–N], thus changing *i* from a PM to a verb, no longer unspecified for tense.

That this change did occur is demonstrated by the array of possible constructions for adjectival predicates, which I list again below:

- (73) *Kil lubu (i) branku.* (A & Early M)  
 that hyena (PM) white  
 ‘That hyena is white.’
- (74) *Kil lubu (i) branku ba.* (A)  
 that hyena (PM) white PAST  
 ‘That hyena was white.’
- (75) *Kil lubu i branku.* (M)  
 that hyena be white  
 ‘That hyena is white.’
- (76) *Kil lubu yera branku.* (Early M & M)  
 that hyena be.PAST white  
 ‘That hyena was white.’

Sentences (73)–(74) with the PM optionally overt are generated by both grammars A and Early M. Non-overtness may be accounted for by assuming that, due to the predicative force of adjectives, no matter whether it is explained à la Baker or à la Croft, the PM need not be inserted before adjectival predicates.

According to M grammar, in contrast, *i* is a verb, albeit with special features inherited from its nonverbal past, that must always be inserted, hence (75). The fact that *i*-less sentences like (73) are still produced today is then interpreted as meaning that this stage has not been fully reached yet, and that Early M grammar is still functional for many speakers. This is precisely where K grammar is still in a state of flux. Because of *i*’s integration

into the paradigm, on the other hand, (74) had to give way entirely to (76). In KV, one only finds the counterparts of (75) and (76), *modulo* the contrast permanent *e/era* vs. transitory *sta/staba* which K does not express in these terms, as we saw.

To conclude, we really ought to ask whether K and KV or the ‘proto-creole’ that probably predated both of them (see Rougé 1986) ever made use of true noun predicates at any stage in their history. Put differently, was the ‘pronominal’ account of (46) (*Jon i un diretor*), which I finally rejected even for Ancient K, ever adequate? I submit it never was, and there is historical evidence to back this up.

It is generally very difficult, most often impossible to go back into the far past of creole languages. With African Portuguese Creoles, we are lucky that we know an indirect forebear, the so-called *Língua de Preto* (LdP, ‘Black Tongue’) spoken by slaves brought to Portugal during the 16<sup>th</sup> century, and rather faithfully transcribed by the great early Renaissance playwright Gil Vicente (about 1465–1537) in his *autos*, that is, comedies staging various characters from everyday life, including African slaves (see Kihm & Rougé to appear). No noun predicates can be found in these texts, but there is an overt copula *sa* which has to be a genuine item given its phonological distance from its probable P etymon *está* and the fact that present-day Gulf of Guinea P-lexifier Creoles have it as their copula, while it only left a few traces in KV and K:

- (77) *A mi sá negro de crivão.*  
to me be black of solicitor  
‘I am a solicitor’s Black man.’

There is even one occurrence of *mi saba* meaning ‘I was’, *eu estava*.

*Língua de Preto* wasn’t apparently a pidgin, even less a jargon like Russenorsk, but it rather was an approximation to P, a basic variety in Klein & Perdue’s (1997) sense, and it was already complex enough at the time when we encounter it in written form. Although we lack documents for this, it is conceivable that *Língua de Preto* was shipped overseas along with slaves brought back to Africa to toil in the new colonies. Did it first (re)pidginize there, or did it evolve right away towards the Creoles/expanded pidgins we know today? There is no answer to this question. Be that as it may, the parallel influx of slaves from the African mainland, makes it very possible that LdP served as a common medium, especially because slaves from Portugal would have enjoyed a superior position from

the start. It would constitute that ‘protocreole’, which later gave rise to KV and K, and which may have been instrumental in the genesis of the Gulf of Guinea Creoles.

We may then accept it as a fact that P-lexifiers Creoles of the Cape-Verde – Senegambia area never had bare noun predicates in their grammars, meaning that *i/e* never was a pronoun in [*i/e* NP] sentences, but it always was a PM.

The homophony of PM *i* and pronoun *i* is intriguing, nevertheless. It actually is the main clue that makes us suspect there must have been a time when *I un diretor* did consist of an NP predicated of a pronoun, before the grammar of what was then a pidgin (or a repidginized basic variety) was realigned onto CV syntax and *i* became reanalysed as a PM.

*I*’s etymology, an interesting topic at any rate, is then likely to be complex. Although a sound change from P *é* to K *i* is quite plausible, one is hard put to understand how the P strong pronoun *ele* ‘HE’ could be the immediate etymon of *i*, European Portuguese being a notorious pro-drop or null subject language that has therefore no subject clitics, a property fully shared by the 16<sup>th</sup>-century language. A more roundabout process is conceivable, however. *Ele*, [el] or [elʲ] in present-day standard European Portuguese, was pronounced something like [ele] with a very closed final unstressed /e/ in Renaissance Portuguese, therefore, one may surmise, in LdP, which shows no tendency to delete final unstressed vowels (see Teyssier 1982; Kihm & Rougé to appear). Now suppose this form was reanalysed (‘metanalysed’) as /el-e/, probably during the transition from pidginized LdP to protocreole, thus simultaneously giving rise to the strong pronoun *el* and to the weak subject pronoun *e*, which remained as such in KV, but was raised to [i] in K. This weak pronoun was then reanalysed one more time, as a PM, in this environment (i.e. [el/NP \_ VP]), while it remained a weak subject pronoun in all other contexts, namely when not preceded by *el* or an NP, or when preceded by clearly dislocated *el* or NP (as intonationally signalled). The subject *i/e* was then integrated to the weak subject pronoun paradigm, which rounded off the divorce of pronominal and PM *i/e*, henceforth homophones. Reanalysing PM *i/e* as copula *i/e* was the last step in this long chain of events, which I illustrate below for the sentence meaning ‘S/he (is) my friend’:<sup>35</sup>

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<sup>35</sup> PRO = strong pronoun; pro = weak pronoun.

- (78)  $Ele_{\text{PRO}} sa_{\text{VCOP}} \text{minha amigo (LdP)} > Ele_{\text{PRO}} \text{nya amigu (pidginized LdP)} > El_{\text{PRO}} e_{\text{pro}} \text{nya amigu (protocreole)} > El_{\text{PRO}} e/i_{\text{PM}} \text{nya amigu (Ancient KV and K; also Early Modern K)} > El_{\text{PRO}} e/i_{\text{VCOP}} \text{nya amigu (Modern KV and K)}$

Two links must be singled out in this chain: (a) between LdP and pidginized LdP; (b) between protocreole and the ancient Creoles. Between (a) and (b) we find pidginized LdP and protocreole, both of which present bare noun predicates; on either side of (a) and (b), there are no bare noun predicates. In other words, pidginized varieties contrast with full language varieties. This suggests that, rather than links, (a) and (b) actually represent breaks and turning points in the transmission: when they pidginize grammars cease to overtly mark the beginning of noncanonical (i.e., nonverbal) predicates; they do it again when they creolize, i.e. become full languages again.

To sum up in a slightly formalized way, (78) spells out the following sequence of grammatical stages:<sup>36</sup>

- (79)  $[S' \text{ NP}_i [S \text{ i}_{\text{proi}} [\text{PRED NP}]]]$  (pidgin)

- (80)  $[S \text{ NP } [\text{PRED } i_{\text{PM}} \text{ NP (ba}_{\text{PAST}})]]$  (Grammar A, Ancien Kriyol)

- (81) a.  $[S \text{ NP } [\text{PRED } i_{\text{PM/COP}} \text{ NP}]]$   
 b.  $[S \text{ NP } [\text{PRED } \text{yera}_{\text{COP.PAST}} \text{ NP}]]$  (Grammar M, Modern Kriyol)

If we assume, which we don't know, that  $[\text{NP NP}]$  (say, *Jon diretor*) was a possible pidgin form as well, then (79), the only stage we are able to reconstruct, may have come about to make parsing easier, a key factor in a pidgin. From the A stage on, the language conforms to CV syntax requirements: true noun predicates do not exist in it, i.e. predicative NPs not in the scope of an overt element whose special function is to bracket the predicate. Predicate bracketing may be the sole function of the element, in which case it is a PM, or it may be one function among several, and we have a verbal copula. The peculiarity of K and KV is that *i/e*, originally a pure PM, was recruited into the copula paradigm without losing some of its PM properties, hence its exceptional behaviour vis-à-vis negation, etc.

An issue I haven't really addressed so far, except in passing, is why does the K/KV PM only bracket NP predicates, whereas similar items in other

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<sup>36</sup> COP = (verbal) copula.

Creoles, e.g. Tok Pisin and Seselwa, bracket all predicates? Integration into the copula paradigm is again the answer: *i/e* is no longer a pure PM, it is one form of BE with PM properties. There are then no more reasons it should bracket verbal predicates than there are reasons for the copula to do so. Full support for this assumption would come from evidence that before *i*'s integration a sentence such as *Jon i bay* simply meant 'John left'. Unfortunately such evidence from Ancient Kriyol is unavailable.

## 7. Conclusion: Creoles as CV-conform languages

PMs and verbal copulas, nearly always peculiar items in some way, make the basic articulation of the sentence impossible to miss whenever the predicate happens to belong to the same category as the argument it is the predicate of. For instance, *Kil lubu branku* (40) is structurally ambiguous out of context, as we saw: it may be a sentence meaning 'That hyena is white', but it may also be a noun phrase meaning 'that white hyena'. In contrast, *Kil lubu i branku* (56) with *i* a PM or a copula is unambiguously a sentence. Similarly, *\*/ña amigu diretor/* might be misunderstood as an appositive NP 'my friend the director', instead of the intended meaning 'My friend is a director' clearly conveyed by *Ña amigu i diretor*.

True, prosodic factors and real discourse conditions make such mistakes unlikely, which may be why many 'old' languages tolerate such potentially confusing utterances. Consider however that creole languages arose in particular circumstances, namely within human groups which had to plod on without a common native language for a sizeable amount of time. The effect, we may surmise, was felt at two levels: at the surface level of parsing where maximum perspicuity probably helped maintain the group in communicatively adverse conditions; at a deep level where more than usual recourse to UG was forced, that is to CV syntax if the previous developments are on the right track.

SVO ordering which K and KV exhibit like almost all Creoles, achieves the same perspicuity effect, as has often been argued. Indeed, given such an order, whatever comes after the initial NP has to be the predicate. Now a crucial notion is that this knowledge of the SVO character of most Creoles may be considered to come for free, since SVO or, more accurately, S-PRED is what comes out from transposing syllabic structure into the syntactic domain. Notice that SOV, the commonest order after (or perhaps before) SVO, also instantiates S-PRED and, as a matter of fact, there are a

couple of SOV Creoles in India and Sri-Lanka (see Clements 1996). But no other ordering is represented. In particular there seem to be no VSO Creoles, even though this order, which implies a superficially disrupted predicate, is not uncommon in 'old' languages.

Tense-aspect particles also contribute to delineate the S-PRED articulation of the sentence. Consider the following K examples:<sup>37</sup>

- (82) *Jon na kasa.*  
 John IPF<sub>[+PUNCT]</sub> marry  
 'John is marrying / will marry.'
- (83) *Jon ta kasa (kada anu).*  
 John IPF<sub>[-PUNCT]</sub> marries (every year)  
 'John marries (every year).'
- (84) *Jon kasa.*  
 John marry  
 'John married.'

Predicate bracketing is unambiguous in (82) and (83) since *na* and *ta* cannot be anything but the first element of the predicate domain, unless they are preceded by the negation *ka* which, being an exclusive predicate negation as we saw, brings about no ambiguity either.<sup>38</sup>

In (84), in contrast, there is no overt predicate left-bracket. This is where the unavailability of a PM because of copula integration makes itself felt (compare Tok Pisin *Meri i danis* 'Mary danced'). To interpret (84) it is therefore necessary to already know that *kasa* is a verb – whereas this can be retrieved 'on line' in (82) and (83) thanks to *na* and *ta*. Of course stored knowledge is required in any event, namely the meaning of *kasa* and that *na* and *ta* are tense-aspect particles with their own meanings. The point

<sup>37</sup> PUNCT = punctual.

<sup>38</sup> A potential problem with (80) is that *na* is also a preposition meaning 'in, on'. In that case, however, it heads a PP which, as a predicate, must be in the scope of the locative copula *sta*, hence *Jon sta na kasa* 'John is in the house', a meaning that cannot be conveyed by *Jon na kasa*, only interpretable as an appositive noun phrase 'John in the house'. Moreover N *kása* 'house' with penultimate stress (cf. P *casa*) is well distinct from V *kasá* 'marry' with final stress (cf. P *casar-se*) in K – although not in the Sotavento variety of KV where both are pronounced *kása* (see Andrade & Kihm 2000).



again is that creole languages evolved in difficult conditions, so that maximizing structural transparency and making parsing almost trivial must have significantly alleviated the task of still incompletely competent speakers-hearers.

With (84), relative transparency was achieved only when the lexicon became paradigmatically organized, that is when verbal bare forms were recognized as members of the same set as forms preceded by a tense-aspect particle. In other terms, language users could tacitly assume that the predicate left boundary stands immediately to the left of *kasa* in (84), because they knew it stands to the immediate left of what immediately precedes *kasa* in the other two members of this form set. To put it graphically, a paradigmatic implicational rule was established that can be represented as in (85):

$$(85) \text{ } [_{\text{PRED}} \text{ na/ta V}] \supset [_{\text{PRED}} \emptyset \text{ V}]$$

Although the psychological reality of  $\emptyset$  in (85) is certainly open to debate, calling it a meaningful void doesn't seem too bold a move.

We thus come up with three properties that conspire to make creole languages maximally CV-conform: predicative NPs obligatorily in the scope of copulas or PMs, tense-aspect particles unambiguously bracketing the predicate domain, and S-PRED ordering. All these properties concern the predicate and its delineation vis-à-vis the rest of the sentence. This is what CV syntax provides for. The inner structure of NPs, in contrast, is indifferent at this level. There is no innate blueprint it has to conform to. Creole NPs therefore are free to vary according to what the respective lexifier or (possibly) substrate languages provide, whereas predicates (VPs) are nearly formally uniform across Creoles.

These, in a nutshell, are the two faces of creole grammar, which the present theory accounts for instead of leaving as a mere unexplained, often unseen fact of life. In turn, interpreting this peculiarity of creole languages as a meaningful property lends support to the CV-syntax hypothesis, i.e. to the assumption that the formal, arbitrary properties of natural syntax are founded on the formal, necessary properties of syllable structure. It even gives us some insight into the specifications of CV-syntax, insofar as it shows the overall structure of the predicate and its delineation with respect to the subject argument to be universally regulated, whereas the inner structure of argument NPs is left to particular grammars to settle, a natural divide that only creole languages bring to light. This, I think, is the most significant result of the present inquiry.

## Appendix: On the perpetuation of complex language

CV-syntax, the grounding of syntax in syllabic structure, is a possible answer to the question: why is human language as it is rather than otherwise? It is **not** a theory of the origin of the language faculty *per se*. Yet, it suggests that when thinking about language origin we are actually facing two problems of different orders: the emergence of the language faculty at some point in the evolution of the human species, on the one hand, and the perpetuation of this faculty under the form of complex language, on the other hand.

The former problem is an empirical one, that may never be satisfactorily solved, but which raises no particular conceptual difficulty insofar as the Neo-Darwinian theory of evolution provides us with the necessary notions to formulate it adequately, namely genetic mutation and exaptation.<sup>39</sup> No such thing can be claimed about the latter problem, however. Why did human beings develop this faculty into complex language and why did they perpetuate it? What advantage did it give them? How did it contribute – in a decisive way apparently – to the survival of the particular human species that did develop and perpetuate it over other human or hominid species that did not?<sup>40</sup> Scenarios have been proposed, but none is convincing. Tool making or communal hunting do not require complex language. It may even be an impediment – remember the last time you tried to build a kit with only the assembly instructions to go by. The management of social relations in hunter-gatherer groups where each member can have direct bodily contact with every other member does not call for it either. Some language is of course useful, but a rudimentary, pidgin-like system would do the job quite well. Why then complex language?

I wish to suggest that creole formation might give us an idea of what a possible answer would be like. As we know, Creole languages – at least the ones we know – appeared under conditions of stress. The stress was enormous (deportation and slavery) or it was somewhat slighter, like having to deal with strange-looking, unsettling, often brutal newcomers as was the case in West Africa or the Pacific. Whenever the stressful situation itself

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<sup>39</sup> The main empirical problem is when exactly the faculty appeared. According to some authors, it may be as old as *Homo Habilis*, that is about 2 million years BP (see Tattersall 1998). The point is that having the faculty does not imply using it to its full capacity.

<sup>40</sup> This disjunction is essential as we shall see presently.

could not be changed and had to be dealt with somehow, developing new vernaculars certainly counted among the spontaneous remedies that helped to make the stress more bearable by enabling forlorn individuals to regain a collective identity and a sense of mutual support. Stress in turn became a crucial factor because it alone, I think, could supply the energy to trigger such an exceptional event as Creole formation.<sup>41</sup>

Imagine now that complex language may also have been a consequence of stress. What was the most terrible stress humankind ever suffered? A fair guess is: realizing we're all going to die. There can't be a more dreadful prospect for a sentient organism than its own annihilation. Of all sentient organisms, only human beings seem to be plagued with this awareness. Of course, we cannot be sure when exactly it settled with all the bitterness it has for us modern humans, and we must be wary of anachronisms. It is a near certainty, however, that the Neandertalians who buried their dead and the contemporary Cro Magnon who painted caves knew they were mortal just as vividly as we do. The fossil evidence about the mental capacities of older species suggests that this awareness may even go as far back as *Homo Habilis*.

Complex language was a response to this predicament. It, and it alone makes narration and the invention of virtual worlds possible. The deeds of the dead could thus be recounted as if they were still with us the living. The possibility of their not being really dead, and therefore of us not really having to die, could be envisioned, told, and passed down. Consoling myths of eternity could be created.<sup>42</sup>

Here, therefore, is the answer I submit to the distinct problem of complex language development and perpetuation: its unique properties such as infinite recursivity, counterfactuals, modalities, nonreferring designators, and so forth, allowed human beings to take refuge in a meaningful world entirely their own where nothing ever fully disappears. These properties in

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<sup>41</sup> In plantation contexts, Creoles emerged and/or took root mainly (perhaps only) in those territories where the maintenance of the slaves or indentured labourers populations was ensured, for a significant amount of time, by renewed arrivals rather than by natural reproduction (see Corne 1999). From a linguistic viewpoint, this meant repeated repidginization and repeated recourse to UG, to the point where the finally resulting Creole was too estranged from the lexifier to be easily absorbed by it. On the human side, it meant enduring stress and misery (See Arends 1995 on Surinam).

<sup>42</sup> Religiously inclined readers are welcome to change the formulation as suits them best.

turn grew and were refined to suit narrative and mythical use instead of remaining unexploited potentialities of the language faculty.

Complex language thus helped and continues to help human beings to endure the obvious shortness of life and the certainty of death. As such, it was and remains a survival factor, the end product of the fortuitous chain of events that first led to the capacity to distinguish consonants and vowels.

The idea that language and narration, including myth making, are inherently related is of course not novel. My contribution is to propose that mythopoesis, far from being just one among language functions, is the *raison d'être* of complex language, and it became vital as a response to the realization of inescapable death. If there is a grain of truth in this hypothesis and if, as seems probable, death awareness is as old as humanity, it follows that complex language – perhaps not so complex as with *Homo Sapiens Sapiens* and *Homo (Sapiens) Neandertalensis*, but complex enough – is much older than commonly thought, perhaps already present in *Homo Habilis*.<sup>43</sup> This in turn strongly suggests that larynx lowering cannot constitute a crucial factor in language emergence. CM-C's central thesis that syntax developed from syllabic structure is not thereby weakened or falsified, however. It simply compels us to look for another evolutionary accident (or a bundle of such accidents) which enabled our forebears to produce an array of distinct sounds that were no longer signals but potential meaningless segments of meaningful signs.

To conclude on a personal note, I hesitated a long time before venturing to write down this idea, especially at the end of what is supposed to be a scholarly paper. In the end, I decided it was not more ridiculous than many scenarios that have been proposed about the same issue, even though it looked crazy (as well as suspiciously simple) when it first occurred to me. I now face the judgment of the readers. If they think it **is** crazy or simplistic (which it certainly is to an extent), complex language will certainly help **me** to live on.

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<sup>43</sup> Since it is not yet ascertained whether Neandertalians were a subspecies of our own or a separate species altogether, I put the '*Sapiens*' within brackets. I assume Neandertalians were endowed with a version of complex language, so their demise is unlikely to be due to Cro-Magnon's being a better talker and/or myth-maker. See the discussion in Lieberman (1984: ch.12); also see Arsuaga (1999); Patou-Mathis (2006); Boë et al. (to appear).

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# **Functional similarities between bimanual coordination and topic/comment structure**

*Manfred Krifka*

Human manual action exhibits a differential use of a non-dominant (typically, left) and a dominant (typically, right) hand. Human communication exhibits a pervasive structuring of utterances into topic and comment. I will point out striking similarities between the coordination of hands in bimanual actions, and the structuring of utterances in topics and comments. I will also show how principles of bimanual coordination influence the expression of topic/comment structure in sign languages and in gestures accompanying spoken language, and suggest that bimanual coordination might have been a preadaptation in the development of information structure in human communication.

## **1. Introduction**

While language is presumably unique to humans, there are possible pre-linguistic features that developed in the course of human evolution which predate features of language, and might have even been essential for its evolution. A number of such possible preadaptations for human language have been discussed, like the permanent lowering of the larynx, the ability to control one's breath, or the inclination of humans to imitate. In this paper I would like to point out another candidate for a preadaptation, namely the functional differentiation of the hands and the way in which they cooperate in manual actions.

To be sure, a number of researchers have tried to establish a relation between (a) the fact that humans show lateralization in their forelimb use to a greater degree than other primates, and (b) the development of the human language faculty, which is characterized by a pronounced lateralization of the brain. For example, MacNeilage (1986) proposed a relation between the form/content structure of human language and bimanual action, and Annett (2002) argues that manual lateralization required cerebral lateralization that,

once established, laid the foundation for the development of language. Here I would like to point out a possible connection not seen so far, namely between the pervasive topic/comment structuring that we find in human language and the functional asymmetry of the hands in bimanual tasks.

I will first remind the reader that topic/comment structuring is indeed an essential and well recognized feature of human language, and characterize its function in human communication. Secondly, I will summarize findings on bimanual coordination which show that the two hands play quite different roles in many tasks that involve both hands. Then I will identify a number of functional similarities between these seemingly widely divergent domains of human behavior, and I will show that these similarities show up when the hands function as organs of communication, as in gesture and sign language. I conclude with a possible scenario according to which asymmetric bimanual coordination played a role in the rise of topic/comment structures in communication.

## 2. Topic/comment structure in communication

### 2.1. Topic/comment structure in linguistics

The structuring of utterances into a topic part and a comment part is a pervasive phenomenon in human language well known to language scholars over the last centuries. It has been identified by medieval Arab grammarians in their distinction between *mubtada* ‘beginning’ and *xabar* ‘news’ as differing from the grammatical subject/predicate distinction, cf. Goldenberg (1988). It was introduced into modern European thinking about language by Weil (1844) as *le point du depart* and *l’énonciation*, and by Gabelentz (1869) and Paul (1880) as *psychologisches Subjekt* and *psychologisches Prädikat*, respectively. It is worthwhile to read the initial attempts to define this fundamental distinction:

There is then a point of departure, an initial notion which is equally present to him who speaks and to him who hears, which forms, as it were, the ground upon which the two intelligences meet; and another part of discourse which forms the statement (*l’énonciation*), properly so called. This division is found in almost all we say.

(Weil 1844/1978: 29)

Evidently I first mention that which animates my thinking, that which I am thinking about, my psychological subject, and then that what I am thinking about it, my psychological predicate.

(von der Gabelentz 1869: 370f., author's translation)

The psychological subject is [...] that which the speaker wants the hearer to think about, to which he wants to direct his attention, the psychological predicate that what he should think about it. (Paul 1880, author's translation).

Marty (1884) questions whether all sentences are structured this way (cf. later Kuroda 1972; Sasse 1987). He distinguishes “categorical” sentences for which this is the case, from “thetic” sentences that do not have a constituent identifying a psychological subject. But even thetic sentences may have a psychological subject that is just not realized as part of the utterance because it is given in the situation of utterance. Marty's remark also suggests a wider notion of potential topics including situations and events.

The psychological subject is not expressed in the sentence *es brennt* ‘there's fire’. But it would be wrong to believe that there is none. In this case we find a combination of two ideas as well. On the one hand there is the realization of a concrete phenomenon, and on the other the notion of burning and fire which already rests in the soul and under which the phenomenon can be subsumed. (Marty 1884: §91, author's translation).

The notion of topic and comment were prominently introduced into American linguistic thinking by Hockett (1958):

The most general characterization of predicative constructions is suggested by the terms “topic” and “comment” [...]: The speaker announces a topic and then says something about it.

It played a central role in the tradition of the Prague School (Firbas 1964; Daneš 1970; Sgall et al. 1986), which tends to use the terms *theme* and *rheme* and identifies them with “old” and “new” information, similar to the influential article by Chafe (1976). However, even though this correlation of Topic and Comment to entities used previously, and to entities being introduced holds in many cases, it is not a necessary one. Halliday (1967) showed that the comment can contain given expressions, and Reinhart (1982) showed that topichood, while strongly correlated with old information, cannot be reduced to it.

Reinhart (1982) also elucidated the notion of topic in terms of a formal model of information and communication. Information can be modelled as a set of file cards that identify an entity and list properties of that entity and its relations to other entities. A topic expression identifies a file card by naming the entity it contains information about, and a comment expression adds information to it. This notion has been made more precise in the framework of file change semantics (Heim 1983) by Portner & Yabushita (1998). Thus, while the two sentences in (1) are true under the same circumstances, they carry different information under normal prosody: while (a) is an utterance about Jacqueline Kennedy, (b) is an utterance about Aristoteles Onassis.

- (1) a. Jacqueline Kennedy married Aristoteles Onassis.  
b. Aristoteles Onassis married Jacqueline Kennedy.

Various authors have pointed out phenomena that are now subsumed under the notion of contrastive topics (cf. e.g. Jacobs 1984, 1996; Lambrecht 1994; Molnár 1998; Büring 1998). What is special about contrastive topics is that they do not only identify an entity about which a comment is made, but in addition signal that, at the current point of discourse, there are other entities about which a comment could have been made which would have resulted in a coherent contribution. Hence contrastive topics indicate that the speaker chooses among a number of alternative topic candidates.

The notion of “topic” has been used in a wide variety of ways, including reference to presupposed information and contextually given expressions, which arguably are phenomena of a different nature. Chafe (1976) and more recently Jacobs (2004) have argued that one should differentiate between a notion of topic that identifies the entity about which a comment is made (the aboutness topic), and another notion that sets the frame for which a proposition holds (the frame setting topic). The following sentence is clearly about Onassis, so *Onassis* is its aboutness topic. The predication is restricted to financial aspects, indicating that Onassis may not be fine altogether; so *financially* is the frame setting topic. However frame setters can be analysed, they are clearly different from aboutness topics.

- (2) Financially, Aristoteles Onassis is doing well.

Frame setters might set a temporal frame (*last year*), a local frame (*in Greece*), a hypothetical frame (*if he had won the election*), and other types that are not easy to generalize about but apparently have important aspects in common.

It is safe to say that the notion of topic/comment structuring, with a number of modifications, refinements and clarifications, has withstood the test of times better than most other linguistic notions, even quite fundamental ones like subject and object, or noun and verb. It is a powerful concept that has been used to explain a wide range of phenomena, from case marking patterns (see e.g. DuBois 1987) to quantification (see e.g. Partee 1991). While it is disputed whether all human languages have a grammaticalized subject/predicate structuring, there is not a single language for which the topic/comment structure has been claimed to be irrelevant.

## 2.2. Properties of the topic/comment structure

While topic/comment structure has turned out to be an important feature of human languages, the forms in which this feature can be realized in particular languages are quite diverse (cf. e.g. Gundel 1988).

In many languages there are specialized syntactic constructions that indicate topics, like the English *as for* construction, cf. (3). Japanese and Korean are well known to have postpositions *wa* and *nun* to mark topics, cf. the Japanese example in (4).

(3) As for the elections, people hope to see more candidates to support these goals.

(4) Sakana wa tai ga ii  
fish TOP red snapper NOM excellent  
'As for fish, red snapper is excellent.'

Also, we frequently find dedicated syntactic positions for topics. The examples in (3) and (4) above illustrate this, as the topic phrases obligatorily occur as sentence-initial, in fact pre-clausal phrases (cf. the ungrammaticality of *\*People, as for the elections, hope to see...*). But frequently, topic positions have been identified in which an expression receives a topical interpretation without any additional marking. In English, left-dislocated phrases, and generally non-subject phrases at the left periphery, are interpreted as topics provided they have no focus accent, as in (5).

(5) a. The Romans, they are crazy.  
b. The next day, we went down to the village.



Left-dislocation is a common way to mark topics (cf. Lambrecht 2001), but there are also languages with grammatical topic positions within the clause. For example, Szabolcsi (1997) identified a sentence-initial topic position in Hungarian that differs from cases like (5b) as it also can identify subjects as topics. Also, Frey (2001) argues for a topic position in the front of the German middle field. What all these findings have in common is that topics tend to occur early within the sentence or within the clause.

Interestingly, this tendency for topic initiality can also be found in the formal language of mathematics. For example, equations are typically given in the form illustrated in (\*). In spite of the commutativity of the equality relation, this is a statement about  $f(x)$ , the value of  $x$  when  $f$  is applied to it, hence this sign typically occurs at the beginning of the equation.

- (\*)    a.  $f(x) = x^2 + 3x + 1$  (usual order)  
       b.  $x^2 + 3x + 1 = f(x)$  (unusual order)

A topic need not assume a grammatical function such as subject or object, witness examples (3), (4) and (5b). However, there is a strong statistical correlation between subjects and topics in running texts (cf. the seminal collections in Li (1976) and Givón (1985)) that suggests that subjects emerged as grammaticized combinations that prototypically combine topic-hood and some semantic role, like agenthood. The tendency for sentence-initial realization of topics then explains why most human languages have, in their basic word order, subjects that are sentence-initial. With the creation of subjects as grammatical pivots, a new device of topic marking becomes available: passive voice, which raises objects to subject position.

Topics typically refer to an entity that already has been mentioned in the previous discourse, is supposed to be part of the common background knowledge of speaker and hearer, or at least construable from known entities, as e.g. *the next day* in (5b). Indefinites may occur as topics in generic sentences, then it can be argued to specify the restrictor set of a generic quantifier, which in itself is topical. For example, (6a) is a statement about potatoes in general, and bare plurals and mass nouns as in (6b) have been analyzed as names of kinds in Carlson (1977) (see Krifka et al. 1995 for discussion).

- (6) a. A potato contains vitamin C, amino acids, and thiamine.  
b. Potatoes contain vitamin C, amino acids, and thiamine.

If the topic is a non-generic indefinite, then it is construed as specific, as an entity that can be identified, but not necessarily by the addressee, as in (7).

- (7) One of my friends had a car accident yesterday.

But many languages disallow indefinite topics altogether, as for example Chinese (cf. Li & Thompson 1981), where indefinite subjects in most cases cannot be sentence-initial.

That topics are given, and hence presupposed, is also the reason for an asymmetry observed by Strawson (1964), who reported his intuition that (8a) has no truth value in our world because the king of France does not exist, whereas (b) is simply false.

- (8) a. The king of France visited the exhibition.  
b. The exhibition was visited by the king of France.

Turning to quantified NPs, such as *every friend of mine*, it has been observed (by Barwise & Cooper 1981) that all natural-language quantifiers have the property that it is sufficient for verifying them to look at the extension of their noun (here: *friend of mine*), and to the VP extension only insofar as it intersects with the noun extension.

- (9) Every friend of mine has sent me a birthday present.

Quantified statements can be seen as topic/comment structures, where the quantifier – here *every* – indicates the degree to which a predication holds – here, a total degree (cf. Löbner 2000). The observed asymmetry has been called “conservativity”. The statement can be verified by first identifying the set of friends of mine, and then checking whether all of them have the property of having sent me a birthday present.

As a consequence of the fact that they refer to given or construable constituents, topics are typically expressed in a prosodically weak way – they are deaccented. This is illustrated in the following contrastive pair of examples. In the context suggested in (10a), *my purse* is not a topic, and it gets an accent; in (b), it is a topic, and it cannot get an accent.

- (10) a. A: What happened? B: My *púrse* was stolen!  
b. A: What happened to your purse? B: My purse was *stólen*!

Deaccentuation may signal topics even in cases in which, for grammatical reasons, they occur in other positions than sentence-initially. One case is the following small text, from Reinhart (1982).

- (11) Kracauer's book is probably the most famous ever written on the subject of the cinema. Of course, many more people are familiar with *the book's* catchy title than are acquainted with its turgid text.

The second sentence is about Kracauer's book. Notice that the topic phrase *the book* is clearly deaccented in this case.

Topics are often pronominalized, as in *it was stolen!*, and in many languages they may be not realized phonologically at all, as e.g. in Chinese. There is one case in which topics receive an accent, namely with contrastive topics. Here, accent indicates that the speaker selects one topic out of a set of several topic candidates. But even in this case the topic does not carry the main accent of the sentence (in the following, ` represents secondary accent, and ´ represents primary accent).

- (12) A: How are your parents doing?  
B: My mòther is still wórking, but my fàther has retired.

Another phenomenon concerning the encoding of topic and comment has been pointed out by Jacobs (2004), who captured frequent findings about topic/comment structuring by claiming that topics and comments cannot be informationally "integrated". On an observational level, this means that topic and comment form distinct phonological phrases. If a sentence like *the train arrived* is meant to be an assertion about the train, it is realized as in (13a), with two phrases each carrying an accent, not as in (13b), with one phrase carrying just one accent.

- (13) a. (the tràin) (arríved)  
b. (the tráin arrived)

Jacobs interprets this as indicating that in the first case, the meaning of *the train* and *arrived* are addressed independently, and then they are combined. In the second case, a simple thought, that an arrival of the train happened, is expressed.

2.3. Is topic/comment structuring necessary for communication?

Topic/comment structuring is so ubiquitous in human communication it may appear a virtual necessity for communication and/or for the storage of information. However, this is not so.

There are simplifying, but quite far-reaching theories of linguistic communication that work without any notion of topic. For example, Stalnaker (1974) suggested a theory of communication in which an information state is a set of situations or possible worlds (the worlds that are compatible with the description of the information state), and updating of this state consists in restricting this set. No notion of topic is employed. Similarly, even though classical discourse representation theory as developed by Kamp (1981) assumes discourse referents in addition to possible worlds, the notion of topic is not required. Of course, there are suggestions how to include topic/comment structuring in the theory developed by these authors, such as Reinhart (1982), Jäger (1996), or Portner & Yabushita (1998). But the point is that they are not essential for the theoretical reconstruction of what happens in communication according to theories like Stalnaker’s or Kamp’s.

Also, in theories of storing and retrieving information in a database, the notion of topichood is superfluous. Consider the following relational database of vulcanoes, dates of their eruptions, and strengths of the eruptions:

(14)

| Vulcano     | Year    | Strength |
|-------------|---------|----------|
| Pinatubo    | 7460 BC | 6+       |
| Sakura-Jima | 3550 BC | 4        |
| Karymsky    | 2500 BC | 5        |
| Pinatubo    | 3550 BC | 6        |
| Sakura-Jima | 2900 BC | 4        |

Is there a “topic column” in this table? It is tempting to consider the names of the vulcanos as such, but observe that names can occur multiple times, just as years and strengths. Also, in database queries there is no dedicated topic:

- (15) a. When did Pinatubo erupt?  
Query: name = ‘Pinatubo’, year = X  
Result: X = 7460 BC, 3550 BC

b. Which volcano erupted around 3550 BC?

Query: name = X, year = '3550'

Result: X = Sakura-Jima, Pinatubo

Typically, a query specifies the values of certain features, while leaving the values of others open. But the constant parts are not in any way topics in the query language. For example, there is no necessity to formulate a query in which items that stay constant come first. The way in which search algorithms work, e.g. for the programming language PROLOG, is blind for the order of specification; the query "year = X, name = 'Pinatubo'" will give the same result as (16a).

In animal communication, topic/comment structuring also seems to be lacking. Animals do not identify an object and then comment on it. It is even questionable whether they can refer to objects in the first place. Tomasello and Zuberbühler (2002) state: "Virtually no ape gestures are referential in the sense that they indicate an external entity (i.e., there is no pointing in the human fashion)." The warning calls of Vervet monkeys signal, for example, "danger from above/an eagle", or "danger from the ground/a snake" (cf. Struhsaker 1967), but they do not first identify a particular region, or a certain type of animal, and then say something about it. Tomasello (2003) notices that chimpanzees produce attention-getting gestures but appear to have no strategy of a combination of such gestures with ones that communicate more specific semantic content that could be seen as precursors of topic/comment structures (cf. also Tomasello, this volume). The only instance remotely comparable to topic/comment structuring I am aware of occurs in species that are very far removed from humans (T. Fitch, pers. comm.): there is some justification to see a topic/comment structure in bee communication, as they bring a sample of pollen to the hive (the topic) and indicate with their dance the direction and distance where more of it can be found (the comment).

This contrasts drastically with human communication, for which topic/comment structuring is an essential feature. There is also evidence that topic/comment structuring occurs early and effortlessly in the process of language acquisition; for example, De Cat (2002) adduces evidence that French children use topic/comment structures early on in their second year.

## 2.4. Topic/comment structure and predication

One well-recognized, but still little-understood semantic property of human language is that it consists, to a large part, of predications that have truth values. For example, a minimal sentence like *Mary left* consists of a predicate, *left*, that is combined with a name; the result can be true or false in a given situation. The standard semantic model for this, going back to Frege (1892), is that the predicate is a function that maps entities, supplied by names, to the truth values True or False. As far as I can see, there is no predication in animal communication (cf. also Nehaniv 2005). A Vervet monkey performing a warning call for a snake does not say something like: *Over there, there is a snake*, but rather announces *Snake!*, or *Beware of Snake!*, which triggers a particular behavior in the addressees. Humans can lie by claiming that a predicate applies to an argument, yielding True, where in fact they know it yields False. Animals cannot lie, they only can deceive, e.g. by uttering a warning call where there is actually no warrant for it. To appreciate the difference, consider a house owner who warns a prospective thief by: *I have a dog*. This is a lie if there is no dog. Now consider a house owner who warns by: *Beware of the dog!* This is not a lie, it is a deception.

How did predication develop from animal signalling systems? Surprisingly, this is a question that has hardly ever been asked, let alone answered. Nehaniv (2000, 2005) has suggested that predication emerged from the simple symmetric association of two ideas via a stage in which one idea has a topic role, and the other one is a comment. The genealogy of predication can be sketched as follows, where “ $a + b$ ” denotes symmetric association of ideas  $a$ ,  $b$ , and  $a \leftarrow b$  denotes that an idea  $b$  is commented on an idea  $a$ .

- (16) Stage 1: association between ideas:  
 $Berries + Sweetness, = Sweetness + Berries.$
- Stage 2: topic/comment structure:  
 $Berries \leftarrow Sweetness, \text{ or } Sweetness \leftarrow Berries.$
- Stage 3: predication:  
 $Berries \text{ are sweet, or } Sweetness \text{ is berryish.}$

The starting point is the simple association of two ideas, which denotes that the two referents often occur together, in whichever way. In our example, berries occur where sweetness occurs, and sweetness occurs where berries occur. This is how Hume conceived of association through contiguity (cf. Hume, *An Essay concerning Human Understanding*). This association is

essentially symmetric. In a topic/comment structure, a first element of asymmetry arises: One term refers to an entity given, the other expresses something new. We can say that one idea is “about” another one. In our example, we identify the concept of berries and add the concept of sweetness to it, or vice versa. The relation is easily reversible. It gets solidified in the case of predication, where one idea refers to an object, and the other is predicated about it, for example when we say that berries are sweet. Now the relation is not easily reversible anymore. Typically, we must make use of a grammatically marked nominalized form of a predicate if we want to make it subject, as in *Sweetness is berryish*. Languages might differ quite drastically in how well developed a predication relation they have. There are topic-prominent languages that do not have a well-established subject relation (cf. Li & Thompson 1976), and there are languages in which the distinction between nouns and verbs, the typical categories suited for topics and comments, is less clear, if present at all (cf. Sasse 1991).

Granted that this scenario still does not tell us where truth values came from. But at least it provides a road map for the development of this asymmetry that is essential for truth values. If the combination of two ideas  $\alpha$ ,  $\beta$  leads to a truth value, and if the one idea is simple, then the other one must be conceived of as containing one element that does the combining and mapping to a truth value. As indicated above, the topic/comment structures can be seen as the source of predication.

The claim that there is no predication in animal communication might be questioned on the basis of the evidence for the suggestion of Hurford (2003) that a functional precursor or neurological equivalent of the predicate-argument structure might exist in the visual processing.<sup>1</sup> Researchers have identified a dorsal stream that identifies the location of objects, corresponding to arguments (or more specifically, to argument variables, or deictically identified entities), and a ventral stream that identifies the qualities of objects, corresponding to predicates. While this structure might be a functional precursor of both predicate/argument structure and asymmetric bimanual communication, I would like to point out that the proposal here differs from Hurford (2003) insofar as it concerns communication, and not simple categorization. Communication is seen as an action that dynamically changes the information content of the common ground, just as manipulation is an active process that changes the properties of entities in the environ-

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<sup>1</sup> Thanks to an anonymous reviewer for making me aware of this connection.

ment. Categorization, on the other hand, is a more passive in that it adjusts the information state of an individual to its environment.

Nevertheless, there is an obvious connection here: The way how the common ground is changed may reflect the predicate-argument structure rooted in more elementary features of categorization. In the hypothetical development of (16) we have assumed, with Hume, that it all starts with a symmetric association of ideas, like *Berries + Sweetness*. This may be wrong if one “idea” is deictically identified, as in *This is sweet*. A paraphrase like *Sweetness is this-ish* is impossible. Even the periphrase *Sweetness is berry-ish* is strange, as we normally use nouns in a deictic function.

## 2.5. Recursivity of topic/comment structure

The way how asymmetric bimanual action was characterized so far does not allow, in a straightforward way, for recursivity, as humans only have two hands for manipulation, with at most ancilliary functions assigned to the feet.<sup>2</sup> Topic/comment-structure in communication is also typically non-recursive. For example, it has been observed that *wa*-marked NPs rarely occur in embedded clauses in Japanese. However, we do find cases that can be understood as recursive topic/comment structures, as in the following example:

- (17) As for my siblings, my sister lives in Lithuania, and my brother lives in Armenia.

Here, *as for my siblings* constitutes the general topic, and *my sister* and *my brother* constitute subtopics. The comment to *as for my siblings* is the rest of the sentence, which itself consists of two topic/comment structures.

Such topic/comment structures and the way how they structure human discourse of have been investigated by a number of researchers, such as van Kuppevelt (1995), Roberts (1998) and Büring (1998, 2003). Typically, the topics in such cases are related to each other, e.g. the referent of *my sister* is a part of the referent of *my siblings*.

While recursivity of topic/comment structures may not directly follow from manual action, it is evident that once it is established in communica-

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<sup>2</sup> As the anonymous reviewer points out, this is different with non-human primates.



tion, the general feature of human language of allowing for recursivity (cf. Hauser e.a. 2002) can affect topic/comment structures as well. In this sense, recursivity of topic/comment structures does not contradict the idea that it is originally derived from a non-recursive process.

### **3. Bimanual coordination in human action**

#### **3.1. The evolution of manual laterality and language**

One of the striking features of human behavior is the differential use of the hands. In all current human populations, most people use their hands in distinct ways for a great number of tasks, like throwing stones, removing a tick, eating with a spoon, or writing with a pen. This has led us to speak of a dominant hand and a non-dominant hand. In all human populations, most people will prefer to use the right hand for such tasks, and this can even be reconstructed for much of human history (cf. Faurie & Raymond 2004, who give an overview and report results, in particular, of hand prints at paleolithic cave sites). Statistics about handedness are surprisingly unreliable because different tasks were considered; they vary between 5% and 20% of left-handers in given populations. There is a genetic factor involved that is still little understood, as monozygotic twins can exhibit different handedness (see Annett 2002; Corballis 2002, 2003 for genetic explanations).

For non-human primates there are reports about asymmetry in hand use, but it is considerably weaker, and there is ongoing debate about this issue. MacNeilage (1984, 1990) finds evidence for a successive development in primates: Prosimians have a left-hand preference for manual prehension, whereas the right hand is used for clinging to branches. There is no real bimanual coordination yet. Monkeys appear to have a weaker left-hand preference for grasping, and a right-hand preference for manipulation, presumably acquired because clinging to trees became less important and freed the right hand for other tasks to some degree. Apes show this tendency even more pronounced: The left hand tends to be used for prehension or other tasks that make strong visuospatial demands, whereas the right hand is preferred for manipulations like joystick-controlled computer games. Schaller (1963) reports that gorillas prefer the right hand to initiate chest-drumming, which functions as a dominance signal. Hopkins et al. (2005) found that captive chimpanzees predominantly use the right hand in pointing to desired objects that they cannot reach without help by the experimenter. But Palmer (2002) criticizes research on handedness in apes quite generally

as inconclusive. In any case, it seems clear that the lateralization of hand use is considerably farther developed in humans than in non-human apes.

Manual lateralization has been related to the other well-known lateralization in humans, the location of speech in the brain. A causal link between these domains was suspected already by Broca in 1865, and is supported by various types of evidence. For example, Rasmussen and Milner (1977) have shown that left handedness is positively related to right-cerebral dominance for speech, and Knecht *e.a.* (2000) have shown that left cerebral activation during word generation is positively related to the degree of right-handedness. Manual lateralization has been implied in the evolution of language. Annett (2002) and McManus (2003) assume that the same genetic mutation is responsible both for handedness and brain lateralization, thus enabling the development of human language; also, MacNeilage *e.a.* (1984) consider manual lateralization a precursor of the brain lateralization necessary for the development of human language. Furthermore, there is evidence that the homologue of Broca's area in monkeys and apes (area F5) contains mirror neurons that are important for the perception and interpretation of manual actions and grasping, which Corballis (2002, 2003) took as evidence for a gestural language that predates spoken language in humans, an hypothesis also advanced by Kendon (1991), Kimura (1993), Rizzolatti & Arbib (1998) and McNeill (2005). In addition, there is evidence that the dominant hand is used more frequently when gesturing, in particular when gestures accompany speech (*cf.* Kimura 1973). This even holds for apes; see Vauclair (2004) for a recent overview of research results.

### 3.2. Asymmetric bimanual coordination

There is a general shortcoming in the traditional view of manual laterality, which assumes that one hand is doing the job and the other is just an appendix that is used for ancillary tasks in case a second hand seems useful. This view dismisses the differential function of the two hands in bimanual action. As a matter of fact, both hands have similarly important functions in many tasks. For example, in the eight tasks used by Annett (1967) to determine handedness, five refer to acts like sweeping, striking a match, using scissors or threading a needle that crucially require an intricate coordination of the activities of both hands. Even for apparently monomanual tasks the non-dominant hand is important, for example in throwing an object, where it is crucial for balancing the body. The role of the non-dominant hand can also

be seen in handwriting, perhaps the classical test for handedness. Athènes (1984) could show that the speed of handwriting reduces by 20% when subjects are instructed not to use the non-dominant hand for fixating and repositioning the paper on which they wrote.

Surprisingly, there are relatively few studies that investigate the importance of coordination of both hands. Perhaps the first one is the Frame/Content Model of MacNeilage, cf. MacNeilage et al. (1984). According to this model, the non-dominant hand holds an object, and the dominant hand acts upon it. That is, the non-dominant hand provides the “frame” into which the dominant hand inserts “contents”. MacNeilage (1986) argues that this is a homologue to the frame/content organization of speech, in particular organization of syllables (frames) and segments (contents), and of syntax (frames) and words (contents). However, MacNeilage (1998) distances himself from this explanation. He argues that no conceivable adaptation regulating hand movements could have been transferred to the vocal system, and suggests instead that the opening and closing movement of the mouth was a precursor to syllable structure. While it is certainly possible to make a strong argument for mandibular motion related to CV (Consonant-Vowel) syllable structure, the frame/content structure relates to other levels of linguistic organization as well that are not directly related to the phonetic realization of language, such as the slot-and-filler structure in syntax and semantics. (In this structure, an intransitive verb like *snore* opens a slot for a subject, and a verb like *hit* opens two slots, one for the agent, and one for the patient). For structures of this sort the cyclic mandibular motion doesn’t seem a more likely precursor than bimanual coordination as sketched above. This holds in particular as there is growing evidence that the supplementary motor area (SMA) close to Broca’s area is involved both in the planning of hand movements and in speech production; also, as mentioned above, the physiological homologue of this area in the monkey brain, F5, contains neural networks relating to manual actions such as grasping and manipulating an object, as well as the corresponding mirror neurons (cf. Rizzolatti & Arbib 1998; Alario et al. 2006).

A second study stressing the specifics of bimanual coordination is Guiard (1987). In his Kinematic Chain Model, he argues for a differential role of hands seen as “motors” that form a “kinematic chain”, following three principles:

- a) Spatio-temporal reference of motion. The motion of the dominant hand typically finds its frame of reference<sup>3</sup> in the results of motion of the non-dominant hand. For example, the nondominant hand fixes the position of an object, whereas the dominant hand manipulates it. Examples are threading a needle, positioning paper in writing, or handling the cue in billiard. Notice that these observations correspond to the frame/content model of MacNeilage.
- b) Spatio-temporal scale of motion. The non-dominant hand produces motions on a more coarse-grained scale in time and space, whereas motions of the dominant hand are quicker and more precise. Experimental evidence for this includes pointing, finger tapping and tracing of points with a cursor. This is consonant with the postural role of the non-dominant hand and the manipulative role of the dominant hand.

A particularly interesting example is playing the violin: In spite of the high additional demand on finger coordination, it is the nondominant hand that is used for holding the violin, thus providing a frame of reference for the bow held by the dominant hand, in addition to providing a frame of reference for its own fingers. This follows from (a). But the conventional way of holding a violin runs against (b), as the finger movements of the non-dominant hand are more rapid and more precise than the bow movements of the dominant hand. This might be tentatively interpreted by stating that frame issues are more important than issues of speed and precision, as far as hand alignment is concerned.

- c) Precedence of non-dominant hand in action. The contribution of the left hand to a bimanual action starts earlier than the contribution of the right hand. The non-dominant object first has toprehend the object before the dominant hand can start manipulating it. In addition, during the action, the non-dominant hand often repositions the object while the dominant hand pauses and gets into action only after the object is in the desired position.

Viewed in this way, bimanual coordination shows surprising similarity to topic/comment articulation, to which we turn in the next section.

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<sup>3</sup> Note that this notion of reference is different from the one used before, of referring to an object.

#### 4. Bimanual coordination and topic/comment structuring

##### 4.1. Similarities between bimanual coordination and topic/comment structuring

It turns out that there are a number of similarities between topic/comment structuring and asymmetric bimanual coordination, as seen in the Frame/Content model or the Kinematic Chain model.

This is quite obvious for frame-setting topics and the Frame/Content model, whose very name captures this similarity. As we have seen, a frame-setting topic identifies a temporal, local or other frame, to which a statement is added that is supposed to hold in this frame, as discussed in example (2). This corresponds strikingly to the way in which the frame/content model viewed the interaction of the two hands, one providing a frame into which another adds content.

There is also a natural interpretation for aboutness topics from the viewpoint of the Kinematic Chain model. As we have seen, the aboutness topic “picks up” or identifies an entity that is typically present in the common ground of speaker or hearer, or whose existence is uncontroversially assumed. This corresponds to the preparatory, postural contribution of the non-dominant hand when it reaches out and “picks up” an object for later manipulation. The comment then adds information about the topic, which in turn corresponds to the manipulative action of the dominant hand. The file-card metaphor of Reinhart (1982) expresses this similarity nicely: The speaker, as it were, takes out the file card with the non-dominant hand, and writes down information on it with the dominant hand.

This description of topic selection and comment attribution is compatible with the fact that sometimes new information is added when selecting a topic, as in the following example:<sup>4</sup>

- (18) A: Did I tell you about my new neighbour?  
       B: Who is it?  
       A: Well, she / the bastard is a professor of Oxford.

Choice of *she / the bastard* as topic expressions add new information, about the gender of the referent or the attitude of the speaker to the referent. However, this added information is clearly to be accommodated, and not part of the

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<sup>4</sup> As suggested by the anonymous reviewer.

main message. For example, if B says: *No, that's not true*, then B denies that the referent is a professor of Oxford, not the gender or attitude information.

Beyond these general aspects of similarity, there are a number of more specific points. One concerns the temporal sequence of hand movements and topic/comment structures. As we have seen, the actions of the non-dominant hand typically precede the corresponding motions of the dominant hand in bimanual manipulations. This directly corresponds to the typical temporal order in which topic/comment-structures are serialized, with the topic being mentioned first, and then elaborated by the comment. A second point of similarity concerns the scale of motion. We have seen that the motions of the non-dominant hand are more coarse-grained, whereas the motions of the dominant hand tend to be on a more fine-grained scale, both spatially and temporally. In addition, the movements of the dominant hand are more frequent, and generally expend more energy. This is related to the realization of topic/comment structure, where the topic tends to be de-accented, and the comment typically bears more pronounced accents. Furthermore, notice that the prehension of an object by the non-dominant hand is, in a sense, static, as it does not affect the internal nature of the object. This is only done by the manipulation of the object by the dominant hand. Quite similarly, identifying a topic does not change the information state yet, but only prepares a change; the change itself is executed by the comment.

#### 4.2. Hand Dominance in Sign Languages and Gesture

If there is a relation between hand dominance in bimanual action and topic/comment structure, we should expect to find evidence for it in sign languages, which use hands to communicate, and also in gestures that accompany spoken language. Unfortunately, only few studies in these two active fields of research have recorded the hand dominance of subjects, let alone have formed hypotheses about differential roles of the dominant and the non-dominant hand in communication.

For sign languages, Sandler (2005) summarizes findings about the differential role of dominant and non-dominant hand. The non-dominant hand appears to play a rather minor role in lexical representation. It is largely redundant, but plays a supporting role in a restricted number of handshapes. In particular, for bimanual signs it often forms a "place of articulation"; the dominant hand moves towards the non-dominant hand. This is very similar to what we find in manipulative bimanual coordination. The nondominant

hand may also function as a classifier that signals the semantic class of a participant, for example in the combinations of the signs APPROACH (dominant hand: pointed finger) + PERSON (non-dominant hand: imitation of walking). Again, this can be related to the frame/content distinction, with the more general classifier providing for a frame. Furthermore, the non-dominant hand marks prosodic boundaries by the so-called hand spread that is quite similar to intonational phrasing in spoken languages.

In addition to the functions mentioned above, the non-dominant hand is used to express discourse coherence. Gee and Kegl (1983) observe that a classifier signed by the non-dominant hand can be maintained while the dominant hand signs new information which is understood to be focused. Emmorey & Falgier (1999) describe such a case in American Sign Language in which a classifier is signed with the non-dominant hand as a kind of backgrounded discourse topic:

- (19) My friend has a fancy car, a Porsche.

[Sign: Classifier for car, non-dominant hand, kept throughout the following.]

(She) drives up and parks. (She) enters a store, does errands, and when finished, she gets back to her car and zooms off. [Classifier signed with non-dominant hand moves away.]

Leeson & Saeed (2004) report related cases from Irish Sign Language, in which the topic sign is maintained by the non-dominant hand. Consider the following example, where nd and dh refer to the nondominant hand and the dominant hand, respectively.

- (20) HOUSE nd HOUSE  
dh TREE (be-located-behind)

“HOUSE is (...) topicalized. The informant holds the sign for house with his non-dominant hand to maintain the referential status of the topicalized constituent. HOUSE is normally articulated with two hands, as in the initial sign. A one-handed version of the normally two-handed sign TREE also occurs with this segment. The signer articulates this with his dominant hand, thus indicating that this has assumed higher informational status (i.e., this is new information) than the preceding constituent, HOUSE.”

Liddell (2003) devotes a whole chapter to what he calls “buoys”, signs produced by one hand that are kept constant, serving as conceptual landmarks

while the other hand continues to sign. This includes signs that structure discourse, like the “list buoy” used to list a number of elements in a discourse sequence, a “theme buoy” by which the non-dominant hand identifies a topic of discourse, and a “pointer buoy” that points at objects that are of longer-lasting interest for a stretch of discourse and seem to be commented upon in the discourse. It is, not surprisingly, always the non-dominant hand that signs buoys.

Something quite similar has been reported for gesture accompanying spoken language by Enfield (2004). This article describes a gestural sequence called “symmetry-dominance” in the description of fish traps by Lao fishermen that may turn out to be much more widespread, if not universal. The sequence consists of two parts. In the first part, a bimanual symmetrical gesture describes the shape of an object (here, a particular type of fish trap). In the subsequent second part, one hand holds the position, representing topical information, and the other hand executes a new gesture that represents new or focal information, that is, the comment. Consider the following example for illustration:

(21)

| HR (dominant)                                                                    | HL (non-dominant) | Speech                                                                               |
|----------------------------------------------------------------------------------|-------------------|--------------------------------------------------------------------------------------|
| Depicting trap move forward as if being placed.                                  | = HR              | ‘And (they) place it in the rice fields, also.’                                      |
| fish swimming into trap                                                          | HOLD as previous  | ‘Now, when a fish is going to go down (into it) ... it goes in and is inserted there |
| fish coming out of trap, hold outside trap                                       |                   | ‘and it can’t get back.’                                                             |
| fish going inside trap, with repeated movement of ‘jamming’, holding inside trap |                   | ‘(It) goes in and gets jammed in there.’                                             |

The hand that holds the position quite evidently sets a frame in which the information that corresponds to the other hand has to be interpreted. Interestingly, it is always the non-dominant hand that keeps the position, and is associated with that frame-setting function.

It should be stressed that while there are highly relevant cases of asymmetric use of the hands in signing and gesturing, hand movements are very often symmetric, and often only one hand is used, especially if the other engages in other, non-communicative abilities. Hence effects of topic/comment structure on signing and gesture will be subtle, and carefully designed



experiments will be necessary to establish or refute this association between gesture/signing and information structure. It might also be that information structure plays a role in symmetric gestures that correspond to utterances which cannot be differentiated in topic and comment parts, as in spontaneous expressions of joy, amazement, fear, defense, etc., which often appear to be symmetrical.<sup>5</sup>

#### 4.3. Bimanual Coordination as a Preadaptation for Topic/Comment Structuring?

The similarities between asymmetric bimanual coordination and topic/comment structuring, and the different roles of the two hands in gesturing, suggest that the manual coordination typical for humans and perhaps higher primates may be a preadaptation that facilitated the development of topic/comment structure in communication. The basic idea is this: Humans and their immediate ancestors have acquired or refined, possibly over several millions of years, the ability to manipulate objects by grasping and positioning them with the non-dominant hand, and modifying them with the dominant hand. Once established, this way of handling objects in the real world became the model of the treatment of objects in communication. Here again, topics were picked up freely, to be modified by comments.

This hypothesis is particularly plausible if one assumes a gestural predecessor of human language, as the same organs, the hands, would have been used both for object manipulation and for communication, and we have seen evidence for a differentiated role of the hands in gesturing and signing even today. That there is such evidence is encouraging, as few researchers have explicitly looked at the differential role of the hands in gesture and signing in relation to topic/comment structuring. Investigations aimed at this issue directly might very well unearth further phenomena that point towards a relation between handedness dominance and the manual expression of information structure.

It should be stressed that the hypothesis is not tied to the assumption of a gestural stage in the development of human language. We could also imagine that the way of manipulating objects had led to a particular way of conceptualizing objects as things that can be picked up, held constant, and modified, which then served as a model for communication.

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<sup>5</sup> Thanks to the anonymous reviewer who pointed this out.

As for the neurological part of the hypothesis, there is evidence that the precursor of (parts of) the Broca area was specialized for bimanual action, in particular the sequencing of actions (cf. references cited earlier, and McNeill 2005). Topic/comment structuring is a special case of sequencing, and so a general adaptation designed for the sequencing of manual actions might well have been adopted for this purpose. It would be interesting to find out whether, in addition to the sequencing function, there is evidence for special neural circuitry responsible for the differential use of the two hands in bimanual manipulations, which then might have been co-opted by the newly acquired tasks of the the Broca area, communication.

On a metaphorical level, the similarities between bimanual coordination and topic/comment structuring are quite striking. Just as *homo habilis* can selectively pick up an object, position it appropriately, and modify it in various ways, *homo loquens* can selectively pick up a topic matter and modifying it by adding, changing or subtracting information about it. This is quite different from how most animals deal with the objects in their environment, and it is very different from how they communicate.

## Acknowledgements

I wish to thank the audience of the Blankensee Colloquium, in particular Tecumseh Fitch, as well as Ives Guiard, Peter MacNeilage, Kita Sotaro, Michael Tomasello, Wendy Sandler and an anonymous referee for comments. That bimanual coordination and topic/comment structures have similar features first occurred to me when reading Wilson (1998).

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# Inflectional morphology and universal grammar: *post hoc* versus *propter hoc*

John H. McWhorter

## 1. Introduction

A tradition in theoretical syntax hypothesizes that Universal Grammar includes parameters that differ in terms of the presence or absence of inflectional morphology. More specifically, various researchers hypothesize that inflectional morphology, or its absence, constitutes one of the signals that infants are innately disposed to attend to in setting the parameters that determine the structure of the grammar they are acquiring. Baker (1996) is a particularly explicit and comprehensive argument of this kind, paralleled by the school arguing that inflectional affixation determines whether or not the verb moves to INFL (e.g. Roberts 1993; Bobaljik 1995; Bobaljik & Thráinsson 1998; Rohrbacher 1999, et al.).

Analysts in this tradition have demonstrated robust typological correlations according to the presence or absence of inflectional morphology. As such, the innatist implications that they draw from the data appear logical on their face. However, theorists positing that language is the product of natural selection (e.g. Pinker & Bloom 1990; Bickerton 1990, 1995; Dunbar 1996) assume that a useful theory of Universal Grammar will be compatible with the tenets of modern evolutionary theory.

I will argue that any conception of Universal Grammar that includes parameters distinguished by the presence, or “robustness”, of inflectional affixes is, despite its *descriptive* attributes, incommensurate with the tenets of genetic inheritance. This implies that the concept of “inflectional parameter” be rejected, and that the descriptive correlations support the claim that a great deal of, if not all, of our language ability is an accidental by-product of the natural selection of other neurological features, as argued by Deacon (1997), Kirby (1999), Lightfoot (2000), and others, as well as Noam Chomsky in passing in assorted writings.

In the discussion that follows, it must be clear that the object of analysis is inflectional affixation, not “Inflection” as an abstract component of syntactic generation.

## 2. Demonstration case: The Polysynthesis Parameter

For example, Baker (1996) proposes that Universal Grammar includes a Polysynthesis Parameter, under which a subset of the world's languages require that arguments can be theta-marked only 1) via incorporation into the verb, as in the noun incorporation typical of his prime demonstration case Mohawk, or – central to this discussion – 2) when co-indexed with an inflectional affix on the verb. Baker formalizes this conception as the Morphological Visibility Condition:

A phrase X is visible for  $\theta$ -role assignment from a head Y only if it is coindexed with a morpheme in the word containing Y via:

- (i) an agreement relationship, or
- (ii) a movement relationship (Baker 1996: 17, 496)

In Mohawk, this property is demonstrated by three sentences that Baker gives (21):

- (1) a. \**Ra-núhwe'-s ne owirá'a.*  
           he-like-      HAB NE baby  
           'He likes babies.'
- b. *Shako- núhwe'-s (ne owirá'a).*  
           he/**them**-like-      HAB NE baby  
           'He likes them (babies).'
- c. *Ra-wir- a-núhwe'-s.*  
           he-**baby**- $\emptyset$ -like-      HAB  
           'He likes babies.'

In (b), the object *baby* is co-indexed with an inflectional affix on the verb (contained within the portmanteau morpheme *shako*); (c) takes the alternative route of incorporating the object (as *-wir-*); (a) shows that using neither strategy, while most immediately intuitive to an Indo-European speaker, is ungrammatical. Baker proposes that various syntactic features, such as absence of adpositional phrase arguments and infinitives, or sharper restrictions on the incorporation of verbs than nouns, follow from the Polysynthesis Parameter.

Crucial to this conception is the requirement that a language with the Polysynthesis Parameter set to "on" have verbal inflectional affixes. The very "agreement relationship" that he stipulates implies this, and all of the

languages that he classes as demonstrating the parameter are highly inflected. Moreover, his hypothesized reconstruction of how a grammar comes to be set with the Polysynthesis Parameter diachronically is centered on the development of inflectional affixes via phonological erosion (Baker 1996: 503).

Indeed, neither Baker nor any linguist to my knowledge has revealed any analytic language with regular and grammatically central noun incorporation. Absent any documented polysynthetic languages in West Africa, Southeast Asia, or Polynesia, it appears safe to assume that inflectional affixation is inherent to languages classed as polysynthetic according to Baker's definition.

And following from this, we assume a logical entailment of Baker's hypothesis: that *sensitivity to the presence of inflectional affixation is part of humans' innate endowment for language*.

### **3. Inflectional affixation as diachronic accident**

In squaring this analysis with evolutionary biology, however, a diachronic perspective is germane. The relevant facts, while seeming at first glance tangential to synchronic analysis, are crucial to situating our conception of Universal Grammar within a Darwinian framework.

#### **3.1. How inflections develop**

A fundamental tenet of historical linguistic theory is that inflectional affixes develop from what begin as free morphemes. The development in Vulgar Latin, for example, of forms of the verb *habēre* into future and conditional marking inflections in many Romance languages is well-known: *amare habeo* "I have to love", i.e. "I will love" > Italian *amerò*. Givon's (1971: 413) well-known dictum that "today's morphology is yesterday's syntax" is an eloquent distillation of historical linguists' basic assumption that inflectional affixes are the product of the phonetic erosion and semantic grammaticalization of erstwhile free morphemes.

This ontogeny of the inflectional affix is further supported by the existence of intermediate forms: clitics. Phonetically less integrated with the root than affixes but subsumed within their intonational contour, intimately associated with certain constituent classes but less likely to select idiosyncratic subclasses of same than affixes (cf. Zwicky & Pullum 1983), clitics

are a “snapshot” of the intermediate stage between free morpheme and affix, in the same way as fossils of *Archaeopteryx*, a toothed, tailed, yet feathered creature intermediate between reptile and bird, show that birds evolved from dinosaurs.

Certainly free morphemes are not the sole source of inflectional affixes. Nichols (1992: 141-2) notes, for example, that nominal gender marking has also arisen via the reanalysis of happenstance phonetic correspondences between nouns. However, in all documented cases, inflectional affixation arises by the recasting of material previously serving in other functions.

Inflectional affixation, then, is the result of historical accident: aspects of human anatomy combine with cognitive tendencies to gradually erode free morphemes and bind them to roots, or to reconceptualize phonetic correspondences, via pattern recognition, as classificational markers.

### 3.2. Inflections: entrenched but unnecessary

Some have argued that inflections, once they develop, are not “useless” but serve various purposes, such as lending a grammar reference-tracking strategies (Heath 1975; Foley & Van Valin 1984: 326). But the fact remains that a great many languages do without the very features these scholars describe (a language can thrive and even acquire world dominance without switch-reference marking, such as the one I am writing in), and even often do without inflectional affixes entirely.

All linguists are aware that many languages are analytic, but less often is it acknowledged that this alone puts into question the idea that inflectional affixation is fundamental to natural language in itself. Even in inflected grammars, inflections’ functional contributions are quite often redundant, serving at best to reinforce relations made clear otherwise. Trudgill (1999) usefully notes that while in many grammars inflections distinguish gender in the first person (Russian *ja znal* “I knew” [masc.], *ja znala* “I knew” [fem.]), this cannot be taken as “functional” given that in oral usage – the original and still fundamental modality of language – this distinction is utterly redundant, the speaker’s gender always being obvious.

This evidence suggests that while inflectional affixes are highly *entrenched*, in the sense of Langacker (1987: 59), they became so in the absence of any functional purpose. They are, in Lass’ (1997: 13) terms, “linguistic male nipples”.

### 3.3. Most aspects of a grammar are not “cultural”

Arguments of this kind make many linguists uncomfortable, in part because of the roots of modern linguistics in anthropology, which stresses language as a reflection of culture. No one could deny that grammars reflect culture to an extent: Japanese and Korean honorifics, for example. However, such connections are unpredictable from one grammar to another: quite a few languages spoken in sharply hierarchical societies lack grammaticalized honorific marking of the Japanese type, such as Indo-Aryan languages, which emerged in caste-based societies.

Thus work such as Wierzbicka's (e.g. 1992) on grammatical reflections of cultural differences between, for example, “Anglo-Saxons” and Slavs, or Perkins' (1980) demonstration that languages spoken by small, less “developed” groups have particularly rich deictic strategies because shared context among intimates encourages this in a way that less contextualized usage by strangers does not, are well-taken. But these observations must be seen in context: they hardly imply that anything but a small subset of a given grammar will submit to analyses such as these. Any realistic perspective on natural language grammars must acknowledge that the bulk of any grammar – morphophonemics, extent of overt marking of semantic roles, word order, ergativity, extractability constraints, phonological derivations, and most of the things that one attends to in controlling a grammar – has arisen quite disconnected from the culture of its speakers.

Hill (1993: 451–452), for example, argues against the notion that polysynthetic structure is the product of small societies based on intimate relations, the volume of shared information presumably allowing the null anaphora, loose word order, absence of a distinct subordinate clause structure, and other “pragmatic mode” features (in Givon's (1979: 229) terminology). The Aztecs created a hierarchical civilization, and yet Nahuatl is polysynthetic; meanwhile, languages of Southern Australia are not polysynthetic, despite their speakers having been small bands of hunter-gatherers. Baker (1996: 510–511) presents various features of Mohawk society and pointedly observes “Does it follow from any of these features of their traditional society that their syntactic constructions should be consistently head marking?” In general, it is impossible to propose a cultural aspect uniting Germans, people of India, Ethiopians, and the Japanese that would predict verb-final word order, or one uniting Celts, Polynesians, and inhabitants of the Philippines that would predict verb-initial.

Few if any linguists would contest any of these observations. And they highlight the simple fact that inflectional affixation is no more “cultural” than any number of other grammatical features, and more importantly, that it is on the contrary a random, accidental development in natural languages.

#### **4. Inflection-based parameters: The collision with evolutionary theory**

If we acknowledge that inflections are linguistic “male nipples” that do not reflect culture in any significant way, then we are in a position to realize that conceptions of Universal Grammar based on the presence of inflectional affixes run up against the tenets of evolutionary biology.

##### **4.1. Inflectional affixes lend no survival advantage**

Evolution, under the Darwinian conception that all biologists consider valid regardless of controversies over the details, is driven by the selection of features that lead individuals to propagate their genes to future generations, as the result of greater fitness for their particular environment. The first problem, then, for a theory proposing an innate capacity for producing and processing inflectional affixation is the sheer number of languages that lack inflections. Presumably, we might assume that if inflectional affixation itself conferred an advantage in survival, then all languages would by now long have become inflected. But obviously they have not, nor do human groups demonstrate a correlation between inflectional affixation and historical success: the Chinese languages are an obvious example, as are the Polynesian ones.

Nor would an argument that modern technology largely exempts today’s humans from the exigencies of evolutionary biology stand. Even in societies less developed today, we find polysyntheticity, fusional and agglutinative inflection, and outright analyticity distributed randomly across the globe.

##### **4.2. Was universal grammar “set” after inflections had already arisen?**

One way around this problem would be to surmise that Universal Grammar happened to be incorporated into the human genetic code only after the first language (or languages) had developed inflectional affixes via drift. But this scenario is again decisively incompatible with a survivalist imperative.

If UG was set after inflections had already arisen, then we would presumably predict that when languages were born anew as creoles, they would be inflectional – and that perhaps analytic languages arise when natural processes of erosion eliminate affixes.

But that prediction is not borne out. Many creoles have no inflections at all, such as the creoles of Surinam like Sranan and Saramaccan, or those of the islands in the Gulf of Guinea such as the Portuguese-based creoles Angolar and São Tomense. Others have one or two, such as Tok Pisin and its sisters in New Guinea and the South Pacific, or perhaps, depending on one's definition of inflectional, many French creoles, whose verbs are often realized in short and long forms depending on context.

Some creoles have more inflectional affixes than this, but this is not a random phenomenon; such creoles result only from contexts in which the languages in contact happen to be closely related genetically, or in which the creole develops in close contact with an inflected older language. Specifically, creoles with a moderate degree of inflection all 1) were created by people mostly speaking the same language who retained some native inflections (e.g. Berbice Dutch), or 2) were born from contact between speakers of very closely related languages whose kinship allowed retention of many cognate affixes (e.g. Lingala), or 3) have long existed in intimate contact with inflected superstratal or adstratal languages (e.g. Sri Lanka Creole Portuguese) (cf. McWhorter 2001).

And the fact remains that whenever a creole has emerged outside of these conditions – i.e. where genetic and typological distance among the creators' languages disallowed convenient compromises, and the creole then went on to develop in a context where there was only enough contact with older languages to yield lexical borrowings – the result has been an analytic language.

Proposing that UG was set after inflections developed leads to another problem as well, in that it would seem to predict that all languages in general would be inflected. Yet they are not, and this is another direct contradiction to evolutionary theory. Biology does not record cases where a feature selected *because of its aid to survival* is then regularly arbitrarily suppressed in some individuals while expressed in others. All sparrows have wings, not just most of them; among populations of venomous snakes, we do not find that only 60% have venom while the remaining 40% mysteriously lack it. A possible exception would be sexual dimorphisms, in which the rendering of an individual as male or female entails the suppressing of certain of its genetically encoded traits. But sex differences are crucial to



the survival of the species, whereas humans accrue no such benefit from the co-existence of analytic and inflected grammars.

#### 4.3. The Polysynthesis Parameter and population genetics

One might object that my characterization of the parameter school's argument as claiming inflections as a survival advantage is a *reductio ad absurdum*. Indeed, more properly, their argument is that the parametrical alternations themselves were selected for; that is, the *choice between* affixation and analyticity, not affixation alone. But even this is a formulation difficult to square with the tenets of evolution currently assumed by biologists.

At first glance, the population genetics model of evolution (e.g. Fisher 1930; Haldane 1932) would appear to provide a way to incorporate the analyticity/inflectionality distinction without appealing to survival advantage. It is now accepted that within a species population, random mutation creates innumerable variations upon genes that are passed down generations expressed only rarely if at all. Those which are disadvantageous to survival when expressed will naturally tend to leach out over time, but many if not most such genes are neutral as to survival advantage. However, if conditions arise that render that mutation advantageous to survival and propagation, then it will increase in the population.

A classic demonstration would be the peppered moth case in late-nineteenth century England, under which it was argued that at first, blackness in the moths was a harmless deviation from a white norm, but when factory soot blackened the tree trunks the moths lived near, the black variant became a norm as their color rendered them less visible to predators. (This particular case itself has actually been rejected as fraudulent [Majerus 1998], but I cite it for its heuristic usefulness in conveying a general phenomenon whose validity is accepted.)

We might imagine that the Polysynthesis Parameter began as a random mutation in some individuals. It would have arisen while or after the first language emerged, but was unexpressed until phonetic erosion and grammatical reanalysis created inflectional languages compatible with it. This would allow the parameter to emerge unconnected from any survival advantage and yet be innate.

It must first be noted that this scenario strains credibility even in a general sense. Inflectional affixation is, after all, just one of myriad aspects of human language. A population genetics account requires that of all of the

mutations that serendipity could have conditioned upon our innate specification for language, one of them occasioned an especial facility for producing and processing languages where a subset of grammatical items and relations are expressed, usually redundantly, via brief sequences of segments phonetically appended to major category constituents. Stranger things have happened – but the notion does give one pause.

But more importantly: under the population genetics model, selective pressure still plays a crucial role. Selective pressure does not create the mutation, but is indeed what causes a mutation to spread in a population. And this brings us back to the problem that there is no evidence that inflectionality helps humans propagate their genes in any way. Even if we accept that there was an inflectional grammar mutation, what selective pressure would have rendered it – or especially, polysynthesis more specifically – a survival advantage?

#### 4.4. Parametrical Alternations as a Survival Advantage

Baker (2001: 211f.) explores the idea that parametrical alternation itself may have conditioned in-group linguistic traits reinforcing group solidarity, thereby encouraging altruism that operated to the benefit of the species as a whole by enhancing the propagation of genes within all human groups. This idea would demonstrate what biologists today term *species selection*, in which traits are selected for that benefit the survival of the species overall rather than individuals. Thus while longer necks in giraffes' ancestors made the survival of individuals more likely, an ability among a species' individuals to survive in a wider range of environments makes the survival of that species as a whole more likely. But Baker rejects this explanation for parameters as “overengineered” – differences in accent and lexical choice mark group boundaries quite well.

Of course, technically, evolution often “overshoots” the bounds of necessity, male peacocks' tail feather displays being an example. But there is further evidence against parameters as a survival advantage: the geographical distribution of language typologies. Too often vast areas of the globe are covered by uniformly well-inflected languages (all of North America and Australia until Europeans came, or the Bantu-speaking region of Africa), or analytic ones (China, Polynesia, much of the upper western coast of Africa). Nor is there any reason to suppose that this is just a latter-day, “post-evolutionary” development. Obviously, even in the most undevel-

oped, isolated regions of the globe, either highly inflected languages or highly analytical ones are the norm across imposingly broad geographical areas.

#### 4.5. The Polysynthesis Parameter “along for the ride”?

To be sure, nature provides examples of traits that occur in two or more variations from one individual to another, with none of the variations conferring survival advantage. Handedness and blood types are commonly discussed examples. These examples of what is termed *stable polymorphism* could be seen as providing an account for the Polysynthesis Parameter that is compatible with evolutionary theory.

But the fate of inflectionality in human populations over time does not square with even this innatist framework. Handedness and blood type are specified genetically for each individual and emerge regardless of environmental stimulus (although of course societal custom often encourages individuals to suppress left-handedness). On the other hand, all humans are equally capable of acquiring analytic or inflectional languages: the hypothesized parameter is unset at birth, and thus differs sharply from stable polymorphism.

Nor does inflection subsume gracefully under other categories of features inherited independently of selective advantage. Nature displays innumerable phenomena which, in all of their salience and wonder, are ultimately accidents rather than selected features. The hemoglobin molecule in our red blood cells, for example, was selected for because it is useful in carrying oxygen and carbon dioxide. However, the fact that it happens to be red was not selected for. Clearly its color was irrelevant to ensuring that its bearers sired more offspring, and in fact many creatures that have thrived on earth much longer than humans have blood of other colors (Lightfoot 2000: 237). Fin whales’ lower jaws and baleen plates are black on one side and white on the other, and there is no conceivable competitive advantage that this confers: it is apparently simply a happenstance development of their biologies (Ellis 2001: 237). All of the several dozen other whale varieties do quite well with jaws and baleen of just one color. It is a fundamental observation in evolutionary biology that only a subset of a biological organism’s traits are evolutionary adaptations.

But where the Polysynthesis Parameter is concerned, we are dealing not with a single species-wide trait, but a hypothesized innate disposition to manifest either of two variations upon a trait depending on environment. A

theory of inflection compatible with evolutionary biology requires that there be parallels in nature to this particular phenomenon.

#### 4.6. The Polysynthesis Parameter as an environmentally conditioned phenotype

Nature does provide examples of biological “parameters” that are initially unset in the individual, tipped one way or the other by environmental stimuli. In the life cycle of many organisms, sex is determined by environmental triggers. The sex of crocodiles and some other reptiles is determined by temperature at the time of hatching (Madge 1985). If a larva of *Bonellia*, an echiuran marine worm, lands on a substrate it becomes a female, but if an adult female takes it up with her proboscis, it becomes a male (and spends its life in her uterus!). Could it be that phenomena like these are the biological parallel to linguistic parameters, innate either-ors set by what kind of language the individual happens to acquire?

In fact, the fit is extremely approximate from both the synchronic and the diachronic perspective. In nature, environmental factors inherent to our planet (e.g. temperature, habitat) control a distinction between individuals (sex) that is vital to propagation. In language, we would have to propose that an environmental factor that didn’t exist when the species arose and only emerged via devolutional processes (polysynthetic grammar) controls a distinction irrelevant to survival (analyticity vs. polysynthesis).

Obviously, tautology looms large here: crocodile gender is determined by temperature at hatching, but how humans process grammar is determined by, well, what kind of grammar they hear after hatching. But more pointedly, there is a clear lack of fit here between both nature of dimorphism and nature of environment. Once again, the biological phenomena are variations upon the theme of ensuring survival, while the Polysynthesis Parameter is irrelevant to it. The only way we could save the parallel here would be to surmise that the Polysynthesis Parameter simply “happened” for no functional reason, but obviously a more constrained and falsifiable account would be preferable.

#### 4.7. Implications

At the end of the day, attempts to incorporate the Polysynthesis Parameter hypothesis into evolutionary theory are either logically hopeless or, at best,

depend on “just so” stories to a degree that ultimately suggests that analyses beyond the Darwinian will be more useful.

Specifically, the nature of language in time and space is such that the most useful theories will have room built into them for contingency. Lightfoot (2000) refers to reading and writing as examples of a contingencies which, unlike inflections, have even been central in the success of human populations that have developed them. Yet no one would argue that the capacity to engage in them is specifically encoded genetically. Myriad civilizations have thrived in which most citizens were illiterate. Reading, requiring constant focus upon tiny symbols, is ultimately detrimental to our vision. And none would claim that the illiteracy of a great many of the world’s peoples past and present renders them any less human than the literate. On the contrary, since writing only traces back about six thousand years, illiteracy has been the natural state of our species. We naturally assume that our ability to read and write is a contingent development that “piggybacks” on innate traits developed to other ends.

Indeed, modern evolutionary theory assumes that a vast weight of an organism’s traits are random epiphenomena rather than inheritances selected because of advantage to survival. And this means the following: given that such epiphenomena are rife in nature, under any Darwinian conception of human language, *it would be unexpected that language would lack such “accidental” features*. The only question is which features would fall into this class.

In that class, all linguists would include, for example, phonetic shape of a language’s words. But for all of its centrality to linguistic theory, inflectional affixation gives every indication of being another such feature:

- a) It arises via historical accident.
- b) It serves no function in a grammar that complex, nuanced communication requires.
- c) It has no connection to human culture.
- d) It confers advantage neither in survival nor sexual attractiveness.
- e) It is incommensurate with evolutionary accounts for traits inherited independently of survival advantage, including polymorphic ones.

The evidence is compatible, then, with an analysis stipulating that the Universal Grammar that was selected for in human evolution and passed on to all future generations of *Homo sapiens* lacked inflectional affixation (cf.

Comrie 1992 for a related argument). Eventually, a subset of the manifestations of Universal Grammar developed inflectional affixes via inexorable processes inherent to human anatomy and cognition. These grammars were processible by the massively plastic human brain, but had no effect upon our genome.

In broader view, if inflectional affixation is irrelevant to Universal Grammar, then *this disallows the existence of an innate Polysynthesis Parameter* – or any other that requires that sensitivity to inflectional affixes is innate to the human species.

## 6. A potential counterargument: sign languages and inflections

Modern sign languages have inflectional affixation despite having arisen only within the past two hundred years, a life cycle as brief as creoles'. Specifically, in, for example, American Sign Language, the signs for some verbs are expressed simultaneously with physical deictic indications of person and number; e.g. *I give to you* expressed by moving the hand from the speaker's body outward towards the interlocutor's. Many might take this as implying that sign languages, being recently-born natural languages, indicate that inflectional affixation is indeed innately specified.

But to treat sign languages' pronominal inflections as innate predicts that when analytic pidgins were transformed into natural languages – i.e. creoles – that in at least a subset of cases, their pronouns would be manifested as inflectional affixes. But in over fifteen years' research on pidgins and creoles, amidst investigations that have required reference to the grammars of all of the several dozen creoles extant, I have encountered no such case. When Melanesian Pidgin English evolved into creoles like Tok Pisin, its pronouns remained free morphemes; the case was similar in other documented cases of the pidgin-to-creole transition such as the emergence of Sango in the Central African Republic and Hawaiian Creole English from Hawaiian Pidgin English. This is unexplained under a claim that sign languages' verbal inflections indicate an innate property.

To be sure, the pidgin stage of most creoles was not recorded, meaning that in most cases we cannot track the development from pidgin to creole. But there does not even exist a creole with a paradigm of person/number affixes segmentally homologous with its free pronouns, which would be an obvious indication that erstwhile free pronouns had been affixed as those in sign languages are. The one possible exception would be the more standard-

ized variety of Lingala, created by speakers of closely related Bantu languages (the Bobangi cluster) that have subject-marking inflections. The result was a language with a paradigm of subject inflections varying according to person and number. But the source languages here were so genetically close (essentially a dialect continuum) that the result was essentially what most linguists would class as a koine; indeed, Bantuists often treat Lingala as an ordinary Bantu language rather than as a “pidgin” or “creole”.

Technically, there could theoretically be pidgins lost to history that contradict my claim that truly new languages are always analytic. We might surmise that there are pidgins that have indeed arisen with ample affixation, only for these to be, for some reason, transformed into free pronouns in the creole that developed from the pidgin. But such a surmise would be atheoretical: for affixes to evolve into free morphemes would contradict a very strong tendency in grammaticalization in the other direction. The grammaticalization literature notes occasional exceptions (usefully gathered in Newmeyer 1998: 263–275), but mere exceptions they are. As Newmeyer notes (275f.), “a rough impression is that downgradings [i.e. free morphemes becoming inflectional affixes] have occurred at least ten times as often as upgradings [i.e. inflections becoming derivations, clitics, or free morphemes]. The exceptions do not motivate positing a regular development of affixes into free morphemes in the pidgin-to-creole life cycle – which is what hypothesizing inflected pidgins would require.

On the contrary, where the occasional source-language-modelled inflection occurs in a documented pidgin, if a creole develops from the pidgin, the inflection does not drift into becoming a free morpheme, but always remains an inflection in the creole. An example is the transitive marker *-im* in both pidgin and creole varieties of Tok Pisin, which neither here nor in any of its sister creoles has drifted into becoming a clitic or free morpheme. Pidginized Assamese (Naga Pidgin) is an example of a contact language developing in intimate enough contact with its lexifier to lend it some of its inflections. But its creole varieties retain the same inflectional affixes rather than recasting them as free morphemes.

This sharp contrast between the spoken and manual modalities suggests that the inflections in sign languages stem from something inherent to that modality rather than innate to our species. And a ready solution is that the structural nature of human limbs and hands inherently encourages some verbs and their deictic reference to be indicated within the same physical motion. That is, inflectional affixation in sign language gives all indication of being an exaptation – a recruitment in a new function of a trait selected

for other reasons (Gould & Lewontin 1979) – just as I believe inflection in spoken language to be. Our physical makeup allows and even encourages this feature in sign language without there being any motivation for treating it as an innately specified feature of our language competence.

## 7. Another example: the rich agreement hypothesis

### 7.1. The rich agreement hypothesis: stage one

In the 1980s, various researchers converged upon a hypothesis that ample verbal inflectional affixation in a grammar leads the verb to move to INFL, while when verbal inflection is sparse or absent, the verb stays *in situ*. The relevant contrast is illustrated by the heavily inflected Icelandic versus Danish, whose inflectional affixes are invariant across person and number. In subordinate clauses (chosen because in main clauses, most Germanic languages exhibit a V2 phenomenon that obscures the process), verbs in Icelandic precede various elements that occur at the left edge of the VP, such as sentential negators, while Danish verbs occur to the right of these elements:

Icelandic:

- (4) ...*a hann keypti ekki bókina.*  
that he buy.PAST NEG book.DEF  
'...that he did not buy the book.'

Danish:

- (5) ...*at han ikke købte bogen.*  
that he NEG buy.PAST book.DEF  
'...that he did not buy the book.'
- (Platzack 1986: 209)

Various scholars (Travis 1984; Roberts 1985, 1993; Platzack 1986, 1988; Pollock 1989; Vikner 1997; Rohrbacher 1999) have taken this as evidence that there exists a parameter distinguishing richly versus weakly inflected grammars. The hypothesis appeared strengthened by diachronic evidence, where in some grammars, after inflectional affixes wore away, the verb which once moved to INFL came to rest *in situ*, such as Platzack's (1988) demonstration in Swedish and Roberts' (1985) for English.

However, various exceptions came to light: grammars with weak agreement in which the verb nevertheless moves to INFL. The Kronoby, Finland



variety of Swedish was a particularly noted problem (Platzack & Holmberg 1989), another example being the Tromsø dialect of Norwegian, where inflection is as weak as in Danish and yet the verb moves to INFL (Vikner 1995):

- (6) *Vi va' bare tre støkka, før det at han Nilsen kom ikkje.*  
 we were only three pieces because that he Nilsen came NEG  
 'We were only three because Nilsen didn't some.'

(Iversen 1918: 83 cited in Vikner 1995)

Conversely, Faroese, with somewhat less inflectional allomorphy than its close relative Icelandic but vastly more than Mainland Scandinavian varieties, was shown to variably allow the verb to remain *in situ* (Barnes 1987).

Among responses to the new data were attempts to refine the definition of inflectional "richness" in ways that risked ad hocness from a cognitively plausible perspective (e.g. Rohrbacher 1999), and concessions that an empirically responsible reformulation of the "parameter" must stipulate only that rich inflection requires the verb to move to INFL, while weak inflection may or may not do so (Platzack & Holmberg 1989; Roberts 1999: 292).

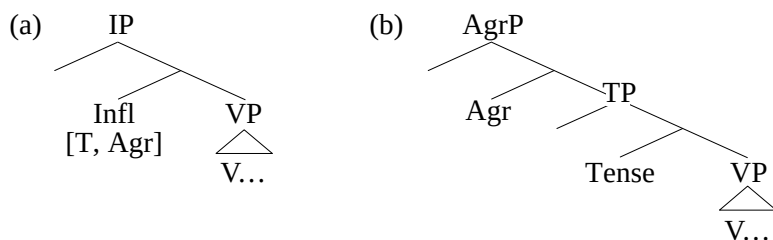
But as Rohrbacher (1999: 147) acknowledges, this latter conception is problematic for a constrained theory of acquisition: "If examples of V to I raising in the input are sufficient to trigger the acquisition of V to I raising, it is unclear why the morphological richness of person or any other agreement should play any role at all in this process". Concurring, Bobaljik (2002) cites evidence given by Lardiere (2000) that indeed, word order alone is acquired by children before they master inflectional affixes.

More generally, this initial version of the Rich Agreement Hypothesis founders upon the same arguments as the Polysynthesis Parameter: both innate sensitivity to inflectional affixation and an innate *option for* inflectional affixation are hopelessly incompatible with an account of language origins couched in Darwinian theory.

## 7.2. The Rich Agreement Hypothesis: Stage two

In response to the exceptions, Johnson (1990), followed by Bobaljik (1995) and Bobaljik & Thráinsson (1998) have sought a resolution in recruiting Pollock's (1989) recasting of the IP into separate agreement and Tense nodes.

The hypothesis is that languages vary in their configuration of the INFL realm: in some, the IP simply manifests itself as an INFL node and a VP node (a), while in others, the IP consists of multiple maximal projections (b):



The proposal here is a “Split IP Parameter”, and its usefulness to retaining a version of the Rich Agreement Hypothesis is that the driving force for verb movement is not inflectional affixes *per se*, but the split IP in the syntax. Crucially, this formulation provides a principled reason for verbs to move even in grammars with weak inflection: split IP is a syntactic rather than morphological feature, and thus a grammar can presumably have a split IP even without rich inflectional affixation.

For adherents of this hypothesis, Kronoby Swedish and Tromsø Norwegian are thus accounted for: despite the erosion of the affixes, the split IP remains, forcing verb movement. Bobaljik (1995) and Bobaljik & Thráinsson (1998) present a principled account for such grammars within the Minimalist framework, under which verbs in languages of the (b) type are forced to move to check their features, because the multiplicity of projections deprives them of adjacency to the verb, in contrast to the situation in languages of type (a). This revision could be taken as resituating the relevant contrasts within an evolutionary framework, given that it removes affixation as the spur for the parameter setting.

### 7.3. Old problems

But viewed more closely, as argued to date even this new version reduces to maintaining inflectional affixation as an innate specification. The relevant linguists are themselves concerned mainly with synchronic issues, valuing the revision for its attribution of the relevant contrasts to the syntactic rather than the morphological realm, and the implications of this for the

Distributed Morphology framework (Halle & Marantz 1993). However, our discussion addresses whether or not this scenario is plausible as a product of evolution, and here, serious problems remain.

For example, Bobaljik (2002) stipulates that even in split-IP grammars where the verb moves despite weak overt inflectional affixation, there remain zero inflectional morphemes in place of overt ones. This alone implies that the learner of a split-IP grammar is still cued by inflectional affixes.

However, Bobaljik does not lay much emphasis upon this presumption. As to the question of what cues the acquirer to set their grammar as one with split IP, he and Thráinsson note other features that split IP conditions. For example, they propose that a split IP grammar, endowed with multiple specifier positions in contrast to simple IP grammars, may recruit these nodes for pragmatic contrasts. A demonstration is Icelandic, where subjects occurring to the left of an adverb connote old information while those occurring to the right of the adverb connote new information (Bobaljik & Jonas 1996: 196):

- (7) a. *Í gær kláruðu {þessar mýs} sennilega\*{þessar mýs} ostinn.*  
 yesterday finished these mice probably these mice cheese.DEF  
 'These mice probably finished the cheese yesterday.'
- b. *Í gær kláruðu {?margar mýs} sennilega {margar mýs} ostinn.*  
 yesterday finished many mice probably many mice cheese.DEF  
 'Many mice probably finished the cheese yesterday.'

Certainly these authors have deftly demonstrated that verb movement need not depend strictly upon affixation *synchronically*, their implication being that affixation is merely one of many epiphenomena of a particular syntactic parameter. But the question is whether their hypothesis requires that the proposed Split-IP parameter was *incorporated as a genetic specification* on the basis of overt inflectional affixation.

And in this light, it is crucial that all of the weakly inflected grammars with verb movement that these researchers present are well documented to have evolved over the past several centuries from languages with ample inflection. Specifically, the case for a Split IP parameter has been to date based on Germanic languages, in various states of evolution from an inflection-heavy Proto-Germanic ancestor.

Demonstrations that some of the weakly inflected among these languages exhibit other evidence of a split IP despite inflectional erosion – such as the transitive expletive constructions grammatical in Icelandic but ungrammati-

cal in Norwegian or English – are well and good. But strictly speaking, these phenomena can be taken as demonstrating that split IP arises first when inflectional affixation emerges, but that its configuration then spins off structural by-products that may, unremarkably, persist even after the inflections erode away. It may well be that the synchronic learner can infer a split IP from these constructions, or simply from the fact that the verb moves in the grammar even when inflection is weak.

But in the evolutionary sense central to my argument, this conception entails that split IP first developed in natural language grammars as a result of the emergence of inflectional affixes. What would belie such a conclusion is grammars assumed to have been analytic for millennia that distinguish new versus old information on the basis of orderings within a multiheaded IP, or other features derivable from split IP other than rich inflection. Only these would clinch a case that split IP is a parametrical setting independent of the development of affixes – but researchers arguing for the hypothesis have presented no such examples to date. All of the Germanic languages are disqualified by virtue of their recent and well-documented descent from a richly inflected ancestor.

In other words, the split-IP hypothesis as currently argued – upon languages where rich inflection is either present or was present only recently – maintains, via logical implication, an assumption that inflectional affixation was a component of the Universal Grammar that was incorporated as an innate feature in our species. Specifically, the arguments give all indication of implying that *split IP arises when inflectional affixation emerges*, not as an independent feature.

Indeed, the very creation of the multiheaded INFL schema was driven not by observations such as Icelandic informational structure contrasts, but by the functional division of labor in inflectional affixes between tense and concord. Icelandic's past tense verbal paradigm is an example, where a segment *-ð-* is treatable as connoting pastness, with further segments corresponding to person and number:

(8) “hear”, past tense

|                  |                  |
|------------------|------------------|
| <i>heyr-ð-i</i>  | <i>heyr-ðu-m</i> |
| <i>heyr-ð-ir</i> | <i>heyr-ðu-ð</i> |
| <i>heyr-ð-i</i>  | <i>heyr-ð-u</i>  |

And in any case, even if we were to accept an argument that the Split IP Parameter alone, unconnected from overt affixation, is a genetic inheritance,

we would be brought before the eternal question: how could either a split IP or the option of having one have possibly conferred a survival advantage upon the human species?

## 8. A thought experiment

At the end of the day, we must acknowledge the risk inherent in native speakers of inflected languages proposing parameters of Universal Grammar based on the presence of inflectional affixation.

This is not to take a page from the traditional cavil that generative syntacticians found their theories upon a mere few European languages. If this was true in the 1960s, it would be difficult to maintain this charge today in view of the typologically broad spectrum of languages that many theoretical syntacticians address, Baker's work on polysynthetic languages being an obvious example. However, in formulations of Universal Grammar that, despite incorporating "exotic" languages, hinge centrally on the presence or absence of features particularly salient in Indo-European languages, we must attend to the danger in what might be called the Mickey Mouse phenomenon.

### 8.1. Talking Animals Three Feet Tall

For decades in the twentieth century, Hollywood cartoon characters tended to be cast as jolly talking animals about three feet high. But this is hardly an inevitable choice in what to animate, as witnessed by the distinctly lesser role that such talking animals have played in foreign animated cartoons. Indeed, American cartoon characters tended to be human before the late 1920s. But when Walt Disney hit paydirt with a sunny talking mouse, all of the other studios followed suit in a quest for a piece of the monetary pie that Mickey had seized, and a tradition was set: enter Bugs Bunny, Woody Woodpecker, Droopy Dog, etc. It could be argued that American animation has been limited in scope by the imperatives of that tradition.

In the same way, a theory of Universal Grammar that includes sensitivity to inflectional affixation may be one that is more contingent upon local traditions and biases than an empirically independent response to the cross-linguistic data set. The inflectional affixation that naturally appears so central to human language to a European is revealed by diachronic analysis to be less a *sine qua non* of language than a by-product of heavy use – a conspir-

acy of phonetic erosion, semantic bleaching, and syntactic reanalysis. Then this must be seen within the context of the plethora of uninflected natural languages worldwide, the strong tendency for new languages to lack inflections, and the impossibility that inflections (or the option to use them) could confer survival advantage. Hence the suggestion is strong that inflectional affixation is a contingent epiphenomenon in natural language grammars – and as such, a poor candidate for innate specification in any sense.

## 8.2. Universal Grammar Nigeria Style

A thought experiment makes this clearer. Imagine that modern theoretical syntax was founded not by Indo-European speakers in the United States, but by Southern Nigerians speaking Edoid languages like Edo (Bini), Urhobo, and Degema, in a hypothetical world where they were unfamiliar with any Indo-European languages. Under our scenario, let us imagine that the linguists in question were familiar only with Edoid languages and Mande languages spoken further up the west African coast.

In Edoid languages, various tenses and aspects are encoded solely by tone rather than affixes (Elugbe 1989: 299; Ben Elugbe, Jan. 2002 p.c.). Mande languages, on the other hand, are notorious in Niger-Congo for making little use of tone to distinguish lexical items or encode morphosyntactic contrasts, although tone plays a role phonologically and intonationally. As it happens, Edoid languages are SVO, while Mande languages are SOV.

Imagine that our Edoid-speaking linguists, steeped primarily in various Edoid languages beyond the ones they spoke natively, hypothesized that the word order contrast between Edoid and Mande represented a parameter distinction, whose setting by the acquirer depended on tonally-marked tense and aspect marking. Under their analysis, the verb in Edoid languages had to move to the left of the object to “get tone”. For them, Mande’s SOV order was the result of verbs remaining *in situ* because there was no tense-marking tone to “get”. Tone would naturally appear central to human language to them, since it is central to the grammars of the languages these linguists would be most familiar with. Meanwhile, inflectional affixation, absent in Edoid and sparse in Mande, would just as naturally seem “beside the point” in specifying what component of language is innate.

These Edoid speakers’ scenario would seem even more plausible given that in Edoid, tone does not distinguish verbs themselves. Instead, they are only marked with tones that encode features such as tense and aspect. Thus

a theory where verbs moved for a purely grammatical reason – to “get” the tone – would follow quite gracefully given their data set.

Now, let us suppose that these linguists then became aware of the many West African languages that have SVO order but, unlike Edoid, encode tense and aspect segmentally rather than with tones. One response they might have is to suppose that because these languages do make ample use of tone to distinguish lexical items, that acquirers are spurred to move the verb on the basis of a more general “High Functional-Load Tone” feature, with Edoid languages simply displaying a particularly robust overt manifestation of this parameter setting.

We might even imagine that they provided a theory-internal mechanism requiring verb movement in such languages, founded perhaps on a notion of a TONE node that “contains” “abstract tone” even in grammars where tense and aspect are themselves encoded with clitics or affixes. Thus they might propose that a verb in, say, Yoruba can only “acquire” its lexically contrastive tone at “TONE”, this requirement falling away only in languages like the Mande ones where tone’s functional load is so sharply reduced.

For we linguists in the real world, such a theory appears almost laughably misguided. No theories of Universal Grammar treat tone as innately specified. We are aware of the tonogenesis literature demonstrating how tone emerges in grammars that initially lacked it, phonetic erosion leading to a reinterpretation of phonetic tonal distinctions as phonemic, or eliminating a grammatical segment and leaving behind its tone to carry its functional load. As such, we casually think of tone as merely one of many developments that a natural language may drift into over time, and see its functional marginality in most European languages as reinforcing this assumption. Nor, in the evolutionary vein, would any of us see it as likely that tonality assured one more progeny. Similarly, the Indo-European-speaking linguist would never dream of proposing parameters based on alienable possession, switch reference marking, or other features largely absent in European languages that we classify as “frills” peripheral to Universal Grammar.

Things would quite naturally appear otherwise to the Nigerian linguist for whom lexically and morphosyntactically contrastive tone was central to all of the languages they encountered most, with those without it such as the Mande ones thus appearing “exceptional”. My claim is that *diachronic linguistic theory reveals inflectional affixes as every bit as epiphenomenal to natural language as tone*. Like tone, they only arise as a result of the phonetic erosion that heavy usage conditions, and from a cross-linguistic

perspective are just one of many pathways of drift that a grammar may wend its way into.

To wit, for our hypothetical Nigerian linguists, a Universal Grammar comprising parameters hinging on inflectional affixation, rare to absent in the dozens of languages most familiar to them, would appear as parochial as their tonally-based parameters would seem to us.

## 9. So What's the "Story"?

We are faced with the fact that on the one hand, inflectional affixation conditions fascinating typological correlations, while on the other, a conception of Universal Grammar that traces these to innate neurological configurations selected for in human populations is implausible. I have focused my argument on inflection-based parameter scenarios, but Baker is quite aware of this problem in relation to the very plausibility of parameters in general as products of natural selection.

In both Baker (1996) and Baker (2001), the rigorous and ingenious analysis of the synchronic correlations forming the body of his argument contrasts almost jarringly with final envoi chapters addressing the innateness question, where he can only offer highly speculative gestures constituting, essentially, a concessionary shrug of the shoulders. In Baker (2001: 228–230) he proposes that an evolutionary explanation for parameters may only emerge from modes of scientific thought far in the future, inconceivable to us at our current state of knowledge. And in Baker (1996) he even surmises, quite soberly, that the ultimate explanation may be divine inspiration.

Whatever the validity of these approaches, it must be acknowledged that the nature of this conundrum can be taken as suggesting that parameters – and especially ones based on inflectional affixation – are simply not innate.

However, resting here, my argument, whatever its validity, stands merely as what has been called a “nyah nyah” paper, and this is not my intention. Despite the implausibility of the specific innatist implications that Baker and the Rich Agreement Hypothesis school draw from their observations, the fact remains that they have revealed robust correlations that give indication of indeed being causally rather than accidentally linked. As such, I believe that it would be hasty to simply dismiss these arguments as just so much mumbo-jumbo – rather, they beg for an alternate systematic account.



### 9.1. The polysynthesis “parameter”

For example, Baker’s claim that obligatory agreement morphemes corresponding to arguments and noun incorporation represent a unified phenomenon is supported by the fact that the incorporated nouns in such languages are the only arguments that do not require a correspondent agreement morpheme (Baker 1996: 20). Moreover, his formulation of the Polysynthesis Parameter as the Morphological Visibility Condition cited in Section 2 is further supported by otherwise anomalous traits that can be treated as falling out of a general requirement, that arguments either correspond with an inflection or be “swallowed” into the head via incorporation. Absence of adpositional phrases follows from the unavailability of agreement morphemes corresponding to such phrases, absence of infinitives from the fact that they have no marking of subject and would thus violate the requirement that subjects be inflectionally marked at all times, etc.

#### 9.1.1. *Where do you draw the line?*

Thus Baker’s observations strongly suggest that these correlations reflect something real about mental representations of grammatical structure. The question is how plausible this grammatical reality is as an innate inheritance. Here, the problem is that languages Baker classes as polysynthetic are actually a subset of those traditionally treated as such: Baker considers the Polysynthesis Parameter to apply only to those languages that *both* require arguments to correspond with agreement inflections and have robust noun incorporation. Yet as he acknowledges, there also exist many languages with only the agreement requirement with no appreciable noun incorporation, such as Warlpiri, Navajo, and Choctaw, as analyzed by Jelinek (1984), as well as languages where the agreement and/or the incorporation is present but hardly required, such as Chichewa, Slave, and Papuan languages like Yimas (cf. Newmeyer 2005 on this problem with Baker’s definition of polysyntheticity).

That is, languages of Baker’s “pure” polysynthetic typology can be taken as one end of a cline of polysyntheticity as traditionally conceived. That grammars occur along such a cline is a worthy observation in itself: it is significant that there are apparently no languages with noun incorporation without agreement marking, given that there would seem to be no *a priori* reason that such languages would not exist (i.e. why could not a few Polynesian, Sinitic, or Kwa languages not have robust noun incorporation?).

However, this clinal nature also throws yet another wrench into an innatist scenario: Baker's theory cannot explain – *within the tenets of evolutionary biology* -- just why the parameter was “set” at the end of the cline. Just what was it about the combination of obligatory agreement affixes and noun incorporation that would have increased the chances of propagation for those whose genomes rendered them as well predisposed to use this kind of grammar as an analytic one (this presumably being how a parameter, rather than a single option, would manifest itself)? Why would this mutation have only conferred this hypothetical advantage among speakers of languages like Mohawk rather than Warlpiri?

### 9.1.2. *Cognitive versus autonomous*

An alternate analysis beckons: that the cline represents a pathway of drift that languages may possibly take just as Edoid tonal tense/aspect marking does, and that both the implicational, “nested” nature of the cline and the syntactic features that seem to follow from particular degrees of polysyntheticity show that this drift is constrained by cognitive aspects of the human brain.

Other scholars have come to similar conclusions about parameters in general. Hawkins (1994) demonstrates that ordering of verb phrase constituents, proposed as parametrically based by Koopman (1984) and Travis (1989), submits gracefully to a cognitively-based parsing model. In line with my argument, he concurs (1997: 746) that the correlations in question are valid in a descriptive and even predictive sense, “But they are no longer part of UG itself”. Similarly, Newmeyer's (1998: 364) conclusion after comparing formalist and functionalist approaches to parametrical variation is that “one can imagine the possibility of a theory in every respect like recent principles-and-parameters models, but in which the parameters are arrived at inductively by the child”.

Nor does such an approach necessarily entail that language is not innate to some degree. Pinker & Bloom (1994: 183), in a follow-up to their seminal 1990 paper, write that parameters of variation “are not individual explicit gadgets in the human mind... instead, they should fall out of the interaction between the specific mechanisms that define the basic underlying organization of language (‘Universal Grammar’) and the learning mechanisms, some of them predating language, that can be sensitive to surface variation in the entities defined by those language specific mechanisms”.

### 9.1.3. Exaptation and Universal Grammar

Once again, evolutionary theory has space beyond innateness for the emergence of just such a conception of grammar: exaptation, or the recruitment of pre-existing structures in new functions (Gould & Lewontin 1979). Many creatures have fashioned their hands into new structures enabling them to fly, but the extremities themselves were originally selected for the purposes of terrestrial locomotion and manipulation of objects. In the same way, as Lass (1997: 320–323), for example, argues, Finnish case-marking arose through the recruitment of assorted linguistic material which had served other purposes in earlier stages of its grammar.

The correlations that Baker identifies, then, can be seen as support for the exaptationist conception of linguistic evolution espoused by authors such as Deacon (1997) and Kirby (1999). A preliminary observation regarding polysynthesis is that it, like all language change, demonstrates a cognitive tendency towards entrenchment of habit which natural languages manifest over time. Pointedly, there are myriad ways in which this happens in grammars that no linguist would treat as based on innate configurations.

Lass (1997: 318f.) notes, for example, that over time English came to require speakers to mark the present tense with the progressive marker rather than the bare verb: what begins as a marginal strategy in Old English becomes obligatory in Modern English. Yet the anomalousness of this construction amidst Germanic – as well as languages worldwide – makes it plain that there is no “progressively-marked present tense parameter”.

Our question, then, is simply whether there is any more indication that polysyntheticity and its syntactic correlates require an innatist parametrical analysis. Along those lines, it would seem that traditional functionalist studies of grammaticalization processes serve well as an alternate account.

For example, languages in which resumptive pronouns, or the co-indexing of subject- or object-marking clitics or affixes with full NPs, are ungrammatical are unknown to this author: *My mother, she eats a lot of vegetables; I never liked him, that guy*; the French election slogan *La France, nous, on l'aime*. This trait, rooted in the topic-comment configurational inclination typical of human grammars that drives so much of grammatical change (cf. Li 1976), is typical of oral language in particular (cf. Escure 1997). In any grammar, myriad features that are either optional or marked in terms of pragmatics or register are candidates for entrenchment as obligatory. Grammars differ in what choices they happen to make in this regard: some develop obligatory evidential marking, others develop verb-initial word order, etc.

Baker's conception of polysynthesis could plausibly trace to grammars drifting into the entrenchment of the co-indexing of full NPs with pronominal elements. In languages with obligatory subject prefixes, residual phonetic likenesses between the prefixes and the full subject pronouns testify to this pathway of development (e.g. Swahili's second-person singular pronoun *wewe* and the corresponding prefix *u-*, and third-person plural *wao* and *wa-*). In some languages, this particular structural "genius" would become so very entrenched that constituents incommensurate with it would become infelicitous – just the history of a verb-initial language includes a point at which verb-initial sentences, at first a pragmatic option amidst a subject-initial norm, became a new norm rendering subject-initial sentences ungrammatical. Hence, in a Bakerian polysynthetic language, *exeunt* adpositional phrases for which there exist no corresponding resumptive pronouns, and so on.

Germanic diachrony demonstrates that such grammatical correlations falling out from a particular feature need not be seen as suggestions of innateness. There are only two Germanic languages, English and Swedish, that recruited their *be*-verbs as passive markers (Rissanen 1999: 215). As it happens, these two languages are also unique in their family in not marking intransitive perfects with the *be*-verb (German *wir **sind** gereist* "We travelled", but Swedish *vi **har** rest* with a form of its verb *have*). This is not an accidental correlation: few would contest an analysis here based on an ambiguity between the passive and perfect readings rendered by the same verb, resolved in favor of the former. That is, the habit of marking the passive with *be*, beginning through chance drift, eventually became so entrenched in these two grammars that it rendered *be*-perfects potentially opaque, with these thus eliminated. This was essentially an unremarkable cognitive process, and none would assume that it indicated a neurologically encoded parametrical choice for maintaining distinct forms for the intransitive perfect and the passive. Any such argument would founder upon Icelandic, in which the *be*-verb is used both to connote resultativity in the intransitive perfect with verbs of motion and change-of-state (*ég **er** kominn* "I am come", "I am here" [Kress 1982: 152f.]) as well as to connote a substantial resultative domain of the passive (*ég **var** barinn* "I was hit") (ibid: 148f.).

While the entrenchment of NP-pronominal co-indexing is readily intuitive, the analogous origin of the other defining feature of Baker's polysynthesis conception is perhaps less so to Indo-European speakers: the entrenchment of a "habit" of saying *He's house-building* for *He's building a house*. But we can see parallels in English to the beginning of such a process in items like *babysit*, and obviously the further entrenchment of such a

feature is inherently no more remarkable than so many Indo-European languages' developing a verb *have* denoting possession – which has been argued to be a typologically unusual feature, encoded in most languages with case-marking or zero-marking (Lazard 1990: 246f.). Yet we would not accept a proposed “*have*-verb parameter”.

Here, then, my analysis concurs with that of Newmeyer (2005: 73–127), who presents a rigorous and extensive argument that grammatical behaviors often attributed to parameters are more elegantly attributed to aspects of performance; i.e. entrenchment constrained by factors such as economy and iconicity (as well as others). Crucially, there would appear to be no principled motivation for specifying that polysyntheticity of any type be encoded as innate that would at the same time systematically rule out submitting English's progressive-marked present or *be*-marked passive to the same analysis. Moreover, that we are dealing with various stages of an acquired habit rather than a distinct “parameter setting” is supported by the very fact that natural languages evidence polysyntheticity along a cline of varying degrees – along which there is no particular point that we have any *principled* reason to suppose would decisively enhance a species' propagation of its genes *where previous points on the cline would not*.

Such an account does leave open the question as to why languages apparently only drift into noun incorporation *after* having acquired the agreement morpheme “tic”. But the fact remains that this developmental sequence does not submit any more gracefully to analysis as a selected feature than the parameter itself. And all indications are that a cognitively-based address of this question is more immediately promising than one based on religion, or modes of thought too fundamentally unlike any presently known to even be productively addressed within our lifetimes.

## 9.2. The rich agreement hypothesis

There are two possible alternate approaches to the Rich Agreement Hypothesis.

### 9.2.1. Another epiphenomenon of human cognition?

One approach would be to proceed according to the assumption, as seems unassailable regarding the Polysynthesis Parameter, that the relevant typological correlations are real. Supporting this would be the restriction of

the Transitive Expletive Construction to languages with verb movement. Bobaljik & Thráinnson (1998: 56) argue that these constructions, where the logical subject occupies a lower position while the expletive fills a higher one, suggest a Split IP, an example being Icelandic:

- (9) *það hefur einhver köttur étið mýsna.*  
EXPL has some cat eaten mice.the  
'A cat has eaten mice.' (i.e. 'There has a cat eaten mice.')

They predict that such constructions would only occur in languages with verb movement, and demonstrate this with Norwegian and English where the Transitive Expletive Construction is absent:

- (10) \**Det har en katt ete mysene.*  
EXPL has a cat eaten mice.the  
'A cat has eaten mice.'

Faroese lends encouraging support, in that speakers of dialects with verb movement accept Transitive Expletive Constructions while speakers of dialects where the verb does not move do not (Jonas 1996: 106).

Data of this sort show that it would be inappropriate to reject the Rich Agreement Hypothesis as based solely on verb movement and various formulations of inflectional "richness". Arguments from adherents of this school are broader and more rigorous than this.

We might propose, then, that the emergence of inflectional affixation conditions a syntactic mental representation with a split IP without there being any innate specification or predisposition for this. Future research may reveal that even when affixes erode, such a syntax can persist in the mental representations of future generations on the basis of other "cues" such as manifestations of multiple specifier positions high in the tree, et al. It is unclear that the current state of neurobiology or human cognition rules out such a hypothesis in any falsifiable way.

### 9.2.2. *An artifact of a narrow data set?*

Yet the fact remains that the Rich Agreement Hypothesis has to date only been argued on the basis of Germanic (one smallish subfamily only about a dozen languages strong) and Romance (a smallish *sub*-subfamily only about a dozen languages strong by conventional estimations), which are two con-

glomerations within just nine living subfamilies of just one smallish family (Indo-European is by no means fecund as language families go) out of dozens of language families worldwide. While strictly speaking, a small subset of languages may well reveal language universals, surely this typologically narrow database qualifies as a potential weakness compared to the truly cross-linguistic focus of Baker's work.

This becomes especially urgent in view of another parameter argued for on the basis of a similarly narrow typological base, perhaps the "textbook case" for the parameter conception as a whole, the null-subject parameter. Rizzi (1982) and Safir (1985) together predicted five out of sixteen potential conglomerations of features in grammars based on their conception of this parameter and its predicted effects upon a syntactic configuration. Yet Gilligan (1987), checking a 100-language sample, found no fewer than nine such conglomerations – that is, four configurations that Rizzi's and Safir's hypothesis ruled out.

Recently adduced evidence suggests that the Rich Agreement Hypothesis may be similarly threatened when the typological net is cast more widely, and that a second alternate analysis to the Rich Agreement Hypothesis may be to reject its cross-linguistic validity altogether.

Cape Verdean Creole Portuguese has but one inflectional affix, the past marker *-ba*. None of the metrics that Rich Agreement Hypothesis adherents have proposed would admit this one marker, invariant across person and number, as lending this grammar "rich" inflection. Yet Baptista (2000) shows that nevertheless in this language, the verb moves past adverbs and quantifiers left-adjoined to VP, *even when the past affix is not even phonologically realized*:

- (11) a. *João xina    **ben** se lison.*  
           John learned well his lesson.  
           'John learned his lesson well.' (Baptista 2000: 14)
- b. *Konbidadu txiga    **tudu** na mismu tempu.*  
           guests        arrived all    in same time  
           'All the guests arrived at the same time.' (ibid. 17)

Yet Baptista argues that Cape Verdean syntactic behavior gives no evidence of a Split IP such as multiple specifier heads available to subjects.

The facts are even more problematic in Cape Verdean's close sister language, Guinea-Bissau Creole Portuguese. Here, the cognate of *-ba* is less an affix than a floating clitic, modifying not only verb phrases but even nominal predicates:

- (12) *I un prosesu difícil ba.*

it a process difficult PAST  
‘It was a difficult process.’

(Kihm 1994: 108,  
cited in Baptista 2000: 26)

But in this inflection-free language, the verb again moves leftward of floating adverbial quantifiers left-adjoined to VP:

- (13) *Konbidadu tudu ciga na mismu tenpu.*

guests all arrived at same time  
‘The guests arrived all at the same time.’

(ibid.)

Yet again, there are no residual indications of split IP in Guinea-Bissau Creole Portuguese either (unattested in the most complete grammar of the language [Kihm 1994] and confirmed as absent by Kihm himself [Mar. 2002 p.c.]).

Finally, there are dialects of Louisiana Creole French where verb stems alternate between short and long forms (Baptista 2000:28). The short forms condition verb movement obligatorily with the negator *pa* and optionally with VP adverbs:

- (14) a. *Mo mōzh pa gratō.*

I eat NEG cracklin’  
‘I don’t eat cracklin’.

(Neumann 1985: 321,  
cited by Rottet 1992: 277)

- b. *Mo marsh (pa) zhame ni-pje deor.*

I walk NEG never barefoot outside  
‘I never walk barefoot outside.’

(Neumann 1985: 330,  
cited by Rottet 1992: 267)

But the long forms remain in place under both conditions:

- (15) a. *Na lōtō mo pa mōzhe gratō.*

in long-time I NEG eat cracklin’  
‘I haven’t eaten cracklin’ for a long time.’

(Neumann 1985: 321, cited by Rottet 1992: 277)

- b. *Mo (pa) zhame (te) zhōgle ōho sa.*

I NEG never PAST think about that  
‘I never thought about that.’

(Neumann 1985: 330, cited by Rottet 1992: 267)

The variation here is difficult to square with an analysis positing a Split IP categorically requiring verb movement – some finite verbs move and others



do not. And it is furthermore ominous that, to the extent that one might be moved to treat the final *e* on the long forms as an “inflection” of some kind, it is precisely the verbs with this segmental appendage that *never* move.

These data from three natural languages beyond the data set that the Rich Agreement Hypothesis school have addressed suggest that it may be found that the correlations in question are restricted to a small subset of the world’s languages, and that the explanation found for them will fall outside of innateness.

## 10. Conclusion

Because inflectional affixation emerges as the result of contingent accident, is absent or marginal in languages that arise anew, and is impossible to square with the tenets of evolutionary biology as currently configured, I submit that the syntactic parameter whose setting depends on inflectional affixation is a logical impossibility.

This implies that where typological correlations hingeing on affixation are evident, that they be traced to factors beyond natural selection. Furthermore, arguments such as Newmeyer (2005) strongly suggest that a similar verdict applies to the conception of UG parameters as a whole: Newmeyer elegantly demonstrates that 1) cross-linguistic analysis fails to support the idea that grammars are determined by parametrical alternations and/or their presumed effects upon assorted features of grammars and 2) performance factors scientifically explain phenomena often attributed to parametrical alternations. In other words, this argument supports positions such as Deacon’s (1997) situating the language competence in general aspects of human cognition.

In light of Baker’s claim that parameters’ evolutionary purpose will only become evident from forms of thought as yet unfamiliar, I surmise that the most promising infant paradigm for providing a principled explanation of the correlations in question will be that charting the self-organizing principles of complex adaptive systems (Kauffman 1995) (cf. Kirby 1999).

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# Why don't apes point?<sup>1</sup>

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Chimpanzees gesture to one another regularly. While some of their gestures are relatively inflexible displays invariably elicited by particular environmental events, an important subset are learned by individuals and used flexibly – such things as ‘arm-raise’ to elicit play or ‘touch-side’ to request nursing. We know that such gestures are learned because in many cases only some individuals use them, and indeed several observers have noted the existence of idiosyncratic gestures used by only single individuals (Goodall 1986). And their flexible use has been repeatedly documented in the sense of a single gesture being used for multiple communicative ends, and the same communicative end being served by multiple gestures (Tomasello et al. 1985, 1989). Flexible use is also evident in the fact that apes only use their visually based gestures such as ‘arm-raise’ when the recipient is already visually oriented toward them – so-called audience effects (Tomasello et al. 1994, 1997; Kaminski et al. 2004).

Chimpanzees and other great apes also know quite a bit about what other individuals do and do not see. They follow the gaze direction of conspecifics to relatively distal locations (Tomasello et al. 1998), and they even follow another's gaze direction around and behind barriers to locate specific targets (Tomasello et al. 1999). This gaze following is not an inflexible response to a stimulus, as from a certain age chimpanzees look where another individual is looking and, if they find nothing interesting on that line of sight, check back a second time and try again (Call et al. 2000). Indeed if a chimpanzee follows another's line of sight and repeatedly finds nothing there, they will quit following that individual's gaze altogether (Tomasello et al. 2001). And some experiments have even demonstrated that chimpanzees know the content of what another sees, as individuals act differently if a competitor does or does not see a potentially contestable food item (Hare et al. 2000, 2001).

And so the question arises: If chimpanzees have the ability to gesture flexibly and they also know something about what others do and do not see –

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and there are certainly occasions in their lives when making someone see something would be useful – why do they not sometimes attempt to direct another's attention to something it does not see by means of a pointing gesture or something equivalent? Some might object that they do do this on occasion in some experimental settings, but this in fact only deepens the mystery. The observation is that captive chimpanzees will often “point” (whole arm with open hand to food so that humans will give it to them (Leavens & Hopkins 1998) or also, in the case of human-raised apes, to currently inaccessible locations they want access to (Savage-Rumbaugh 1990). This means that apes can, in unnatural circumstances with members of the human species, learn to do something in some ways equivalent to pointing (in one of its functions). And yet there is not a single reliable observation, by any scientist anywhere, of one ape pointing for another.

But maybe we should look at this question from the other direction, that is, from the direction of humans. The fact is that chimpanzees and the other great apes are doing the typical thing, by not pointing; it is human beings who are doing this strange thing called pointing. What are humans doing when they do this, and why are they doing it? As an advocate of the comparative method to psychological research, I believe that these two questions – why apes don't point and why humans do – are best answered together. I will attempt that here, using for comparison human infants (to avoid the dizzying complexities of language) and our nearest primate relatives, the great apes, especially chimpanzees (for whom there is the greatest amount of empirical work).

### **The comprehension of pointing**

In an experiment with apes and human children, Tomasello, Call & Gluckman (1997) had one person, called the “hider”, hide food or a toy from the subject in one of three distinctive containers. Later, a second person, called the “helper”, showed the subject where it was by tilting the appropriate container toward them, so that they could see the prize, just before their attempt to find it. After this warm-up period in which he defined his role, the helper began helping not by showing the food or toy but by giving signs, one of which was pointing (with gaze alternation between subject and bucket as an additional cue to his intentions). The apes as a group were very poor (at chance) in comprehending the meaning of the pointing gesture, even though they were attentive and motivated on virtually every trial. (Itakura, Agnetta,

Hare & Tomasello 1999, used a trained chimpanzee conspecific to give a similar cue, but still found negative results.) Human two-year-old children, in contrast, performed very skillfully in this so-called object choice task. Subsequent studies have shown that apes are also generally unable to use other kinds of communicative cues (see Call & Tomasello 2005, for a review), and that even prelinguistic human infants of 14 months of age can comprehend the meaning of the pointing gesture in this situation (Behne et al., forthcoming).

It is important to recall that apes are very good at following gaze direction in general (including of humans), and so their struggles in the object choice task do not emanate from an inability to follow the directionality of the pointing/gazing cue. Rather, it seems that they do not understand the meaning of this cue – they do not understand either that the human is directing their attention in this direction intentionally or why she is doing so. As evidence for this interpretation, Hare & Tomasello (2004) compared this pointing gesture to a similar but different cue. Specifically, in one condition they had the experimenter first establish a competitive relationship with the ape, and then subsequently reach unsuccessfully in the direction of the baited bucket (because the hole through which he reached would not enable her arm to go far enough). In this situation, with an extended arm that resembled in many ways a pointing gesture (but with thwarted effort and without gaze alternation), apes suddenly became successful. One interpretation is that in this situation apes understood the human's simple goal or intention to get into the bucket, and from this inferred the presence of food there (and other research has shown their strong skills for making inferences of this type; Call, in press).

But understanding goals or intentions is not the same thing as understanding communicative intentions. Nor is following gaze the same thing as understanding communicative intentions. In simple behavior reading or gaze following, the individual just gathers information from another individual in whatever way it can – by observing behavior and other happenings in the immediate surroundings and making inferences from them. The object choice task, however, is a communicative situation in which the subject must understand the experimenter's communicative intentions, that is, she must understand that the looking and/or pointing behavior of the human is done "for me" and so is relevant in some way for the foraging task I am facing. Said another way, to use the cue effectively, the subject should understand that the experimenter intends for her gaze and/or point to be taken as informative. Instead, chimpanzees seem to see the task as simply another

case of problem-solving in which all things in the context should be taken as potential sources of information – with the gaze direction or pointing of the interactant as just another information source. Human infants, on the other hand, understand in this situation that the adult has made this gesture for them, in an attempt to direct their attention to one of the buckets, and so this gesture should be relevant for their current goal to find the toy (see Sperber & Wilson 1986, on relevance). That is to say, they understand the adult's communicative intention – her intention to inform me of something – which is an intention toward my intentional states (an embedded intention).

An important aspect of this process is the joint attentional frame, or common communicative ground, which gives the pointing gesture its meaning in specific contexts (Clark 1996; Enfield 2006). Thus, if you encounter me on the street and I simply point to the side of a building, the appropriate response would be “Huh?” But if we both know together that you are searching for your new dentist's office, then the point is immediately meaningful. In the object choice task, human infants seem to establish with the experimenter a joint attentional frame – perhaps mutual knowledge – that “what we are doing” is playing a game in which I search for the toy (and you help me) – so the point is now taken as informing me where the toy is located. The infant thus asks herself, so to speak, why is the adult directing my attention to that bucket, why is it relevant to this game?

It is very likely that apes do not create with one another such joint attentional frames, or common communicative ground, with either conspecifics or humans. Tomasello et al. (in press) argue and present evidence that, more generally, apes do not form with others joint intentions to do things collaboratively (an analysis which also applies to their so-called cooperative hunting; see footnote 1 below), and without some kind of joint goals or intentions there are few opportunities for joint attention. In a direct cross-species comparison, Warneken, Chen & Tomasello (forthcoming) found that human one- and two-year-olds already engage with others collaboratively in various ways (even encouraging the other in his role when he is recalcitrant), whereas young chimpanzees engage with others in a much less collaborative fashion (with no encouraging of the other to play her role; see Povinelli & O'Neill 2000, for a similar finding). And Tomasello & Carpenter (forthcoming), in another direct comparison and using identical operational criteria, found basically no joint attentional engagement in young chimpanzees interacting with humans. It is also relevant that from their earliest attempts at communication, human infants engage in a kind of conversation or “negotiation of meaning” in which they adjust their communicative attempts

in the light of the listener's signs of comprehension or noncomprehension (Golinkoff 1993) – a style of communication that is essentially collaborative, and that other primate species do not, as far as we know, employ (there are no observations of one ape asking another for clarification or repairing a communicative formulation in anticipation of its being misunderstood).

And so my answer to the question of why apes do not seem to comprehend the pointing gesture is that: (1) they do not understand the embedded structure of informing or communicative intentions (she intends to change my intentional states, i.e., by informing me of something); and (2) they do not participate with others in the kinds of collaborative joint attentional engagements that create the common communicative ground necessary for pointing and other deictic gestures to be meaningful in particular contexts.

### **The production of pointing**

Classically, human infants are thought to point for two main reasons: (i) they point imperatively when they want the adult to do something for them (e.g., give them something, “Juice!”); and (ii) they point declaratively when they want the adult to share attention with them to some interesting event or object (“Look!”) (Bates, Caimioni & Volterra 1975). Although some apes, especially those with extensive human contact, sometimes point imperatively for humans (see above), no apes point declaratively ever. Indeed, when Tomasello & Carpenter (forthcoming) repeatedly used procedures that reliably illicit declarative pointing from young human infants, they were unable to induce any declarative pointing from any of three young chimpanzees. Typically developing human infants, on the other hand, spontaneously begin pointing declaratively at around the first birthday – the same age at which they first point imperatively. The difference between these two types of pointing is clearly not motoric or cognitive in any simple and straightforward sense. The main difference is motivational (with perhaps a cognitive dimension to this in the sense that infants may be motivated to do things that apes cannot even conceive). So why do human infants simply point to things when they do not want to obtain them?

In a recent study, Liszkowski et al. (2004, 2006) addressed this question by having an adult react to the declarative points of 12-month-olds systematically in one of four different ways – and then observing their reaction. In one condition, the adult reacted consistent with the idea “she wants me to look at the object” by simply looking at the object. In a second condition

the adult reacted consistent with the idea “she wants me to get excited” by simply emoting positively toward the child. In a third control condition the adult showed no reaction. In all three of these cases infants reacted in ways that showed they were not satisfied with the adult’s response – this was not their goal – by doing such things as pointing again. In contrast, in a fourth condition the adult responded by looking back and forth from the object to the infant and commenting positively. Infants were satisfied with this response – they pointed one long time – implying that this response was indeed what they wanted. One interpretation of this adult response is that it represents a sharing of interest and attention to some external entity, and this by itself is rewarding for infants – apparently in a way it is not for any other species on the planet. This interpretation is supported by the fact that infants at this age also regularly hold up objects to show them to others, seeming to want nothing from the adult but a sharing of experience (and emotion), and again apes simply never hold things up to show them to others (Tomasello & Caimioni 1997).

An important clarification. In the case of imperative pointing, which some apes sometimes do for humans, it is important to recognize that an individual may point imperatively in different ways, with different kinds of underlying understanding. One might point imperatively simply as a procedure for making things happen, based on past experience in which this behavior induced others to do such things as fetch objects. But it is also possible that one might point imperatively in full knowledge that what is happening is that one is making one’s desire manifest, and the other person understands this and chooses, deliberately, to help obtain it. Thus, Schwe & Markman (1997) had an adult respond to the requests of two-year-olds by, among other things, refusing them or misunderstanding them. When the child’s request was refused she was not happy and displayed this in various ways. But when her request was misunderstood – even in cases where the adult actually gave her what she wanted unintentionally (“You want this (wrong object)? You can’t have it but you can have this one (right object) instead.”) – the child was not fully satisfied and often repeated her request. Under this interpretation, infants from a certain age are pointing imperatively not as a blind procedure for making things happen, but as a request that the adult know her goal and decide to help her attain it. We cannot be certain, but it may be that apes with humans are doing one kind of imperative pointing and human infants are doing another.

In addition to these two main motives for infants’ pointing, Liszkowski et al. (in press, 2006) identified a third major motive. An adult engaged in

one of several activities in front of the child. This was an adult activity, such as stapling papers, and the adult did not attempt to engage the child in it in any way. The adult was then distracted for a moment, during which time the key object, for example, the stapler, was displaced (in one of several ways). The adult then returned, picked up her papers, and looked around searchingly (palms up, quizzical expression – no language). Preverbal infants as young as 12 months of age quite often pointed to the stapler for the adult (and not to a distractor object which had been displaced at the same time). In our interpretation, the infant in this situation is simply informing the adult of something she does not know, that is to say, helping her by providing her with information she does not have. This interpretation is not far-fetched, as a similar helping motive is also evident in 18-month-old infants' behavior in non-communicative situations, when they do such things as help adults reach out-of-reach objects, open doors for them when their hands are full, and so forth – whereas in this same paradigm human-raised apes showed few signs of such helping (Warneken & Tomasello, forthcoming).

As hinted at above, these motives may imply some unique understanding of others. For example, the declarative motive assumes a partner with the psychological states of interest and attention, which one can then attempt to share. But perhaps most strikingly, the informative motive implies an understanding of the distinction between knowledge and ignorance in the partner. I inform you of things because, presumably, I think that you do not know them and you would like to have the information. It is widely believed that young infants do not have an understanding of knowledge vs. ignorance, but recent research has demonstrated that they do. Tomasello & Haberl (2003) had an adult say to 12- and 18-month-old infants "Oh, wow! That's so cool! Can you give it to me?" while gesturing ambiguously in the direction of three objects. Two of these objects were 'old' for the adult – he and the child had played together with them previously – and one was 'new' to him (though not to the child, who had played with it also previously). Infants gave the adult the object that was new for him. Infants knew which objects the adult had experienced, and which he had not.

In a recent similar study, Moll et al. (forthcoming) found that when an adult looked at an object she and the child had just finished playing with together and said excitedly "Oh, wow! That's so cool!", 14- and 18-month-old infants assumed she was not talking about the object – they knew she could not be excited about the object which they had just played with together – and so they looked for some other target of her excitement. When the object was new to the adult – they had not previously played with it to-

gether – infants simply assumed that the adult was excited about the object. There is no systematic research on apes' skills of determining what is new or old for another person. But when the Moll et al. paradigm was used with three young chimpanzees, they did not differentiate between the cases in which the object was old and new for the human (Tomasello & Carpenter, forthcoming). It is also relevant that in a systematic review of ape vocal and gestural communication, Tomasello (2003a) considers their ability to adjust for different audiences and notes that the audience effects that exist are based on whether others are present or not in the immediate context, or whether they are oriented towards them bodily. There is no evidence that primates take account of others' intentional or mental states in order to adjust their communicative formulations.

In general, in the current analysis, the underlying motives for infants' pointing, and responding to adult points, may be decomposed into two basic underlying motives: helping and sharing. With imperative points they are requesting help, and when they respond to these from adults they are helping. With declarative points, and in responding to these, they are sharing. With informative points they are helping others by sharing information (and as they learn language they begin to ask questions as a way of requesting that others share information with them). Apparently, other ape species do not have these same motivations to help and share with others. And so my answer to the question of why chimpanzees and other apes do not produce points – for sure not declarative and informative points, no matter how they are brought up – is that: (1) they do not have the motives to share experience with others or to help them by informing; and (2) they do not really know what is informationally new for others, and so what is worthy of their communicative efforts.

### **Learning to point**

No one knows how human infants come to point for others. But given cross-cultural differences in infants' gestural behavior (though these have not been documented as specifically as one might like), it would seem clear that the major process is one of learning. There are two main candidates.

First is some form of ritualization. For example, a very young infant might reach for a distant object, at which point her mother might discern the intention and obtain the object for her – leading to a ritualized form of reaching that resembles pointing (Vygotsky 1978). We can also extend this

hypothetical scenario to the case that, by most accounts, seems more likely, when infants use arm and index finger extension to orient their own attention to things. If an adult were to respond to this by attending to the same thing and then share excitement with the infant by smiling and talking to her, then this kind of pointing might also become ritualized – that is, a learned procedure for producing a desired social effect. In this scenario it would be possible for an infant to point for others while still not understanding the pointing gesture of others, and indeed a number of empirical studies find just such dissociations in many young infants (Franco & Butterworth 1996). Infants who learn to point via ritualization, therefore, may understand their gesture from the “inside” only, as a procedure for getting something done, not as an invitation to share attention using a mutually understood communicative convention.

The alternative is that the infant observes an adult point for her and comprehends that the adult is attempting to induce her to share attention to something, and then imitatively learns that when she has the same goal she can use the same means, thus creating an intersubjective symbolic act for sharing attention. It is crucial that in this learning process – one form of what Tomasello, Kruger & Ratner (1993) called cultural learning – the infant is not just mimicking adults sticking out their fingers; she is truly understanding and attempting to reproduce the adult's intentionally communicative act, including both means and end. It is crucial because a bi-directional symbol can only be created when the child first understands the intentions behind the adult's communicative act, and then identifies with those intentions herself as she produces the “same” means for the “same” end.

Empirically we do not know whether infants learn to point via ritualization or imitative learning or whether, as I suspect, some infants learn in one way (especially prior to their first birthdays) and some learn in the other. And it may even happen that an infant who learns to point via ritualization at some later point comes to comprehend adult pointing in a new way, and so comes to a new understanding of her own pointing and its equivalence to the adult version. Thus, Franco & Butterworth (1996) found that when many infants first begin to point they do not seem to monitor the adult's reaction at all. Some months later they look to the adult after they have pointed to observe her reaction, and some months after that they look to the adult first, to secure her attention on themselves, before they engage in the pointing act – perhaps evidencing a new understanding of the adult's comprehension.

Virtually all of chimpanzees' flexibly produced gestures are intention movements that have been ritualized in interaction with others. For example,



an infant chimpanzee who wants to climb on its mother's back may first actually pull down physically on her rear end to make the back accessible, after which the mother learns to anticipate on first touch, which the infant then notices and exploits in the future. The general form of this type of learning is thus:

1. Individual A performs behavior X (non-communicative).
2. Individual B reacts consistently with behavior Y.
3. Subsequently B anticipates A's performance of X, on the basis of its initial step, by performing Y.
4. Subsequently, A anticipates B's anticipation and produces the initial step in a ritualized form (waiting for a response) *in order to* elicit Y.

The main point is that a behavior that was not at first a communicative signal becomes one by virtue of the anticipations of the interactants over time. There is very good evidence from a series of longitudinal and experimental studies that chimpanzees do not learn their gestures by imitating one another but rather by ritualizing them with one another in this way (see Tomasello & Call 1997, for a review). This means that chimpanzees use and understand their gestures as one-way procedures for getting things done, not as inter-subjectively shared, bi-directional coordination devices or symbols. At least some support for this hypothesis is also provided by the fact that young chimpanzees, unlike human infants, do not spontaneously reverse roles when someone acts on them and invites a reciprocal action in return; that is, they do not engage in role reversal imitation of instrumental acts (Tomasello & Carpenter, forthcoming).

In general, two decades of experimental research have demonstrated conclusively that, among primates, human beings are by far the most skilled and motivated imitators (see Tomasello 1996, for a review). More controversially, I would claim that some types of imitative learning are uniquely human, specifically those that require the learner to understand the intentions of the actor, that is, not only the actor's goal but also his plan of action or means of execution for reaching that goal. When the intentions are actually communicative intentions – involving the embedding of one intention within another or the reversing of roles within a communicative act – apes are simply, in my view, not capable of either understanding or reproducing these. This means that their communicative devices are not in any sense shared in the manner of human communicative conventions such as pointing and language.

## **Shared intentionality**

So why don't apes point? I have given here more or less five fundamental reasons:

- they do not understand communicative intentions
- they do not participate in joint attentional engagement as common communicative ground within which deictic gestures are meaningful
- they do not have the motives to help and to share
- they are not motivated to inform others of things because they cannot determine what is old and new information for them (i.e., they do not really understand informing, *per se*)
- they cannot imitatively learn communicative conventions as inherently bi-directional coordination devices with reversible roles

And so the obvious question is: is this really five different reasons, or are these all part of one or a few more fundamental reason(s)?

My proposal here is that all of these reasons are basically reflections of the more fundamental fact that only humans engage with one another in acts of what some philosophers of action call shared intentionality, or sometimes “we” intentionality, in which participants have a shared goal and co-ordinated action roles for pursuing that shared goal (Gilbert 1989; Bratman 1992; Tuomela 1995; Searle 1995; Clark 1996). The activity itself may be complex (e.g., building a building, playing a symphony) or simple (e.g., taking a walk together, engaging in conversation), so long as the interactants are engaged with one another in a particular way. In all cases the goals and intentions of each interactant must include as content something of the goals and intentions of the other. When individuals in complex social groups share intentions with one another repeatedly in particular interactive contexts, the result is habitual social practices and beliefs that sometimes create what Searle (1995) calls social or institutional facts: such things as marriage, money, and government, which only exist due to the shared practices and beliefs of a group.

In my previous approach to these problems (e.g., Tomasello 1999), I hypothesized that only human beings understand one another as intentional agents – with goals and perceptions of their own – and this is what accounts for many uniquely human social cognitive skills, including those of cultural learning and conventional communication, that would seem to involve one or another form of shared intentionality. We now have data, how-

ever, that has convinced me that at least some great apes do understand that others have goals and perceptions (not, by the way, thought and beliefs), as summarized by Tomasello, Call & Hare (2003). The details of these data do not concern us here, but the immediate theoretical problem is how we should account for uniquely human cultural cognition, as we sometimes call it, if not by humans' exclusive ability to understand others intentionally.

Tomasello et al. (in press) present a new proposal that identifies the uniquely human social cognitive skills not as involving the understanding of intentionality *simpliciter*, but as involving the ability to create with others in collaborative interactions joint intentions and joint attention (which in the old theory basically came for free once one understood others as intentional agents). These basic skills of shared intentionality involve both a new motivation for sharing psychological states, such as goals and experiences, with conspecifics, and perhaps new forms of cognitive representation (what we call dialogic cognitive representations) for doing so as well. Evolutionarily (see also Boyd et al. 2006), the proposal is that individual humans who were especially skilled at collaborative interactions with others were adaptively favored, and the requisite social-cognitive skills that they possessed were such that, at some point, the collaborative interactions in which they engaged became qualitatively new – they became collaborative interactions in which individuals were able to form a shared goal to which they jointly committed themselves. Following Bratman (1992), such shared intentional activities, as he calls them, also involve understanding others' plans for pursuing those joint goals (meshing sub-plans), and even helping the other in his role if this is needed. There is basically no evidence from any non-human animal species of collaborative interactions in which different individuals play different roles that are planned and coordinated, with assistance from the other as needed.<sup>2</sup>

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<sup>2</sup> The most complex cooperative activity of chimpanzees is group hunting, in which two or more males seem to play different roles in corralling a monkey (Boesch & Boesch 1989). But in analyses of the sequential unfolding of participant behavior over time in these hunts, many observers have characterized this activity as essentially identical to the group hunting of other social mammals such as lions and wolves (Cheney & Seyfarth 1990; Tomasello & Call 1997). Although it is a complex social activity, as it develops over time each individual simply assesses the state of the chase at each moment and decides what is best for it to do. There is nothing that would be called collaboration in the narrow sense of joint intentions and attention based on coordinated plans. In experimental studies (e.g., Crawford 1937; Chalmeau 1994), the most complex behavior observed is some-

Tomasello et al. (in press) take a very close look at human infants from this point of view and find that whereas infants of nine months of age can coordinate with adults in some interesting ways that might reflect an initial ability to form joint goals – such things as rolling a ball back and forth or putting away toys together – it is at around 12 to 14 months of age that full-fledged shared intentionality seems to emerge. It is at this age that infants for the first time seem truly motivated to share experience with others through declarative and informative pointing, that they encourage others to play their role when a collaborative interaction breaks down, that they can reverse roles in collaborative interactions, and that they start to acquire linguistic conventions.

So the specific proposal here – with regard to the question of why human infants point but other apes do not – is that only humans have the skills and motivations to engage with others collaboratively, to form with others joint intentions and joint attention in acts of shared intentionality. The constitutive motivations are mainly helping and sharing, which obviously (and as argued above) are an important part of indicating acts such as pointing. Understanding and coordinating with others' plans toward goals is in general a necessary part of human communication, understood as joint action (Clark 1996). Reversing roles is a very important part in these collaborative interactions, and it is likely that the understanding of perspectives is simply the perceptual/attentional side of such role reversal (Baressi & Moore 1996). And so, although we certainly do not have at the moment all details worked out, it would seem a plausible suggestion that uniquely human forms of communication – including both nonlinguistic and linguistic conventions – rest fundamentally on a foundation of uniquely human forms of collaborative engagement involving shared intentionality.

### **And how about language?**

I would like to conclude my discussion of pointing by making the argument – in a very cursory fashion – that many of the aspects of language that make it such a uniquely powerful form of human cognition and communication

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thing like two chimpanzees pulling a heavy object in parallel, and during this activity almost no communication among partners is observed (Povinelli & O'Neill 2000). There are no published experimental studies – and several unpublished negative results (two of them ours) – in which chimpanzees collaborate by playing different and complementary roles in an activity.

are already present in the humble act of pointing. And so in searching for the phylogenetic roots of human linguistic competence, we might profitably begin with the pointing gesture, which is at least a bit less complicated.

First of all, as stressed by Clark (1996) and as argued above, both pointing and language are collaborative communicative acts. In both cases, recipients either signal comprehension or non-comprehension, and communicators adjust accordingly, sometimes repairing their communicative acts to help the other understand. This collaborative communicative structure derives from a human adaptation for collaborative activity more generally, involving the ability and inclination to form with others joint intentions and joint attention. As part of this collaborative structure, humans have developed various conventionalized devices for coordinating their social interactions, including both pointing and linguistic symbols. Both of these are bidirectional communicative signs, learned by imitation and enabling the reversal of roles in the communicative act; they are both therefore socially shared. Because they are “arbitrary”, and not indexical, humans may use linguistic symbols to indicate explicitly a virtual infinity of different conceptual perspectives on things – but still the collaborative structure of pointing and linguistic symbols are fundamentally the same.

Second and relatedly, to be effective both pointing and linguistic communication must take into account the perspective of the recipient (“recipient design” à la Schegeloff 2006; see also Enfield 2006, and Levinson 2006). In many cases, pointing presupposes the joint attentional common ground as “topic” (old or shared information), and the pointing act is actually a predication, or focus, informing the recipient of something new, worthy of her attention. In other cases, pointing serves to establish a new topic, about which further things may then be communicated. Both of these are functions served by whole utterances in linguistic communication (see Lambrecht’s 1994, predicate focus and argument focus constructions). When human infants first begin talking, many of their earliest utterances are combinations of gestures (mostly pointing) with words, which divide up in various ways the topic and focus functions (Tomasello 2003b). Language goes beyond deictic gestures in the ease with which linguistic symbols may be grammaticalized into constructions with complex topic-focus configurations, of course, but again the building blocks are the same.

Third, the motivations for pointing and communicating linguistically are basically the same (with the possible exception of some performatives that can only be formulated linguistically). In both cases, the most fundamental motivations are helping and sharing, including informing as a special case.

Interestingly, Dunbar (1996) has argued and presented evidence that in the evolution of human language the motive to gossip – to simply share information for no immediate reason – is the key motive. His argument is that modern languages are much more complicated than they need to be to simply coordinate human actions in the here and now; their complicated structure reveals in various ways the need to communicate about things displaced from the here and now in complicated ways. The important point for current purposes is simply that the basic motives of such narrative discourse are sharing and informing, the same basic motives underlying the pointing gesture.

Fourth and finally, in most analyses acts of linguistic communication comprise two fundamental components: proposition and propositional attitude (locution and illocution). But pointing also includes these two components. The locutionary aspect is the spatial indication of the intended referent, for example, a toy. But then it is something else again – the illocutionary aspect – that determines whether the pointing gesture is taken to be an imperative (I want you to bring me that toy) or declarative (I want you to share my interest in that toy) or informative (I want you to find the toy you are seeking). It hardly needs emphasizing how much further language takes us in formulating complex linguistic constructions embodying complex propositions and propositional attitudes. But again it must be noted that the roots of this complexity are already present in the much simpler communicative act of pointing.

## **Conclusion**

To explain human cognitive uniqueness, many theorists invoke language. This of course contains an element of truth, since only humans use language and it is clearly important to, indeed constitutive of, uniquely human cognition in many ways. However, as I have noted before, asking why only humans use language is like asking why only humans build skyscrapers, when the fact is that only humans, among primates, build freestanding shelters at all. And so for my money, at our current level of understanding, asking why apes do not have language may not be our most productive question. A much more productive question, and one that can currently lead us to much more interesting lines of empirical research, is asking the question why apes do not even point.

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# Index of subjects

- accommodation, 42, 47, 61
- acquisition, 3, 6, 9, 82–87, 90, 94–95, 143–145, 147–148, 151–152, 154, 156, 163–166, 191, 352
- adaptation, 7, 29, 41–42, 322, 329, 346, 388
- adaptive, 4, 26, 28, 32, 41–42, 44, 144, 242
- adjunct, 11, 192–193, 227, 229–230, 232–235, 241
- agent-based models, 83
- analogical extension, 35, 37–38, 56, 58
- analogy, 5, 6, 12–14, 24–26, 35–38, 41, 48, 57, 135, 156, 225, 239–241
- Bequemlichkeitstrieb, 1
- bias, 3, 5, 82, 85, 87–88, 92–93, 95–97, 119, 157, 166
- bimanual coordination, 6, 307, 308, 320, 321, 322, 323, 324, 325, 328, 329
- communities
  - open ~, 5, 12
  - tight knit ~, 12, 203
- complement, 11, 12, 179, 181–182, 240, 253, 255, 264
- complementizer, 184, 186, 188, 191
- complex adaptive system, 143–144, 368
- complexity, 2, 5, 12, 82, 105, 118, 124–125, 135, 139, 205, 207–208, 213, 272, 389
- compositional, 4, 10, 14, 104, 121, 122, 123, 124, 131
- compositionality, 10, 121, 124, 134–135, 229
  - emergence of ~, 29, 122–124
- computational models, 144, 167, 169
- connectives, 103, 115, 130, 134–135, 240
- conspecific, 13, 375, 377–378
- construction, 3, 5, 7, 11–12, 35–37, 52, 56, 60–61, 77–78, 84–85, 92, 95, 97, 115, 149, 184, 192, 219–229, 232, 235–240, 242–243, 256–257, 260, 276, 279, 285, 290, 309, 311, 341, 354–355, 362, 365, 388–389
- construction grammar, 219, 220, 223–224, 228, 235–236, 238–240, 242–243
- convention, 54, 108, 110–111, 119, 383
- convex concepts, 4, 118–120
- convex meaning, 118–120, 133, 136–137
- creole genesis, 28, 143, 146, 165, 167
- creolization, 8, 9, 12, 16, 254, 261, 270
- cultural evolution, 23–26, 28, 62–63, 80, 82–83, 88, 92, 98, 105, 119
- cycle, 2–3, 9, 82, 157, 159, 161, 163, 165–166, 179, 185–186, 192–193, 204, 207, 234, 347, 349–350
- degree modifier, 219, 220, 226–227, 229–231, 233–236, 238–241
- Deutlichkeitstrieb, 18
- economy, 4, 11, 30, 45–46, 179, 183–184, 187–188, 190, 194, 241, 364
  - ~ principles, 11, 46, 179, 183–184, 187, 194
- E-language, 147, 154, 160, 162, 164–166, 168
- evolutionarily stable, 110–112, 115, 117, 119–120, 122, 131
- evolutionary game theory, 4, 8, 10, 30, 105, 110–112, 115, 122, 124–126
- evolutionary stable strategies, 10, 111, 117, 126
- evolutionary theory, 8, 24–25, 27, 31, 34, 45, 51–52, 337, 342–343, 346–348, 362
- exaptation, 35–36, 38, 63, 190, 297, 350, 362

- exemplar models, 37, 50–51
- expansion, 29, 146, 166, 168, 201, 206, 209, 222, 230, 234
- features, 6, 9, 10, 15–16, 26, 34, 52, 81, 86–87, 94, 104–105, 112, 118, 122–123, 145, 149, 151, 153–154, 158, 166, 169, 181, 187, 190, 220, 241, 260, 271, 278, 281, 283, 285–286, 288–290, 307–308, 311, 316, 319–320, 329, 337–338, 340–343, 346, 348, 351, 353–358, 361, 362, 363, 364, 366, 368
- Filtered Learning Model, 9, 83–88, 90, 91–92, 94, 96–98
- Filtered Variational Learning Model, 90, 96–98
- first language acquisition [L1 acquisition], 9, 51, 145, 148
- frequency, 3, 39, 42, 46, 59, 78–80, 83–84, 86–87, 90, 92, 161, 165, 265, 272
- game theory, 103, 106, 108, 110–111, 139
- gaze-following, 13, 375, 377
- generative grammar, 11
- gesture, 14, 190, 265, 307–308, 316, 321, 325, 327–328, 359, 375–379, 383–385, 388–389
- gradual language death, 97
- gradualness, 225
- grammaticalization, 3, 7, 15, 31–32, 35, 40, 63, 179–180, 183, 187, 189–190, 193, 194, 219–225, 227, 234–243, 256, 260, 339, 350, 362
  - ~ cline, 3
- head, 11, 76, 182–186, 189–190, 193–195, 226–227, 229, 233–234, 260, 262, 264, 283, 287, 338, 341, 360
- holistic communication, 10
- i-language, 160, 162–163, 166, 168
- inflectional affixation, 337, 339–340, 342, 344, 348–357, 359, 365, 368
- inflectional morphology, 145, 147, 150, 156, 163–165, 205, 208, 212, 243, 337
- information structure [topic, comment], 6, 13, 43, 307, 328
- innovation, 11, 12, 33–40, 42–46, 192, 204, 208, 210, 238, 243, 269
- intentional [intentionality], 14, 378–379, 382, 385–387
- intraspeaker variation, 39, 92, 97
- invisible-hand process, 12, 44–45
- language
  - ~ acquisition, 3, 6, 11, 33, 51, 80, 87–88, 90–91, 98, 122, 144–145, 148, 156, 191, 316
  - ~ bioprogram, 143, 145, 167
  - ~ change, 2–5, 7–9, 11–12, 23–25, 27–33, 38, 45, 47, 50–52, 54–55, 60–63, 75, 82, 84, 86, 88, 92, 94, 97, 177, 189, 194, 200, 203, 219, 239, 243, 254, 362
  - ~ contact, 5, 8, 60–61, 82, 85, 92, 143, 151–153, 156–157, 159–160, 169, 254
  - ~ decay, 2, 58
- larynx, 6, 262, 265, 299, 307
- late merge, 187, 189–190, 195
- laterality, 320–321
- lingueme, 36, 48–49, 51–52, 54, 56, 59
- meaning space, 118–120, 123, 127–128, 133, 136–137
- meme, 26–27, 48–49, 50–54, 55–56, 59
- memetics, 50–52, 54
- merge, 179–183, 187, 189–190, 192, 194
- monkeys, 117, 316–317, 320–322, 386
- motivation, 38–39, 46, 63, 103, 105, 109, 111, 129, 134, 137, 139, 147, 241, 351, 364, 382, 386–388
- Nash equilibrium, 108–113
- parameters, 8, 13, 15–16, 39, 81, 92, 149, 154, 157–158, 161, 168, 182, 186, 221, 286, 337–339, 342, 344–347, 349, 351–362, 364, 366, 368
  - linguistic, 8, 347
- partitive, 226–233, 235–236, 238–239, 241

- predication, 269, 283, 287, 310, 313, 317–318, 388
- prehistory, 6, 199–200, 202–203, 208–209, 212–213
- prepositions, 56–57, 59, 135–137, 183–184, 188, 190–191, 193, 195, 228, 260, 295
- priming, 7, 24, 47, 55–62
- quantifier, 103, 220, 226–228, 230–231, 233–234, 312–313, 366–367
- quasi-modals, 5
- reanalysis, 11, 36–39, 193, 220, 225, 239–240, 340, 344, 357
- recursivity, 298, 319–320
- relations, 32, 40, 81, 135–138, 179, 182, 192, 194, 199–200, 204, 221, 224–225, 242, 256, 264, 297, 307, 310, 312, 318, 325, 328, 340–341, 345, 359
- relexification, 15, 147
- renewal, 63, 192, 234, 240
- replication, 3, 7, 26–28, 30, 32–33, 35–36, 38–40, 45, 47–52, 54–56, 58–62, 234
- Rich Agreement Hypothesis, 351–353, 359, 364–366, 368
- schema, 263–264, 355
- second language acquisition [L2 acquisition], 8–9, 143, 145–148, 151–152, 162, 165
- selection, 3, 7–8, 15–16, 24–30, 32–34, 38, 40–47, 49, 59, 91, 105, 111, 119, 169, 187, 324, 337, 342, 345, 359, 368
- semantic universals, 103–104, 136, 139
- sign language, 307–308, 325–326, 349–351
- signalling games, 4, 10, 105–108, 110
- simplification, 167, 205, 207–213
- simulation model, 8–9, 82–88, 94–99, 159, 165, 162, 167
- sociolinguistics, 41–42, 44, 47, 148, 194, 203, 207, 209, 213
- specifier, 11, 179, 181–185, 190, 192–195, 264, 278, 354, 365–366
- substrate, 15, 52–53, 147, 150, 152, 169, 253, 261, 270, 296, 347
- superstrate, 15, 143, 146–147, 150, 152–153, 161, 270
- SVO language, 4, 9, 150, 287
- syntactic parameter, 13, 354, 368
- topic/comment structure, 307–308, 311, 313, 316–320, 325, 327, 329
- transformational learning, 87–89
- typology, 75, 77, 195, 202–203, 287–288, 345, 360
- unidirectionality, 3, 7, 58, 62–63, 241
- uniformitarian principle, 202–203, 241–242
- Universal Darwinism, 24–25, 31
- Universal Grammar, 13–15, 103–104, 148, 180, 195, 255, 260, 267–268, 271, 337–339, 342, 348–349, 355–359, 361–362
- utility, 4, 10, 104–107, 109–112, 116–117, 120–121, 124, 126, 130, 135, 139
- variation, 8–9, 24–28, 32–34, 38–43, 46–47, 75, 80, 83, 89–90, 92, 95–97, 199, 202, 204, 208, 213, 224, 242, 265, 271, 344, 346–347, 361, 367
- Variational Learning Model, 83, 86, 90–92, 94–95, 97–98
- verb-final, 9, 341
- Voronoy tessellations, 10
- word order, 1, 9, 29, 31, 75, 80–83, 86, 90, 93, 95, 149–150, 161–162, 220, 239, 243, 265, 312, 341, 352, 357, 362
  - ~ correlations, 75, 81–83, 90, 93



## Index of languages

- Akan, 75  
American Indian, 150  
American Sign Language, 326, 349  
Cape Verdean Creole Portuguese, 366  
Creole languages, 6, 12, 15, 275, 297  
Danish, 212, 351, 352  
Dutch, 56, 150–151, 240, 258–259, 275, 343  
Early Modern English, 150, 155  
Edo, 357  
E-language, 147, 154, 160, 162, 164–166, 168  
English, 4, 9, 29, 31, 35, 43, 49, 57, 60, 75–78, 80, 104, 150–152, 165, 180, 184–185, 188–192, 195, 206, 210–212, 219–220, 226, 228, 230–234, 239–240, 244, 254, 256–257, 263, 269, 275, 280, 286, 311, 349, 351, 355, 362–365, 395  
Ewe, 150, 261  
Faroese, 212, 352, 365  
Fon, 150, 155, 162, 165  
French, 17, 29, 61, 146, 151, 184, 186, 189, 192, 206, 220, 228, 234, 240, 256–259, 261, 263, 269–270, 280, 316, 343, 362  
German dialects  
    Alemannic, 43  
    Bavarian, 43  
Guinea-Bissau Creole Portuguese, 366–367  
Gumuz, 75  
Icelandic, 212, 351–352, 354–355, 363, 365  
Japanese, 76, 311, 319, 341  
Javanese, 150  
Juba Arabic, 15, 255–257, 260–261, 274–275  
Kikongo, 150, 162, 165  
Kriyol, 15, 255–257, 261–262, 264, 275, 282, 289, 293–294  
Lingala, 343, 350  
Louisiana Creole French, 367  
Mande, 357–358  
Martiniquais, 15, 255–259, 261  
Middle English, 35, 77–79, 192, 228, 230, 232, 244  
Mohawk, 269, 338, 341, 361  
Ngukurr Creole, 15  
Norwegian, 185, 211, 352–353, 355, 365  
Old English, 35, 43, 77–79, 187, 192–193, 228, 232, 234, 362  
Old Norse, 184–185  
Portuguese, 15, 150, 256–257, 259, 261, 264, 275–276, 282, 291–292, 343  
Riau Indonesian, 12  
Romance, 61, 254, 339, 365, 396  
Seme, 75  
sign language, 307–308, 325, 349–350  
Siroi, 75  
Slave, 75, 154, 360  
Sorbian, 75  
Spanish, 60, 272  
    Colombian ~, 60  
Sranan Tongo, 8, 144–145, 148–151, 158, 166–169, 271, 288, 343  
Surinam Creoles, 8, 145, 150–152, 154, 271, 298, 343  
Swedish, 191, 351–353, 363  
Twi, 150, 162, 165

## Index of persons

- Abraham, Werner, 183  
Adone, Dany, 255  
Agnetta, Bryan, 376  
Alario, F.-Xavier, 322  
Allen, Cynthia, 79  
Andersen, Henning, 199, 203, 207, 225, 235, 240, 242  
Andor, Jozsef, 239  
Andrade, Ernesto d', 295  
Andrade, Lelia Lomba De, 275  
Annett, Marian, 307, 320–321  
Anttila, Raimo, 225, 239  
Arbib, Michael A., 55, 321–322  
Arends, Jacques, 143–146, 149–152, 154, 160, 166–168, 270–271, 288, 298  
Arsuaga, Juan Luis, 299  
Athènes, S., 322  
Axelrod, Robert, 158  
Axtell, Robert L., 145  
Bailey, Beryl L., 258, 260, 273  
Bailey, Charles-James N., 254  
Baker, Mark C., 16, 231, 269, 278, 283, 286, 290, 337–339, 341, 345, 356, 359–363, 366, 368  
Baptista, Marlyse, 261, 275–276, 286, 366–367  
Barnes, Michael, 352  
Barsalou, Lawrence W., 55  
Barth, Fredrik, 204  
Barwise, Jon, 313  
Bates, Elizabeth, 379  
Baxter, Gareth J., 45  
Beaken, Mike, 207  
Becker, Angelika, 167, 271  
Behne, Tanya, 377  
Bentley, Jerry H., 207  
Berg, Margot van den, 17, 144, 149, 375  
Bergs, Alexander, 243  
Bernabé, Jean, 255  
Bhatia, Tej K., 148  
Bialystok, Ellen, 157  
Biber, Douglas, 231  
Biberauer, Theresa, 81  
Bichakjian, Bernhard H., 25  
Bickerton, Derek, 6, 118, 143, 145, 167, 192, 199, 256–257, 271, 283, 337  
Bierwisch, Manfred, 52  
Bisang, Walter, 223, 237  
Blackmore, Susan, 26–27, 52–54  
Blake, Barry J., 81  
Blevins, Juliette, 28, 30, 33, 38–40, 61  
Bley-Vroman, Robert W., 148  
Bloom, Paul, 337, 361  
Bobaljik, Jonathan David, 337, 352–354, 365  
Bock, J. Kathryn, 41, 55, 56, 57, 60  
Boë, Louis-Jean, 265, 299  
Boesch, Christophe, 386  
Boesch, Hedwige, 386  
Bollée, Annegret, 271  
Bomhard, Allan R., 207  
Boroditsky, Lera, 57, 59  
Borst, Eugen, 240  
Boyd, Robert, 386  
Bratman, Michael E., 385–386  
Braun, Maria, 149  
Braunmüller, Kurt, 199  
Brighton, Henry, 29, 81  
Brinton, Laurel, 63  
Briscoe, Ted, 3–4, 9, 83–84, 144  
Brooks, Alison S., 243  
Bruyn, Adrienne, 144, 149  
Büring, Daniel, 310, 319  
Bush, Robert R., 91  
Butterworth, George, 383  
Bybee, Joan L., 51, 54, 220–222, 236–238

- Caïd-Capron, Leila, 274  
 Call, Josep, 375–377, 384, 386  
 Calvin, William H., 54  
 Campbell, Donald T., 204  
 Campbell, Lyle, 225  
 Carlson, Greg N., 312  
 Carpenter, Malinda, 378–379, 382, 384  
 Carstairs-McCarthy, Andrew, 15, 61, 253  
 Castells, Manuel, 212  
 Cavalli-Sforza, Luigi L., 84, 208  
 Chafe, Wallace L., 309–310  
 Chalmeau, Raphael, 386  
 Chang, Franklin, 58–59  
 Changeux, Jean Pierre, 53  
 Chaudenson, Robert, 143, 146, 151–152, 160  
 Chen, Frances, 169, 378  
 Cheney, Dorothy L., 386  
 Chomsky, Noam, 11, 155, 179–181, 187, 189, 239, 272, 337  
 Christiansen, Morten, 23  
 Clahsen, Harald, 148  
 Clark, Brady Z., 3–4, 8–9, 28, 31, 75–102, 395  
 Clark, Herbert H., 378, 385, 387–388  
 Claudi, Ulrike, 225  
 Clements, J. Clancy, 295  
 Comrie, Bernard, 201, 242, 349  
 Cooper, Robin, 313  
 Corballis, Michael C., 190, 320–321  
 Corbett, Greville G., 272  
 Corne, Chris, 258, 261, 280, 298  
 Crawford, Meredith P., 108, 386  
 Crelin, Edmund S., 262, 265  
 Croft, William, 28, 33–34, 36–39, 42, 44–49, 51–52, 54, 59, 62–63, 220, 223–226, 237, 241–242, 269, 278, 288, 290  
 Cruse, D. Alan, 220, 224, 226  
 Culicover, Peter W., 144  
 Cziko, Gary, 25, 36, 40, 53  
 Dahl, Östen, 238, 242–243  
 Dasher, Richard B., 241  
 Dawkins, Richard, 24–26, 29, 48–49, 52–53, 59  
 De Cat, Cécile, 316  
 Deacon, Terrence W., 199, 337, 362, 368  
 DeGraff, Michel, 155, 156  
 Dehaene, Stanislas, 53  
 Delius, Juan D., 53  
 Dell [ALL], 267  
 Dell, François  
 Dell, Gary  
 Denison, David, 227, 228, 229, 235  
 Dennett, Daniel C., 24, 25, 26, 27, 32  
 Déprez, Viviane, 273  
 Desjardins, Renee N., 147  
 Deutscher, Guy, 2, 38, 396  
 Diessel, Holger, 184  
 Diewald, Gabriele, 243  
 Dixon, Robert M.W., 28–29, 203  
 Donaldson, Matina C., 116  
 Dowty, David R., 137  
 Dretske, Fred I., 133  
 Dryer, Matthew S., 75–76, 92  
 DuBois, John W., 42, 241, 311  
 Dunbar, Robin, 200, 337, 389  
 Eckardt, Regine, 1, 16–17, 234, 395  
 Ehala, Martin, 28, 31  
 Eldredge, Niles, 29  
 Ellis, Richard, 346  
 Elman, Jeffrey L., 241  
 Elugbe, Ben Ohi, 357  
 Emmorey, Karen, 326  
 Enfield, Nick J., 17, 327, 378, 388  
 Epstein, Joshua M., 145  
 Escure, Genevieve, 362  
 Evans, Nicholas, 208  
 Faarlund, Jan Terje, 184–185  
 Fabb, Nigel, 155  
 Falgier, Brenda, 326  
 Faurie, Charlotte, 320  
 Feldman, Marcus W., 84  
 Ferber, Jacques, 145  
 Ferguson, Charles, 275  
 Ferraz, Luiz Ivens, 259

- Ferreira, Victor S., 56, 59, 241  
Fillmore, Charles J., 219, 223  
Firbas, Jan, 309  
Fischer, Olga, 5, 12, 35–36, 57, 79  
Fisher, Ronald A., 344  
Fitch, W. Tecumseh, 6–7, 316, 329  
Flores d'Arcais, Giovanni B.  
Flynn, Suzanne, 148  
Foley, William A., 200, 208, 340  
Fortescue, Michael, 208  
Fox Tree, Jean E., 60  
Fox-Keller, Evelyn, 145  
Fraassen, Bas C. van, 118  
Francis, Elaine J., 223, 227  
Franco, Fabia, 383  
Frazier, Lyn, 55  
Frege, Gottlob, 104, 317  
Frey, Werner, 312  
Fried, Mirjam, 220, 223, 236, 238  
Fromkin, Victoria, 52  
Gabelentz, Georg von der, 1–3, 308–309  
Gallese, Vittorio, 55  
Ganger, Jennifer B., 147  
Gärdenfors, Peter, 4, 118–121, 129–130, 136–137  
Garrod, Simon, 55  
Gazdar, Gerald, 134, 135  
Gee, James Paul, 326  
Geeraerts, Dirk, 238  
Gelderen, Elly van, 4, 11, 179, 183, 192, 395  
Gentner, Dedre, 35, 57  
Gerard, Ralph W., 25, 62  
Gil, David, 12, 182, 194, 195, 291  
Gilbert, Margaret, 385  
Giles, Howard, 204  
Gilligan, Gary M., 366  
Givón, T., 27–28, 42, 193, 220, 312  
Gluckman, Andrea, 376  
Goddard, Cliff, 104  
Goldberg, Adele E., 219, 223  
Goldenberg, Gideon, 308  
Goldinger, Stephen D., 51  
Golinkoff, Roberta M., 379  
Gonçalves, Manuel da Luz, 275  
Goodall, Jane, 375  
Gould, Stephen J., 29, 35, 351, 362  
Goyette, Stéphane, 254  
Greenberg, Joseph H., 75, 81, 92  
Gries, Stefan Th., 238  
Grimshaw, Jane, 268  
Gross, Steven, 146  
Guiard, Yves, 322, 329  
Gundel, Jeanette K., 311  
Günther, Wilfried, 259  
Haberl, Katharina, 381  
Haldane, John B. S., 344  
Hale, Ken, 179, 182  
Hall, Beatrice L., 254  
Hall, R. M. R., 254  
Halle, Morris, 354  
Halliday, Michael A. K., 309  
Hare, Brian, 375, 377, 386  
Harris, Alice, 225  
Hartsuiker, Robert J., 60  
Haspelmath, Martin, 27–28, 30, 32–33, 38, 41–42, 44, 48, 57, 239  
Hauser, Marc D., 320  
Hawkins, John A., 29, 75, 81–82, 84–85, 92, 361  
Heath, Jeffrey, 340  
Heim, Irene, 310  
Heine, Bernd, 5, 35, 57, 60–61, 189, 220, 222, 225–226  
Herzog, Marvin, 32  
Hill, Jane, 341  
Himmelmann, Nikolaus P., 220–222  
Hockett, Charles, 309  
Hoeksema, Jack, 240  
Holland, John H., 144  
Holm, John, 146, 256  
Holmberg, Anders, 352  
Hopkins, William D., 320, 376  
Hopper, Paul J., 34, 36–38, 220, 225, 234–235, 239  
Horn, Laurence R., 135

- Householder, Fred W., 34  
Huddleston, Rodney, 279  
Hudson(-Kam), Carla L., 145, 156, 161  
Hull, David L., 25, 48–50, 54  
Humboldt, Wilhelm von, 1–2, 212  
Hünemeyer, Friederike, 225  
Huttegger, Simon M., 113  
Hyltenstam, Kenneth, 156  
Ichinose, Atsushi, 286  
Ionin, Tonia, 156  
Israel, Michael, 227  
Itakura, Shoji, 376  
Iversen, Ragnvald, 352  
Jackendoff, Ray, 118, 155  
Jacobs, Joachim, 310, 314  
Jäger, Gerhard, 16, 26, 28–31, 33, 36, 41–42, 44–45, 51, 55–59, 63, 82, 96, 99, 103, 105, 119, 125, 315, 395  
Jelinek, Eloise, 182, 360  
Jespersen, Otto, 2–3, 185, 234  
Johannessen, Janne Bondi, 185  
Johnson, Jacqueline S., 145, 147, 156  
Johnson, Keith, 51  
Johnson, Kyle, 352  
Jonas, Dianne, 354, 365  
Jonker, Leo B., 112–113  
Josefsson, Gunløg, 191  
Kaminski, Juliane, 375  
Kamp, Hans, 315  
Kathol, Andreas, 286  
Kauffman, Stuart, 31, 368  
Kaufman, Terence, 146–147  
Kay, F. George, 151  
Kay, Paul, 219, 223  
Kaye, Jonathan, 267  
Kayne, Richard S., 188, 265  
Kegl, Judy, 326  
Keller, Rudi, 28, 33, 44–47, 54, 62–63  
Kelter, Stephanie, 56  
Kendon, Adam, 321  
Kerns, John C., 207  
Keyser, Jay, 179, 182  
Kihm, Alain, 6, 13, 15, 253, 255, 258, 261, 267, 271–272, 277, 286, 291–292, 295, 367, 395  
Kimura, Doreen, 321  
Kiparsky, Paul, 225, 241  
Kirby, Simon, 4, 9, 23, 28–31, 33, 41–42, 46–47, 51, 62, 81–92, 94, 96–99, 119, 123, 131, 139, 144, 337, 362, 368  
Klamer, Marian, 207  
Klein, Wolfgang, 275, 291  
Knecht, Stefan, 321  
Komarova, Natalia L., 126  
König, Ekkehard, 222, 225  
Konopka, Kenneth, 4, 9, 75, 99, 395  
Koopman, Hilda, 85, 361  
Kouwenberg, Silvia, 259  
Krakauer, David C., 107, 117, 122–123, 125  
Kratzer, Angelika, 275  
Kress, Bruno, 363  
Krifka, Manfred, 6, 13–15, 307, 312, 396  
Kroch, Anthony S., 40, 42, 57, 77, 79–80, 87, 89, 92, 97, 239  
Kruger, Ann C., 383  
Kuczaj, Stanley, 191  
Kuppevelt, Jan van, 319  
Kuroda, Shige-Yuki, 309  
Kusters, Wouter, 2, 5, 12–13, 199, 203–205, 208–209, 396  
Kuteva, Tania, 35, 60–61, 189  
Labov, William, 32, 41, 44–46, 202  
Lachmann, Michael  
Lambrecht, Knud, 184, 310, 312, 388  
Langacker, Ronald W., 49, 54, 241–242, 340  
Larsen-Freeman, Diane, 148  
Lass, Roger, 25, 28–29, 31–36, 38, 41, 43, 48, 50, 52, 54, 63, 241, 340, 362  
Lavenda, Robert H., 207–208  
Lazard, Gilbert, 364  
Leavens, David A., 376  
Leech, Geoffrey N., 104, 136  
Leeson, Lorraine, 326

- Lefebvre, Claire, 147, 261  
Lehmann, Christian, 220–222, 237  
Lehmann, Winfred P., 81  
Lennert Olsen, Lise, 208  
Levelt, Willem J. M., 56  
Levinson, Stephen C., 17, 375, 388  
Lewis, David K., 10, 105, 107–111  
Lewontin, Richard, 88, 351, 362  
Li, Charles N., 110–113, 125–126, 312–313, 318, 362  
Lieberman, Philip, 262, 265, 299  
Lightbown, Patsy M., 148  
Lightfoot, David, 4, 11, 28–29, 31, 33, 51, 186, 239, 337, 346, 348  
Lindblom, Björn, 28, 30  
Liszkowski, Ulf, 379, 380  
Löbner, Sebastian, 313  
Loebell, Helga, 56, 60  
Long, Michael H., 148  
Longa, Victor M., 32  
Lounsbury, Floyd G., 136  
Lowenstamm, Jean, 266–267  
Luka, Barbara, 55  
Lumsden, John S., 156  
MacNeilage, Peter F., 307, 320–323, 329  
Madge, David, 347  
Majerus, Michael E. N., 344  
Mallison, Graham, 81  
Marantz, Alec, 354  
Markman, Arthur B., 57  
Markman, Ellen, 380  
Maroldt, Karl, 254  
Marty, Anton, 309  
Matras, Yaron, 60  
Maynard Smith, John, 30, 111  
McBrearty, Sally, 243  
McMahon, April, 23, 25, 28, 30, 33–34, 38  
McManus, Chris, 321  
McNeill, David, 321, 329  
McWhorter, John H., 5, 12–13, 15–16, 147, 160, 166, 199, 337, 343, 396  
Meijer, Paul J., 60  
Meillet, Antoine, 220, 239–240  
Michaelis, Laura A., 223  
Migge, Bettina, 149  
Miller, Catherine, 255, 274  
Milner, Brenda, 321  
Milroy, James, 33–34, 199, 203  
Milroy, Lesley, 33–34, 99, 199  
Miyake, Akira, 145, 156  
Moll, Henrike, 381, 382  
Molnár, Valéria, 310  
Mondorf, Britta, 43–44, 46  
Moore, Chris, 387  
Mosteller, Frederick, 91  
Mufwene, Salikoko, 28, 143  
Mühlhäusler, Peter, 146, 150, 203, 270, 274, 280  
Muysken, Pieter, 146, 148  
Nehaniv, Chrystopher L., 317  
Nettle, Daniel, 28–29, 44, 63, 209, 211  
Neumann, Ingrid, 159, 367  
Neville, Helen J., 147  
Newmeyer, Frederick J., 46, 182, 194–195, 350, 360–361, 364, 368  
Newport, Elissa L., 145, 147, 156, 161  
Nichols, Johanna, 202–203, 209, 340  
Niepokuj, Mary K., 40–41  
Niyogi, Partha, 84  
Norde, Muriel, 63  
Nowak, Andrzej, 144  
Nowak, Martin A., 3, 105, 107, 117, 122–123, 125  
Ochs, Elinor, 161  
Oostendorp, Marc van, 209–210  
Östman, Jan-Ola, 220, 223, 236  
Owens, Jonathan, 260  
Pagliuca, William, 220–222, 236–237  
Palmer, A. Richard, 320  
Partee, Barbara H., 122, 124, 311  
Patou-Mathis, Marylène, 299  
Paul, Hermann, 308, 309  
Pawlowitsch, Christina, 113  
Penke, Martina, 23  
Penny, Ralph, 203, 207

- Perdue, Clive, 275, 291  
 Perkins, Revere D., 199, 208, 220–222, 236–237, 341  
 Peters, Hans, 240  
 Peters, Stanley, 132  
 Pickering, Martin J., 55  
 Pierrehumbert, Janet B., 51, 99  
 Pinker, Steven, 337, 361  
 Pintzuk, Susan, 77–79, 92  
 Plag, Ingo, 149  
 Platzack, Christer, 351–352  
 Plotkin, Henry, 25  
 Pollock, Jean-Yves, 351–352  
 Pommerol, Patrice Jullien de, 256  
 Portner, Paul, 310, 315  
 Post, Marike, 259  
 Povinelli, Daniel J., 378, 386  
 Price, George R., 111  
 Price, Richard, 144  
 Price, Sally, 144  
 Prunet, Jean-François, 267  
 Pullum, Geoffrey K., 134–135, 279, 339  
 Pulvermüller, Friedemann, 53, 58  
 Quine, Willard V. O., 119, 122  
 Quirk, Randolph, 231  
 Rasmussen, Theodore, 321  
 Ratner, Hillary H., 383  
 Raymond, Michel, 320  
 Reinhart, Tanya, 309–310, 314–315, 324  
 Rissanen, Matti, 363  
 Ritchie, William C., 148  
 Ritt, Nikolaus, 23, 27–28, 31, 33, 41–42, 48–54, 58, 61–63  
 Rizzi, Luigi, 366  
 Rizzolatti, Giacomo, 55, 321–322  
 Roberts, Craige, 319  
 Roberts, Ian, 6, 81, 183, 190, 271, 337, 351–352  
 Roberts, Sarah J., 167  
 Rohdenburg, Günter, 46  
 Rohrbacher, Bernhard, 337, 351–352  
 Roland, Douglas W., 241  
 Romaine, Suzanne, 192, 209, 211  
 Rooij, Robert van, 4, 8, 10, 82, 103, 105, 119, 125, 396  
 Rosenbach, Anette, 7–8, 23, 30, 35–36, 55, 57–59, 63, 396  
 Ross, Malcolm D., 5, 12, 203  
 Rottet, Kevin, 258, 367  
 Rougé, Jean-Louis, 259, 291–292  
 Roussou, Anna, 183, 190  
 Rubinstein, Ariel, 138  
 Rumelhart, David E., 53  
 Ryan, Lisa, 148  
 Rydén, Mats, 192  
 Sabino, Robin, 274  
 Sadock, Jerrold M., 227  
 Saeed, John I., 326  
 Saffran, Jenny R., 161  
 Safir, Kenneth J., 366  
 Sakel, Jeanette, 60  
 Sampson, Geoffrey, 25  
 Sandler, Wendy, 325, 329  
 Sankoff, Gillian, 288  
 Sasse, Hans-Jürgen, 309, 318  
 Satterfield, Teresa, 3, 8–9, 143–144, 165, 167, 396  
 Savage-Rumbaugh, E. Sue, 376  
 Schieffelin, Bambi B., 161  
 Schleicher, August, 54  
 Schlieben-Lange, Brigitte, 240, 254  
 Schreuder, Robert, 56  
 Schultz, Emily A., 207–208  
 Schwe, Helen, 380  
 Searle, John R., 385  
 Sebba, Mark, 146, 167  
 Sebeok, Thomas A., 133  
 Seiler, Guido, 28, 33, 39, 43–44  
 Seuren, Pieter A. M., 149  
 Seyfarth, Richard M., 386  
 Sgall, Peter, 309  
 Shah, Priti, 145, 156  
 Sharwood-Smith, Michael, 155  
 Siegel, Jeff, 149, 159, 199, 203  
 Simpson, Andrew, 190  
 Singler, John V., 146–147

- Singleton, David, 148  
Skyrms, Brian, 131  
Slobin, Dan I., 241  
Smith, Kenny, 29, 81  
Smith, Norval, 146  
Sollid, Hilde, 185  
Sorace, Antonella, 147–148  
Southworth, Franklin C., 254  
Spalding, Karen, 201  
Speelman, Dirk, 238  
Sperber, Daniel, 378  
Stalnaker, Robert, 118, 315  
Stamenov, Maxim I., 55  
Starr, Chester G., 207–208  
Stassen, Leon, 269  
Steels, Luc, 144  
Stefanowitsch, Anatol, 238  
Stein, Dieter, 63  
Ste Vick, Robert D., 25, 62  
Stewart, Ian, 31–32, 232  
Strawson, Peter, 313  
Strozer, Judith R., 145, 156  
Struhsaker, Thomas T., 316  
Svenonius, Peter, 81  
Szabolcsi, Anna, 312  
Tabor, Whitney, 225, 227, 229  
Tattersall, Ian, 297  
Taylor, Ann, 77, 79  
Taylor, Peter D., 112–113  
Teyssier, Paul, 292  
Thomason, Sarah G., 146–147  
Thompson, Sandra A., 313, 318  
Thráinsson, Höskuldur, 337, 352–354  
Thurston, William R., 199, 203  
Tomasello, Michael, 7, 13–14, 17, 316, 329, 375–388, 396  
Tosco, Mauro, 260  
Traugott, Elizabeth Closs, 5, 11–12, 34, 36–38, 63, 190, 219–220, 222, 225, 227, 235, 239, 241, 243, 396  
Travis, Catherine E., 60  
Travis, Lisa, 351, 361  
Trousedale, Graeme, 239  
Trudgill, Peter, 5, 12, 61, 199, 203, 208, 340  
Truscott, John, 155  
Tummers, José, 238  
Tuomela, Raimo, 385  
Urciuoli, Bonnie, 204  
Uriagereka, Juan, 262  
Valdman, Albert, 152  
Valin, Robert van, 340  
Vauclair, Jacques, 321  
Veenstra, Tonjes, 16, 145, 167, 271, 395  
Vennemann, Theo, 81  
Vergnaud, Jean-Roger, 267  
Vikner, Sten, 351–352  
Volterra, Virginia, 379  
Vrba, Elizabeth S., 35  
Vygotsky, Lev S., 382  
Wallman, Sandra, 204  
Warneken, Felix, 378, 381  
Wärneryd, Karl, 112, 117  
Wasow, Thomas, 241  
Weber-Fox, Christine M., 147  
Wedel, Andrew B., 28, 33, 40, 50–51  
Weil, Henri, 308  
Weinreich, Uriel, 32, 34, 87, 89, 97  
Weizsäcker, Ernst-Ulrich von, 210  
Werker, Janet F., 147  
Wessén, Elias, 184  
Wexler, Kenneth, 156  
White, Lydia, 148  
Whitney, William D., 202  
Widdows, Dominic, 132  
Wiemer, Björn, 223  
Wierzbicka, Anna, 341  
Wildgen, Wolfgang, 28, 31  
Wilson, Deirdre, 378  
Wilson, Frank, 329  
Winford, Donald, 144–147, 149, 156  
Wolfram, Walt, 97  
Wong, Kate, 219  
Wu, Xiu-Zhi Zoe, 190  
Yabushita, Katsuhiko, 310, 315  
Yang, Charles D., 80, 83, 87–92, 94



Yuasa, Etsuyo, 227

Ziegler, Herbert F., 207

Zipf, George Kingsley, 46, 241

Zuberbühler, Klaus, 316

Zuidema, Willem, 122

Zwarts, Joost, 136–137