

From fieldwork to linguistic theory

A tribute to Dan Everett

Edited by

Edward Gibson

Moshe Poliak

Empirically Oriented Theoretical
Morphology and Syntax 15



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Contents

Acknowledgments	iii
Preface	v
Anarchy, power, festschrifts, and universals Stefan Müller	ix
1 Composition, not infinity: The irrelevance of recursion to theories of language Edward Gibson	1
2 Daniel Everett on Pirahã syntax Geoffrey K. Pullum	23
3 How language began: A theoretical interpretation Robert D. Van Valin, Jr.	75
4 Cohesive rhythms: Choral narrative in Ega Dafydd Gibbon	85
5 Why is UG such a hard question? Iris Berent	111
6 Just where are the universals? Complexities of place Marianne Mithun	133
7 A Peircean turn in linguistics: Syntactic-semantic composition as logical inference Robert Levine	143
8 Hierarchical syntactic structure in Malay/Indonesian, between Pirahã and Had Gadya David Gil	179

Contents

9	The first-person perspective and second-order desires	
	Yaron M. Senderowicz	211
10	The causative alternation in Italian: A case study in the parallel architecture of grammar	
	Delia Bentley	225
11	Transitivity in Séliš-Ql'ispé	
	Sarah G. Thomason & Daniel Everett	275
12	Contextualizing typologically remarkable sound patterns in Pirahã: A quantitative approach	
	Caleb Everett	305
13	What is the simplest semantics imaginable?	
	András Kornai	323
14	Investigating grammatical borrowing in Mosestén through historical sources	
	Jeanette Sakel	341
15	Modern language models refute Chomsky's approach to language	
	Steven T. Piantadosi	353
	Index	415

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We also wish to thank all the speakers, in-person and virtual, who presented their research in the event on June 8th at MIT, *From Fieldwork to Linguistic Theory: A Tribute to Dan Everett*, which was fully recorded and can now be found on Youtube. We also express special gratitude to those who, in addition to presenting their work in the event, contributed their manuscripts to this publication. This festschrift would not exist without you.

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Finally, the first editor would like to extend a special thank you to Ev Fedorenko, for help with the organization behind the scenes, and for emotional support always.

To all, your contributions have been invaluable in making this Festschrift a fitting tribute to Daniel Everett.

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Preface

It is my pleasure to introduce this Festschrift in honor of Dan Everett. I've known Dan since 1987, when he interviewed for a job at the University of Pittsburgh in the Department of Linguistics. I was a second year grad student in a joint program in computational linguistics between Carnegie Mellon and the University of Pittsburgh, and Dan was interviewing for a syntax and morphology position at Pitt. Dan was striking for his fascinating material on several Native American languages including Pirahã, Wari', and Yagua. He was also striking for his attire: He wore old blue jeans and a red t-shirt with a picture of a parrot on it. The t-shirt was also noticeably worn out with holes in it. He was a *cool* academic. Dan got the job.

After knowing him for a while, I asked Dan to be on my thesis committee because of his breadth of knowledge of language and linguistics, and his friendly manner. He ended up being my co-advisor for my PhD thesis. He was an enormous help to me for his advice on the work that I did in my PhD at CMU. Dan of course was working with the Pirahã at the time, and he invited me then to work with the Pirahã, in the 80s and the 90s, but I wasn't able to go at the time. In 2005, he wrote the famous *Current Anthropology* paper, and he invited me again. This time I was able to accept. I visited in 2007 with Mike Frank, who was my then-student, and in collaboration with Ev Fedorenko, who wasn't able to come. We started some fascinating projects on number words in Pirahã and syntactic recursion in Pirahã, some of which is alluded to in this Festschrift.

Dan grew up in extreme poverty on the U.S–Mexican border. He and his mother lived happily in a trailer park, when she died suddenly of an aneurysm at only 29 years old. Dan was only 11, and he had to go to live with his estranged father in San Diego, who Dan did not get along with. Sadly, Dan's father was abusive, and so Dan had to spend a lot of time on the streets. In 1968, Dan was 16 years old, selling drugs at a rock concert when he met the children of missionaries who were trying to help troubled young people like Dan. Dan got along well with them, and was asked to join them at their house for dinner. Soon thereafter Dan met and fell in love with the missionaries' daughter Keren, who was also 16 at the time.

Preface

Dan told me that he'd never met happy people before this. The missionaries attributed their happiness to their Christian beliefs, so Dan converted. By the time Dan was 19, he and Keren were married, and had a child, Caleb (who is now a professor of anthropology at the University of Miami, and who has written a paper for this volume: Chapter 12). Dan became a missionary after studying at the Moody Bible Institute in Chicago and learning some linguistics at the University of Oklahoma. He moved to Brazil in 1977 to work with the Pirahã, by which time he and Keren had three children. Along the way he got a masters in linguistics from Unicamp in Brazil, and he got the first PhD ever awarded in Linguistics in Brazil in 1983, also from Unicamp.

If you don't know him, one of Dan's great talents is that he can learn a language extraordinarily quickly and well. He knows many, many languages and sounds native in many of them. I know that when I travel with him in Brazil, Brazilians always try to figure out where exactly he is from in Brazil. They're surprised when he says San Diego. Although he only learned Portuguese as an adult in the 1970s, he sounds native. He is also a rare linguist who can figure out the sounds, morphemes, and structure of a language, even with no bilinguals. And that's what he did with the Pirahã. He has worked on many languages, perhaps most with the Pirahã, where he started as a missionary in 1977 and spent over seven full years working and living with them, by which time he became close to bilingual, more so than anyone else ever, according to the Pirahã people. Although he went to the Pirahã to convert them to Christianity, he likes to tell the story that he never converted any of them. In fact, he'll tell you that they helped convert him away from Christianity. Dan ended up getting divorced from Keren (and Christianity) and marrying Linda, to whom he has been happily married now for 15 years.

Dan has published in almost every area of linguistics: in phonetics, phonology, morphology, sociolinguistics, psycholinguistics, historical linguistics, syntax, semantics, philosophy of language, and philosophy of linguistics. He started as an assistant professor in Unicamp in the 1980s, then moved to the University of Pittsburgh in the late 1980s, where I met him, and he has had several other academic positions, culminating in his current position which, is a Trustee Professorship at Bentley University. Dan has a great many academic achievements, only some of which will list here. He has done enormous descriptive work: He wrote a grammar of Pirahã and a grammar of Wari', and he did descriptive work in around 20 languages of the Americas. He has done a lot of phonetic work documenting new sounds in Pirahã and Wari'. He has done important morphological and language documentation work. Working with native speakers, he identified Oro Win as a distinct language in the Chapacuran family. The work I know Dan best

for is the 2005 *Current Anthropology* paper, where he documented Pirahã, the first language known to lack number words, one of the simplest kinship systems ever documented, the first culture documented to lack origin myths, and possibly, most importantly – depending on your perspective – this language was claimed to lack syntactic recursion. This was of theoretical interest because of a paper that Hauser, Chomsky & Fitch wrote in 2002 in the journal *Science*, proposing that a core feature of human syntax (in human language) was recursion. So Dan’s proposal that Pirahã lacked this feature created a bit of a stir that exists to this day.

Dan has also worked on linguistic anthropology, in a book called *Dark Matter of the Mind* from Chicago Press, and his most recent work is coming out soon on the philosophy of linguistics, discussing the work of Charles Peirce. Not only has Dan done all these theoretical and descriptive pieces of research, but he has done important expository work for the general public. He has at least three books that I know: *Don’t sleep there are snakes*, *Language: The cultural tool*, and *How language began*. These books have been translated into around 20 languages around the world and they’re lovely pieces of work on getting linguistics out to the general audience.

This Festschrift includes 15 articles that are related to Dan’s work over the years. It is being released after a tribute event for Dan Everett that was held at MIT on June 8th, 2023.

Cambridge, MA, September 25, 2024

Edward Gibson

Anarchy, power, festschrifts, and universals

Stefan Müller 

Humboldt-Universität zu Berlin

This paper discusses the concept of anarchy as the absence of power and power misuse by one of the most influential anarchists and his followers. I also discuss universals and the case of Pirahã. It is argued that there may not be any real non-trivial universals on the sentence level, but that there is a strong candidate for a universal on the text level: the festschrift universal. I also explain why Dan Everett is the first, last, and hence only person on this planet to get a Language Science Press festschrift.

1 Anarchy and power

Noam Chomsky is not only known for his linguistic work but also for his political views. He is an anarcho-syndicalist. His political followers like anarchistic ideas since anarchy is the absence of power. Humans live in self-regulated communities without oppression by a state or by a group of people who somehow gained an advantage at a certain point and then have (mis-)used it to indoctrinate, command, influence, or exploit other people.

But what is described in the movie *Grammar of happiness* (O'Neill & Wood 2012) and even more clearly in Pullum (2024), Chapter 2 of this volume, is exactly the opposite. Chomsky and others have made the statement that all languages may license an infinite number of utterances in principle (Hauser, Chomsky & Fitch 2002: 1571, Epstein & Hornstein 2005: 4, Hornstein, Nunes & Grohmann 2005: 7), however there are several languages that seem to contradict this claim (see Pullum (2024: 25–26, Section 3.1), Chapter 2 of this volume for a recent overview). Instead of admitting the mistake and restating the claim, which would be a real sign of greatness, Chomsky and other linguists from the US and Brazil have started a campaign against a single person with the aim of destroying that



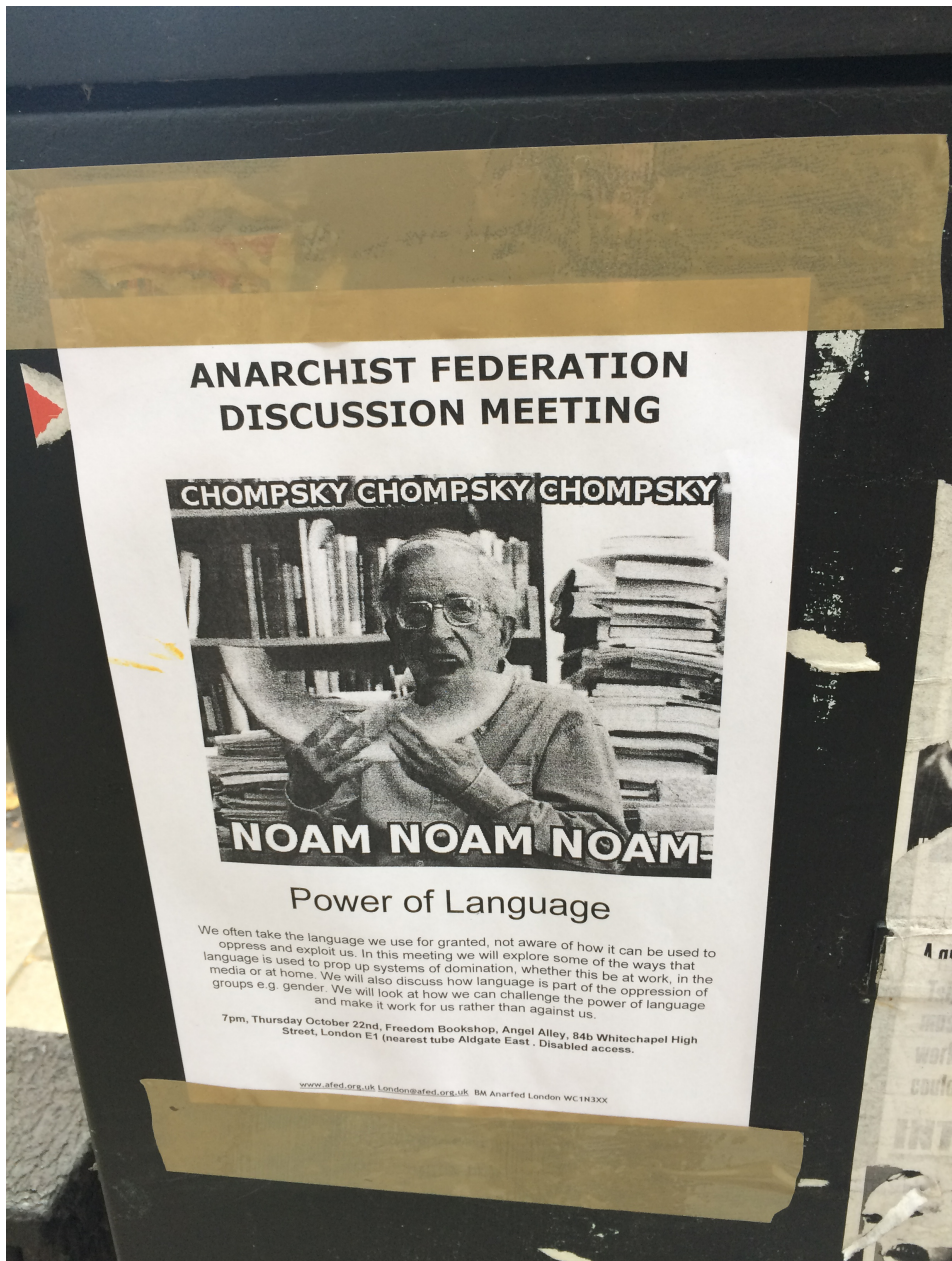


Figure 1: Noam Chomsky sometimes presents his more dangerous ideas using a cover name. This one is probably inspired by the chimpanzee Neam Chimpsky. Anarchist bookstore in London, 2015, picture: Stefan Müller

person's scientific career and harming his integrity. Given the situation the field of linguistics is currently in, this aggressive approach must be seen as a sign of weakness on the part of the attackers.

For somebody who is interested in languages and linguistics, such linguistic wars (Harris 1993) must have a repellent effect: Don't sleep, there are snakes!

2 Everett in Europe

While Pullum's piece reads well, as all of his papers do, it is also depressing. How could this happen? How could an anarchist gain so much power? [For non-linguists: No, the answer is not: "because he or his school of thought have always had better arguments than others." At least not for the past 30 years.¹]

One of the reasons I wrote this contribution is that there is good news: The measurable power of Chomsky drops suddenly after a distance of 5000 km from MIT. It almost reaches the European border but not quite, although it does extend to non-European countries like the UK, as described with respect to Oxford University by Pullum (2024: Section 2.8), Chapter 2 of this volume.

Europe has treated Everett quite differently from what is written in Pullum's paper. Everett has given talks at various Mainstream Generative Grammar (MGG) institutions like the Zentrum Allgemeine Sprachwissenschaft (ZAS) in Berlin. And he was invited to speak at the annual meeting of the Deutsche Gesellschaft für Sprachwissenschaft (DGfS), the analog of the LSA, and dominated by MGG researchers. The invited speaker is chosen by local organizers of the conference and in 2010 it was organized by the ZAS and the Humboldt-Universität zu Berlin.² The same DGfS conference had a workshop on recursion with Tecumseh Fitch, one of the authors of Hauser, Chomsky & Fitch (2002), as an invited speaker. The house was full and I remember lively discussions. Science as it should be.

Everett has been invited to Potsdam, which is also a stronghold of MGG, several times (2014, 2018). I also remember events with Ted Gibson at the ZAS where he discussed Pirahã.

¹Every time the Chomskyan framework has come too close to what other branches of syntax research are doing, Chomsky has changed fundamental assumptions about the architecture of the human language faculty. All derivational models so far have been fundamentally incompatible with psycholinguistic insights. This is really surprising since Mainstream Generative Grammar sees itself as research on one of our cognitive capabilities. So psycholinguistic evidence should be part of the empirical facts on which linguistic theories are built. See Wasow (2021) and Borsley & Müller (2021) for psycholinguistic facts and criticism about the architecture of Minimalism.

See also Lappin et al. (2000a,b, 2001) on Chomskyan "revolutions" in the Minimalist era.

²https://dgfs.de/jahrestagung/berlin_2010/programm_pv.htm, 2024-09-23.

I learned about the movie *Grammar of happiness* from my late colleague Gisbert Fanselow, one of the best German grammarians, who also worked in MGG. He told me that he was watching the movie with his students during the last lecture before Christmas. I then started to do the same. Given the power structure of the field, I normally would not have done this, but since he did this as a serious MGG grammarian, I felt that I was able to do the same. The fact that I initially would not have dared to watch the movie with students reminded me of GDR times, during which an official statement with the wrong content would have ended one's scientific career. But the fact that Gisbert had watched it indicates that the situation in Germany differed from that in the US.

So, the conclusion is: Things do not have to be the way they are in the US. While there are conflicts between the camps, they seem to be more civil and also more fruitful here. There have been joint workshops about Construction Grammar and Minimalism at the Freie Universität Berlin (2007, with Richard Kayne, Adele Goldberg, Gereon Müller, Anatol Stefanowitsch, and others),³ and a workshop on progress in linguistics with researchers from various frameworks present.⁴ There have been framework comparison events in Bergen, Norway (2005, PhD School Languages and Theories in Contrast), and Utrecht, Netherlands (2009, Comparing Frameworks). People talk to each other instead of talking about each other. Or rather in addition to. Starting in the 90s, there was an empirical turn in which researchers did not focus on the intricate suggestions developed by hardcore Minimalists but did more empirically oriented work instead.

3 Language Science Press and festschrifts

When Martin Haspelmath and I founded Language Science Press, we installed the rule that we would not publish festschrifts. The rationale behind this was that nobody in his or her right mind would publish a paper that could be published in *Language* or in the *Journal of Linguistics* in a festschrift. Festschrift papers are usually focused on the person to be honored, they describe how person X was important in the life of the author, how funny, honest, what a person of integrity X is. How helpful X was as a supervisor. Sometimes unpublished material that has been lying in some drawer for decades is recycled. This was the case with the only

³https://www.geisteswissenschaften.fu-berlin.de/izeus/media/program_comparing_languages_workshop.pdf, 2024-09-23.

⁴<https://hpsg.hu-berlin.de/Events/HPSG2013/progling.html>, 2024-09-23.

festschrift article I ever published (Müller 2017).⁵ This rejection of festschrifts is something I learned from the most famous German MGG researcher: Manfred Bierwisch. If I am not mistaken, he never published anything in a festschrift.

When we agreed on the no-festschrift rule, we left an escape hatch open: of course people can do a normal edited volume on a certain topic and give this to somebody as a present. But it should be a normal peer reviewed volume. In general, festschrifts are bad for Language Science Press, since they are expensive. Collections are more expensive than monographs since twenty different authors have twenty different ways of writing strange L^AT_EX code, misciting, doing funny things with figures, or of causing havoc in other unseen ways. Festschrifts are even worse since the authors are usually well-established scholars in the field, which means that all the problems mentioned above become increasingly severe combined with dramatically longer response times. Festschrifts usually come with strict deadlines, which stands in the way of enforcing quality standards. Our usual procedure of community proofreading/editing cannot be applied since the “non-festschrift” has to remain a secret until the day of presentation.

So far, Language Science Press has published six non-festschrifts (Bailey & Sheehan 2017, Bárány et al. 2020, Laszakovits & Shen 2021, McManus & Schmid 2022, Bower et al. 2017, Bonami et al. 2018), some of which were published in my series (Bower et al. 2017, Bonami et al. 2018). In the case of Bonami et al. (2018), the book itself is clean: there is no mention of a festschrift or tribute. But then a review appeared in *Language* by Bauer (2020) mentioning that the volume is a festschrift and who it honored.

The first five volumes of a new series are run through the press directors. After this we trust the series editors of the respective series to continue their good work. Some of the festschrifts appeared this way: I saw a tweet by Susi Wurmbrand saying thank you for her festschrift (Laszakovits & Shen 2021).

In the case of Bower et al. (2017), I missed the statement in the preface that the book is a tribute, but I saw the dedication in the catalog entry. While the book cannot be changed after publication, I changed the catalog entry and informed the editor of the book. This led to quite an email discussion and since the case was lost anyway, we put the dedication back in the catalog. Martin Haspelmath wrote

⁵Now it has a new citation. Including the citation in this paper, there are now seven citations on Google Scholar. All of them are self-citations. The paper is about embedding and recursion, by the way – that you cannot do it in inheritance networks, not even with defaults. So, maybe I am the only one who finds this relevant or the stuff in festschrifts is usually ignored. Either way, this is a further argument against festschrifts.

an email to me back then (p. m. 2017–05–23): “Besser machen bedeutet vielleicht: In Zukunft überhaupt keine Festschrift mehr akzeptieren.”⁶

The current volume was also an interesting case. I explained the no-festschrift policy several times to the editors. When I saw the first outline of the chapters, I remarked that “A journey into Dan Everett’s brain” sounds a bit too festschrift. Geoff Pullum sent me a draft of his paper and told me about the workshop where he would be presenting this paper. I almost fell off my chair when I read the email since the URL of the workshop – probably widely distributed – contained the forbidden keyword “festschrift”.⁷ After all these discussions: Language Science Press does not do festschriften! The workshop was great, and I especially enjoyed the journey into Dan Everett’s brain. This was not sloppy festschrift chitchat but serious science with brain images and so on. Still, it was specially tailored to Dan Everett and perhaps unpublishable in “normal” journals. Thus the planned volume would scream festschrift in every aspect, so that I felt it would be best to call it what it is, a festschrift, and then officially end the seemingly never-ending nightmare of Language Science Press (non-)festschriften.

The team of press directors changed as of 2022–01–01 and Oliver Czulo stepped in for Martin Haspelmath. We discussed allowing festschriften if the editors pay for it. Something like the 10,000€ that is usually charged by profit-oriented publishers.⁸ We abandoned this idea because it would suggest that we publish low-quality work for money. After endless discussions, we finally decided on the 5th of June 2023 to never ever do a festschrift again (starting 2024).

4 Universals

The interesting fact about festschriften is that they create an infinite amount of work. This is somewhat surprising since the number of words and the number of references per chapter are strictly finite. Maybe Friedrich Engels’s insight is correct that a certain increase in quantity may result in a new quality (Engels 1873–1883: 349). This goes against everything mathematicians tell us, but who

⁶Maybe improving things would mean rejecting festschriften, without exception, in the future.”

⁷https://tedlab.mit.edu/everett_festschrift_2023.html, 2023–06–13. Note that the title of the page is *Everett Festschrift 2023* with a typo. I guess this was done on purpose to confuse the Language Science Press search robots which constantly monitor the web to find breaches of the no-festschrift rule.

⁸Brill charges €10,000/\$12,200 for 100,000 words/250 pages (<https://brill.com/page/oacharges>, 2024–09–23). Cambridge charges £10,500 (US\$13,000, €12,000) for 120,000 words and up to 85 figures (<https://www.cambridge.org/core/services/open-access-policies/open-access-books/gold-open-access-books>, 2024–09–23).

knows. On top of the amount of work caused in the editing process alone, we have had endless (!!!) discussions with authors and editors – some even involving Language Science Press’s advisory board – about what it means to publish a non-festschrift with Language Science Press. Since the discussions are endless, the editing process + discussion is definitely endless. This is not just what math tells us but is also supported by our feelings.

One of the weaker arguments for Universal Grammar, innate language-specific knowledge that helps learners in language acquisition, is the claim that there are language universals (Pinker 1994: 237–238, Chomsky 1998: 33). Whether there are such universals and whether they require the assumption of domain-specific innate knowledge to be explained is an ongoing debate (Hawkins 1988, Plank & Filimonova 2000, Evans & Levinson 2009a,b, Müller 2023c: Section 13.1). The response of researchers working in Mainstream Generative Grammar to claims about languages that seem to contradict putative universals is: Yes, but you cannot argue with unanalyzed data (Freidin 2009: 454),⁹ meaning that if you look at an OSV language long enough, you will realize that it is underlyingly SVO.¹⁰

⁹“Data alone cannot speak to the validity of explicit proposals about the content of UG. What is required is an explicit analysis of data that follows from a precisely formulated fragment of a grammar. This is a comment about methodology, independent of any particular linguistic theory. In science there is no alternative to providing explicit analysis of data. The discussion of UG in this article misses the mark entirely.” This statement is false. If a proposal is made that our linguistic machinery allows us to produce an infinite number of sentences and that it follows for all languages that sentences of arbitrary length may be formulated in all languages, then one language that has a maximal sentence length is a counterexample. (See Pullum (2024: 25–26, Section 3.1), Chapter 2 of this volume for a list of putatively finite languages.) If it is stated that Subjacency is a principle that holds for all languages, then it is sufficient to point out that there are examples of extraposition in German that show that this type of non-local dependency cannot cross just two NP boundaries but arbitrarily many (Müller 2004a, 2023c: Section 13.1.5). To make such claims about data, no elaborated formalized grammar is necessary. Some understanding of traditional grammar is sufficient. Sometimes MGG researchers state that examples of a certain kind are predicted by their theory not to be possible. It is then sufficient to find such examples without having a theory about these examples oneself. My dissertation and habilitation are full of such examples (Müller 1999, 2002), as well as of an alternative theory.

¹⁰See Chomsky (1965: 141) and Kayne (1994) for the claim that all languages are underlyingly SVO, McCawley (1970) argued for an underlying VSO order, Bach (1971) and Ross (1973) argued for OV, and Haider (2000, 2010, 2020) claimed that SVO languages are derived from an underlying SOVO pattern.

Note that I am one of those myself. After ten years of working in a more what you see is what you get (WYSIWYG) setting of linearization-based HPSG (Müller 2004b), I developed an analysis of seemingly multiple frontings that assumes SOV to be the underlying order of German clauses (Müller 2005a,b, 2023a). That German is an SOV language is the consensus among linguists working on Germanic. See Müller (2023b) for a discussion of Dryer (2013), whose classification is built on surface occurrences.

Ironically, using “unanalyzed data” is a very common practice among MGG grammarians. Often, they just cherry-pick arbitrary facts from papers describing understudied languages. See Fanselow (2004) for some criticism related to this matter.

As a syntactician one might be inclined to think that grammars should at least require the concept of valence. But if Koenig & Michelson (2012) are right, the Iroquoian language Oneida does not even have syntactic valence. So what we seem to be left with is the triviality that humans combine linguistic material to form larger units (Merge, Hauser et al. 2002, Müller 2015: 52), without any implications about possible sentence length. Note that we seem to require unheaded, flat structures for phenomena like *student after student after student* (Matsuyama 2004, Jackendoff 2008, Bargmann 2015, Müller 2021: Section 4.1). So the universal would be that we combine stuff. Nothing more. Not even a constraint on binary branching.

But note that I have found a different universal. A universal holding on the text level.

- (1) Observation holding at least for (English, French, and German):
Festschrifts cause an infinite amount of work.

I think this is a universal.

- (2) Universal 1 (conjecture):
All festschrifts in all languages cause an infinite amount of work.

Of course more research on this (to be published elsewhere) is needed. But I strongly believe that this conjecture also holds for languages like Pirahã: If we took a festschrift in any language, say English, and translated it into Pirahã, the situation would not improve. Given that the amount of work needed for the English draft is infinite, adding a translation into another language would not make it finite.

Of course there is the question of festschrifts originally written in the language of the final submission. What the result would be for the case of Pirahã is difficult to predict. In order to get an answer here, close collaboration with Dan Everett seems necessary. The first problem is that Pirahã does not have a writing system/culture. I guess the Pirahã people would not see the point in giving somebody a festschrift. But note that my statement is a statement about the cognitive abilities of scientists. Thus: If there were Pirahãian scientists producing festschrifts, these would cause an infinite amount of work. The older and more established Pirahãian scientists would not use reference managers, they would be sloppy and forgetful with respect to sources, and so on.

So, I think, I have found the only true universal: It is a virtual necessity that festschrifts lead to a disaster.

5 Conclusions

Language Science Press therefore invites everybody to publish their festschrifts with the competition and to publish either unedited low-quality stuff or create high costs for our competitors.¹¹ The press managers of Language Science Press have decided that there will not be any further (non-)festschrifts as of 2024. Since this volume is the first official festschrift of Language Science Press, Dan Everett gets the first, last, and only official festschrift published by Language Science Press. A truly outstanding achievement.

Afterthoughts

Of course all papers published up to now in Language Science Press non-festschrifts are exceptionally good. So they cannot be taken as examples of what I said above: papers of low quality, not really about linguistics, with strange argumentations, in a funny festschrift style, and never cited. There is one paper that is an exception: this one. I hope it will never be cited but often read.

Acknowledgements

I thank Sebastian Nordhoff for the final impulse to stop the festschrift madness. Blame him! I also want to thank Sebastian for sending me a list of BIB_{TEX} entries

¹¹De Gruyter is a good candidate, as they do not seem to care about quality. For example, the target article Trinh (2009) in *Theoretical Linguistics* contains 14 occurrences of the phrase *in other word*, which should have been *in other words*. Trinh's reply to replies (2010) contains three occurrences of this phrase. The phrase occurs 31 times in Trinh's MIT thesis (2011), which is based on the discussion paper, and 29 times in the book that was finally published by De Gruyter (Trinh 2019). The book contains an unbelievable amount of further typos. No editor, copy editor, or supervisor seems to have read the papers, the thesis, or the book. The content is also wanting: Trinh argues for a VP analysis of German verbal complexes. An earlier MIT dissertation with the same approach was heavily criticized by Reis & Sternefeld (2004). None of the arguments of Reis & Sternefeld were taken up by Trinh (ignorance of the supervisors, failure of the series editors at De Gruyter). Reis & Sternefeld's criticism and most of the field of Germanic syntax (MGG or alternatives) were completely ignored. See also Reis & Sternefeld (2004: 505) on the phenomenon of MIT theses exclusively citing work from the narrow MIT bubble.

for Language Science Press festschrifts, which he created with ChatGPT. The list was completely useless but fun. See Piantadosi (2024), Chapter 15 of this volume and Müller (2024) for more on ChatGPT. I thank Dan Everett for being the topic of my Christmas movie. I must have watched it at least 20 times by now.

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Chapter 1

Composition, not infinity: The irrelevance of recursion to theories of language

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Hauser et al. (2002) famously proposed recursion to be core to human language. The present paper explains why Hauser et al.'s (2002) written proposal rightly equates recursion with syntactic self-embedding, following Pullum (2020), and the sense in which self-embedding is the mechanism that generates the discrete infinity attested in many languages. This point is important because I then report recent email exchanges with Chomsky in which he clarifies that he thinks Dan Everett intentionally mis-characterized not the linguistic data of Pirahã, but what was intended in Hauser et al. (2002). I argue that Chomsky's accusation here is incorrect. That is, I think it is clear what is meant by what was written in Hauser et al.'s (2002) paper, and Everett and many others discussed those ideas appropriately, following the text written in the paper. Then I move to a discussion of Pirahã itself: Dan Everett proposed that perhaps this language does not have the property of syntactic self-embedding, and therefore provides a counterexample to Hauser et al.'s (2002) universality claims. I will summarize the evidence that I know which seeks to evaluate this question and conclude that there is no strong evidence for recursion in Pirahã syntax. However, I emphasize that this is a difficult question to decide, although the independent claims of other languages without self-embedding make it much more plausibly true of Pirahã. In the final section, I provide an alternative to syntactic self-embedding as being a core component of human grammar: the existence of compositionality in language systems allows compression of what we need to communicate. A small lexicon and grammar gives rise to astronomically large numbers of potential utterances, and whether this set is infinite or not is irrelevant to anything about human nature. I conclude that recursion has been an irrelevant side-track in the theory of language.



1 Introduction: The recursion claim from Hauser et al. (2002)

Hauser et al. (2002) argued that the grammars for human languages are recursive, such that (more important components for my purposes are in italics):

All approaches agree that a core property of FLN (Faculty of Language, Narrow) is recursion, attributed to narrow syntax in the conception just outlined. FLN takes a finite set of elements and yields a potentially infinite array of discrete expressions. This capacity of FLN yields discrete infinity (a property that also characterizes the natural numbers). Each of these discrete expressions is then passed to the sensory-motor and conceptual-intentional systems, which process and elaborate this information in the use of language. Each expression is, in this sense, a pairing of sound and meaning. It has been recognized for thousands of years that language is, fundamentally, a system of sound-meaning connections; the potential infiniteness of this system has been explicitly recognized by Galileo, Descartes, and the 17th-century “philosophical grammarians” and their successors, notably von Humboldt. One goal of the study of FLN and, more broadly, FLB is to discover just how the faculty of language satisfies these basic and essential conditions.

The core property of discrete infinity is intuitively familiar to every language user. Sentences are built up of discrete units: There are 6-word sentences and 7-word sentences, but no 6.5-word sentences. There is no longest sentence (any candidate sentence can be trumped by, for example, embedding it in ‘Mary thinks that ...’), and there is no nonarbitrary upper bound to sentence length. In these respects, language is directly analogous to the natural numbers (see below). (Hauser et al. 2002: 1571)

In fact, as was widely discussed afterwards, there was no definition of “recursion” in Hauser et al. (2002); They simply give an example, and ask the reader to infer what they probably mean. Here I follow Pullum (2020) in assuming that a reasonable interpretation of what Hauser et al. (2002) meant by recursion in a language is *having syntactic devices that could in principle permit the construction of sentences of arbitrary length*. I will refer to this idea as *syntactic self-embedding*. The motivation for this interpretation is given in the following section.

1.1 Elaborating Hauser et al.'s (2002) proposal in context-free grammar terms

Because Chomsky and colleagues worked on phrase structure approaches to natural language syntax, here I elaborate how syntactic self-embedding in the grammar of such a formalism is equivalent to allowing the generation of utterances of unbounded length. A context-free phrase-structure grammar consists of a tuple (T, N, S, R) , such that:

- T is a finite set of terminal symbols: the words that make up the lexicon or dictionary
- N is a finite set of non-terminal symbols
- $S \in N$ is the start symbol
- R is a finite set of rules of the form $X \rightarrow \alpha$, where $X \in N$, and α is a sequence of symbols drawn from T or N

A context-free phrase-structure grammar derivation is the recursive expansion of non-terminal symbols in a string by rules in R , starting with S . A derivation tree is the history of those rule applications (see Chomsky 1959, Lewis & Papadimitriou 1998, Hopcroft et al. 2001).

A simple corpus of a few English sentences is given in (1). A lexicon (a set of terminal symbols) T for this corpus is given in (2). And a rule set R that can generate the corpus in (1) is provided in (3):

- (1) a. The girl saw the dog.
b. Mary thinks that the girl saw the dog.
c. Alison says that Mary thinks that the girl saw the dog.

(2) A Lexicon T :

Name \rightarrow Mary, Alison

$N \rightarrow$ girl, dog

Det \rightarrow the, a

$V_{NP} \rightarrow$ saw, liked

$V_S \rightarrow$ thinks, believes, says

(3) A rule set R:

- | | |
|---------------------------|---|
| 1. $S \rightarrow NP VP$ | 4. $VP \rightarrow V_{NP} NP$ |
| 2. $NP \rightarrow Name$ | 5. $VP \rightarrow V_S \text{ that } S$ |
| 3. $NP \rightarrow Det N$ | |

In phrase-structure terms, a category B is said to be *embedded* within another category A if B combines with some other category to make A: this is simple compositionality within phrase structure. The determiner (Det) *the* can combine with the noun (N) *girl* (according to rule 3) in order to form an NP that generates the string *the girl*. The categories Det and N are then *embedded* within the category NP.

A similar expansion applies to generate the NP *the dog*. In order to form the VP that generates the string *saw the dog*, we combine the V_{NP} *saw* with the NP *the dog* according to rule 5. And finally, the NP *the girl* combines with the VP *saw the dog* to form an S category, according to rule 1, now covering the entire string *the girl saw the dog* in (1a). This set of expansions is represented in the tree structure in Figure 1.

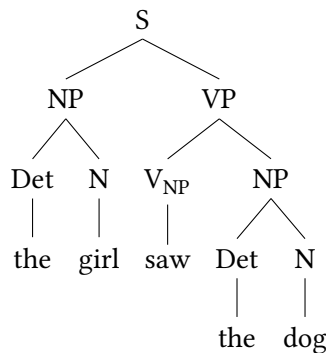


Figure 1: Analysis of *The girl saw the dog*.

If the rules of the grammar are such that the category A can dominate another node of category A, then we have *self-embedding* of A within A. The grammar in (3) has two instances of self-embedding: The category S expands to another S, through rules 1 and 6: an S expands to an NP and a VP in rule 1; and a VP expands to a V_S and then a further S category in rule 5. The two self-embeddings are: S within S, and VP within VP. We can generate arbitrarily long sentences using these rules, as Hauser et al. (2002) indirectly suggest. Parses for self-embedded

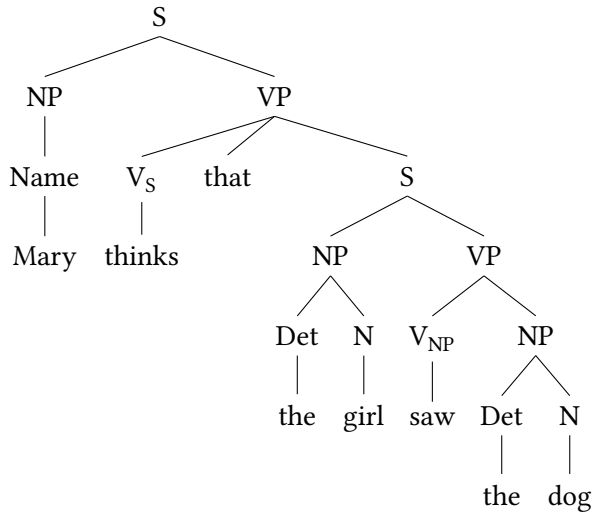


Figure 2: Analysis of *Mary thinks the girl saw the dog.*

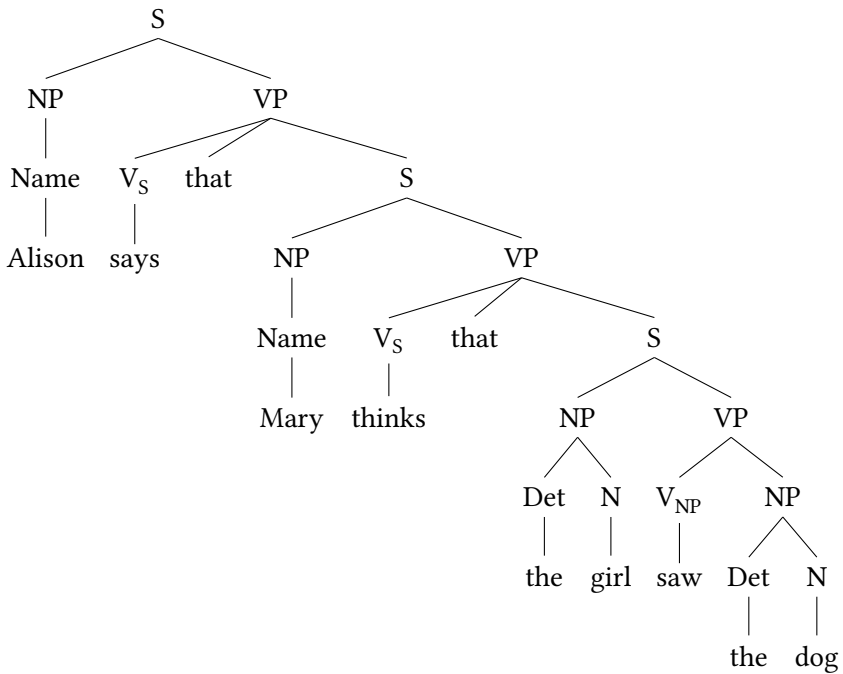


Figure 3: Analysis of *Alison says Mary thinks the girl saw the dog.*

examples like (1b) and (1c) according to the above grammar are provided in Figures 2 and 3.

Within context-free phrase-structure, it is only possible to generate utterances of unbounded length through syntactic self-embedding in the grammar. To see this, first consider a grammar where we have syntactic self-embedding (as in the example above). There is clearly no limit to the number of times that the syntactic self-embedding can apply. In our example, we can apply the category VP to generate an S, which generates a VP as many times as we like. As long as one of VP or S (or both) is associated with the generation of at least one independent nonterminal, then we can generate a sentence of arbitrary length. Let's consider standard human language phrase structure, like X-bar theory (Jackendoff 1977), such that each nonterminal is associated with at least one distinct terminal. That is, a VP in our grammar is always associated with a kind of verb (a V_S or V_{NP}), which expands to exactly one terminal. Thus, there is no bound to the length of an utterance in a grammar with syntactic self-embedding.

Now let's consider the case of a grammar with no syntactic self-embedding. In such a grammar, the strings generated by the grammar *must* be finite, because if you follow any path of nonterminals to their expansion as terminals you can never get to the same nonterminal twice (by hypothesis). There are a finite number of nonterminals, so every path down the tree must be finite. This means that the set of sentences is also finite.

Hence, allowing sentences of unbounded length within a context-free phrase structure grammar is equivalent to having syntactic self-embedding in the context-free representation of natural language. This is why we follow this interpretation of Hauser et al. (2002).

1.2 Everett's claim with respect to Pirahã grammar

Everett (2005) proposed that Pirahã does not have syntactic devices that could in principle permit the construction of sentences of arbitrary length. In particular, Everett claimed that the Pirahã language is *finite*, with sentences of relatively small, finite length (Everett 2012). This means that Pirahã would have no rules like the left- or right-branching rule sets, or center-embedded rule sets. This does not mean that the set of sentences is small, however. Even if each sentence is short in length, there can be an astronomically large number of them, as discussed below in Section 3.¹

¹Everett (2005) also argued that features of Pirahã grammar follow from its culture. While some version of this general hypothesis might be on the right track (see e.g., Gil (2021)), the particular cultural-grammar hypothesis that Everett makes here – the immediacy of experience hypothesis – seems implausible. In any case, I will not discuss the orthogonal question of grammar-

Note that even with no self-embedding in the syntax, we can express recursive meanings, as in (4):

- (4) a. Alison says that Mary thinks that the girl saw the dog.
- b. Alison says the following. Mary thinks the following. The girl saw the dog.

So the English doubly right-branching embedded sentence (4a) can be rephrased as the three separate utterances in (4b). Hence the claim of Hauser et al. (2002) is about the *syntax* of utterances, and the claim of Everett (2005) is about Pirahã syntax, not meaning.

1.3 Some terminological confusion from Nevins et al. (2009)

Over the years there has been some terminological confusion over what Hauser et al. (2002) meant by “recursion”, either (a) self-embedding of a syntactic category, thus allowing for sentences of unbounded length, and a language with an unbounded number of sentences (Everett 2005); or (b) “merge”: effectivity compositionality of any two syntactic elements (Nevins et al. 2009): “In a model with category-neutral Merge, however, a language that lacks recursion would be considerably more exotic. No sentence in such a language could contain more than two words. Pirahã is manifestly not such a language.” (Nevins et al. 2009: 366).

If the “merge” definition results in a grammar with syntactic devices which permit unbounded sentence lengths, then this definition reduces to (a), syntactic self-embedding. On the other hand, if the “merge” definition does not allow syntactic devices which permit unbounded sentence lengths, there is no debate (as Nevins et al. (2009) observe): all human languages have compositionality (hence “merge” in their syntactic terms). In any case, Everett (2005) is assuming definition (a): syntactic self-embedding definition, because that’s what the Hauser et al. (2002) quoted above says. What Hauser et al. (2002) care about is a grammar with devices that allow sentences of unbounded length.

1.4 Chomsky claims that Everett intentionally misinterpreted what was intended in Hauser et al. (2002)

An alternative kind of response to Everett’s claim about Pirahã is to claim that self-embedding may be *available* in all languages, but not all languages use it.

culture connections here. Rather, I stick closely to generative capacity of Pirahã, and whether this language might have a syntactic self-embedding device, but not why or why not.

This is the view of Jackendoff & Wittenberg (2014): they propose that there may be languages whose grammars do not result in self-embedded structures.

In fact, as of 2019, this is Chomsky's claim about what was intended in Hauser et al. (2002), according to an email exchange that I had with him then. The way that this email exchange started was that I wrote to Chomsky asking if he had called Dan Everett "fraudulent", because Dan had been told by students that Chomsky had told them this. Chomsky had written the following in an email to a student (which was shared with me, and which I sent to Chomsky, and which he acknowledged as being his):

The Pirahã story is mostly fraud ... By fraudulently claiming to have refuted assumptions about recursion in language, Everett managed an impressive PR triumph, but nothing beyond that. (Email from Noam Chomsky, September 2019.)

I asked Chomsky to clarify in what sense that Everett was behaving "fraudulently". In the email discussion that followed, it became clear that Chomsky thought that Everett intentionally misinterpreted the Hauser et al. (2002) paper. That is, Chomsky believes that what is stated in the Hauser et al. (2002) paper is that it is irrelevant that there may be exceptions to the generalization that he and his authors were providing: he said that the intended claim was not that recursion is universally *used*. Chomsky said that what was intended was that recursion is universally *available* for human languages. According to Chomsky, Everett is behaving "fraudulently", because Chomsky believes that Everett knew what was intended in Hauser et al. (2002) and the literature that it cites, and Everett was intentionally saying something else, to serve his own purposes which Chomsky terms "a PR triumph".

So yes, Chomsky did say Everett was behaving fraudulently, but in a non-standard use of the word, in my opinion. When I first heard that Chomsky thought that Everett was behaving fraudulently, I assumed that Chomsky thought that the claims about Pirahã were somehow made up, based on falsified data or analyses of some kind, because this is the typical sense of fraud in science. But that was an odd idea with respect to the Pirahã, because the data are mostly non-quantitative, so it's hard to falsify anything in that domain in a technical way. Indeed, this was not Chomsky's sense of fraud here.²

²Chomsky's sense of fraud is particularly odd because it relies on Chomsky knowing what Everett intends, which is obviously impossible. That is, Chomsky would need to be certain that Everett believes that Chomsky meant that recursion is only universally *available* for human languages, not necessarily present in all languages. In discussing this with Everett, it is clear that Everett does not believe this.

Furthermore, while it may be the case that when writing Hauser et al. (2002), Chomsky intended an interpretation whereby recursion is universally *available* for human languages, that's not what Chomsky and his co-authors wrote. In particular, the original writing quoted above suggests that a universal property of language was intended. The line "The core property of discrete infinity is intuitively familiar to every language user" is explicitly about *every language user* not *every English user* or *every Mandarin user*. This is a claim about all speakers of any human language. Furthermore, the line "There is no longest sentence ... and there is no nonarbitrary upper bound to sentence length." is not about any particular language. This is about human language in general. English is given only as an example.

This was Everett's interpretation of Hauser et al. (2002), which I share. This is also every other researcher's interpretation who I have ever talked to. I even have assigned the Hauser et al. (2002) paper to some classes to see what reading the students get, and they always get the same interpretation as Everett, in spite of never having read anything by Everett. So I think it's inappropriate to blame Everett for the "universal" interpretation of Hauser et al. (2002): that interpretation comes from the text itself.

A second response to Everett's claim has been to suggest that Pirahã probably does have syntactic self-embedding, in the same way that English does. Sauerland (2018) suggests that this is the case, based on experimental data that he gathered when visiting the Pirahã. I discuss that in more detail in Section 2.4.

2 Does Pirahã have syntactic devices that could in principle permit the construction of sentences of arbitrary length?

The Pirahã are an indigenous people of the Amazon basin who are hunter-gatherers, with little agriculture. In early 2007, Mike Frank and I traveled with Dan Everett to work with the Pirahã to investigate aspects of Pirahã language and cognition. At that time, there were approximately 800 people in about six Pirahã villages. They had minimal contact or trade with outsiders.

Mike Frank and I (in collaboration with Ev Fedorenko, who didn't make the trip) ran several experiments trying to understand (a) Pirahã number terms and what they meant (Frank et al. 2008), and (b) elementary Pirahã syntax. But investigating a language's syntax is complicated. In collaboration with Eugenie Stapert (a graduate student working with Dan Everett at the time) and Jeanette Sakel (a postdoc working with Everett), we designed some simple methods to try

to get participants to generate self-embedded structures, modeling our attempts on the syntax of English and related languages. In particular, we attempted to get Pirahã participants to generate potential self-embedded possessives and relative clauses, by asking them to describe – in their language – situations that we presented to them.

2.1 Attempts at elicitation of possessive noun phrases

In our attempt to elicit possessive noun phrases along the lines of “Kóxoi’s father’s sibling’s child”, we showed a set of dolls to each of nine Pirahã participants, where the dolls were presented sequentially as in (5). The participants were told that each doll represented a Pirahã person (or animal, as there was also a dog). For example, the first doll in one set of dolls was an adult male named “Kóxoi” (a possible Pirahã name). Each time that a new doll was presented, we told the participant the relationship between the new doll and the previous one, and we asked them to describe the current doll in relation to the initial doll. So, for example, if the first doll was called Kóxoi, the second doll would have a close relationship to Kóxoi, such as his spouse. We asked the participant to tell us who this was, and all participants replied with an order of the name that was given (such as Kóxoi) and the relation, such as “Kóxoi spouse” or “Kóxoi brother” or “brother Kóxoi”. Of 23 trials that we performed, 17 were ordered modifier-head (like “Kóxoi spouse”), and 6 were ordered head-modifier (like “spouse Kóxoi”).

Although these examples could in principle be syntactic self-embedding of a noun phrase within a noun phrase, they could also be the result of a rule that allows only two nouns to combine, and does not allow arbitrary embedding. Hence examples of just two combined nouns does not provide strong evidence of syntactic self-embedding.

- (5) a. This is Kóxoi. (pointing to doll 1, male)
Who is this?
- b. He has a spouse. (pointing to doll 2, female)
Who is this?
- c. She has a parent. (pointing to doll 3, male)
Who is this?
- d. He has a dog. (pointing to doll 4, a dog)
Who is this?

The presentation of the third doll was intended as a test of syntactic self-embedding in Pirahã possessive nominals. We told the participant that this doll

had another close relationship with doll 2, such as the parent of doll 2. We then asked the participants to describe the third doll in relation to the first doll (e.g., Kóxoi). Of the 34 responses at this level, 14 expressed only a single relationship between two of the dolls. Of the remaining 20 responses, only 8 expressed relationships among the three dolls appropriately, but none of these were plausibly syntactic self-embedding in Pirahã. Rather, they were statements like ‘This is Kóxoi, this (his) parents sibling’ or ‘Koxoi’s sibling, he has a spouse’.

The fourth doll also tested the potential existence of syntactic self-embedding in Pirahã possessive nominals. Our nine participants failed to produce any utterances that linked the fourth doll to the first through labels for the second and third, even with multiple clauses. They also did not produce any sequences that could be plausibly analyzed as syntactically embedded connecting three together. The longest sequence that were connected in the same syntactic nominal was two nouns.

This particular study therefore failed to provide evidence that Pirahã speakers could combine sequences of more than two nouns compositionally, in possessive nominals. This does not mean that Pirahã lacks such sequences; but it is suggestive in that direction.

2.2 Attempted elicitation of relative clauses

Our second attempt to elicit syntactic self-embedding in Pirahã consisted of an attempt to elicit relative clauses. To do this, we used two of our dolls that we used to represent Pirahã people. We would show each doll to the participant, and tell them an event that each one took part in, as in (6):

- (6) a. Doll 1: This person paddled a canoe.
b. Doll 2: This person swam in the river.

We would then tell the participant that one of the dolls took part in a third event, as in:

- (7) Doll 1: This person ate some farinha.

We would then take the dolls away, and ask who took part in the last event: *Who ate some farinha?* If the Pirahã language includes relative clauses in its syntax, then it is possible that people might generate materials like *The man who paddled the canoe ate some farinha*. But as discussed briefly above, a language does not need syntactic tools to represent such complex (embedded) meanings. A language can represent such meanings using anaphoric elements (pronouns of

various kinds) to link between separate simple syntactic elements, one for each clause. This general strategy is sometimes called *parataxis*.

Table 1: Number of instances of each type in attempted relative clause elicitation

Single Sentences	Parataxis	Did not follow instructions	Likely relative clause
35	13	22	0

The 70 responses from our 9 participants on this task were distributed as in Table 1. Of the 13 two-clause responses, all were likely parataxis, consisting of pairs of clauses like *canoe paddle* and *same eat farinha*, where the Pirahã word for *same* connects the subjects of the two clauses.

For whatever reasons – bad designs, naiveté, complex or culturally odd meanings – we were unable to get participants to generate anything that might reasonably be analyzed as having self-embedded syntax. We therefore failed to provide evidence that there is self-embedded syntax in Pirahã. But of course we did not provide strong evidence for the lack of self-embedded syntax either.

It is also worth noting that Sakel & Stapert (2010) investigated the use of the suffix *-sai* – which had been proposed earlier by Everett (1986) to be a sentence embedding marker – and they found no evidence of syntactic self-embedding, in either spontaneous elicitation or in a sentence repetition task. Instead, Sakel & Stapert (2010) suggest that *-sai* is a discourse marker, which is not connected syntactically to the clause that it appears adjacent to.

2.3 Futrell, Stearns, Everett, Piantadosi & Gibson (2016)

Futrell et al. (2016) analyzed a corpus of spoken Pirahã that Dan Everett and the previous missionary to the Pirahã – Steve Sheldon – had gathered, to see what the simplest syntactic re-write grammar might be, and whether that grammar had self-embedding. Futrell et al. (2016) parsed 17 Pirahã stories (13 by Sheldon in the 1970s, 4 by Everett in 1980–2009; 1149 sentences; 6830 words), and looked at the resulting structures for evidence of syntactic self-embedding. The full parsed corpus is available at <http://osf.io/kt2e8>.

In this project, Futrell et al. (2016) looked for examples of syntactic self-embedding in a range of kinds of examples that are typically analyzed as syntactic self-embedding in English, including the following:

- Relative clauses: no examples,
- Coordination: Conjunctions/disjunctions: no examples,
- Possessive structures: no examples,
- Nominalizers and complementizers: no examples,
- Reported speech: possible examples; but the examples could also be analyzed as a sequence of non-embedded utterances.

As indicated above, we found no evidence for syntactic self-embedding in the form of relative clauses, coordination structures, possessive structures, nominalizers or complementizers (subordinate clause markers). The one case that might plausibly be analyzed as syntactic self-embedding was *reported speech*, such that the reported content of the speech (what was said) frequently appears immediately following a form of the verb *gá* ('speak'/'say'). In 183 of the 191 instances of this verb in the corpus, the content of the reported speech immediately followed. In the remaining 8 instances, the verb was used clearly intransitively, with no following reported speech. It is possible to analyze this verb as always being intransitive, with the content of the reported speech indicated but not necessarily an argument of the verb, as in: *Mary spoke. Bill will be coming.* The listener can infer that the following clause is what Mary said.

Of course, it is also possible to analyze the reported speech as an argument of the verb *gá* ('speak'), in which case Pirahã would have at least this case of syntactic self-embedding. But since this seems to be the only case where syntactic self-embedding might be motivated, it is also possible to analyze this case without syntactic self-embedding.

So overall, the corpus data do not provide strong evidence for syntactic self-embedding. Of course there are limitations to this work, such as the fact that this is a small corpus. Perhaps obvious instances of syntactic self-embedding are not present in such small corpora. Another limitation of this work is that there is no control corpus from another language whose participants are matched on socio-economic status. That is, it is possible that aspects of education drive the desire/need to talk in a syntactically embedded way. Perhaps we would find few instances of syntactic embedding in a corpus of speech among people who lack general education about other cultures.

2.4 Sauerland (2018)

In contrast to Futrell et al. (2016), Sauerland (2018) purported to have found evidence of syntactic self-embedding in Pirahã, by gathering interpretations of experimental materials. In order to do this, Sauerland went to work with the Pirahã, and had two native speakers record materials of the following form, that he would then play to other Pirahã native speakers:³

- (8) a. Spoken by speaker 1 (Toe):
ce kahápe ogéhiái igeuo
I have.been stars there
'I have been to the stars.'
- b. Spoken by speaker 2:
Toi he gáí-sai ce kahápe ogéhiái igeuo
Toe 3sg say 1sg have.been stars there
i. subordinate interpretation: 'Toe said "I have been to the stars".'
ii. co-ordinate interpretation: 'Toe talked, and I have been to the stars.'

Thus Speaker 1 (named "Toe") said *I have been to the stars*, which is intended to be statement that is obviously impossible. The dependent measure was how the listeners interpreted Speaker 2's statement, which depended on how they would interpret the pronoun "I" (ce) in speaker 2's statement. The pronoun "I" in speaker 2's statement might refer to "Toe", giving a subordinate interpretation. In English, this might correspond to sentences like *Toe said "I have been to the stars"*, or *Toe said that he has been to the stars*. Or the listener might interpret "I" as Speaker 2, to get a coordinate interpretation, corresponding to English materials like *Toe said something and I have been to the stars*. Critically for Sauerland, the coordinate interpretation is false in this scenario (Speaker 2 hasn't been to the stars), while the subordinate interpretation is true (Toe did say that he had been to the stars).

Sauerland hypothesized that in order to get the subordinate interpretation, a language needs syntactic recursion. (This assumption is not actually correct; I return to this problem below.) Thus if people get the subordinate interpretation, then Pirahã must have syntactic recursion. Sauerland investigated this by asking participants if these sentences were true in the context.

³As noted by Everett & Gibson (2019), there are issues with the Pirahã materials: it is not typical Pirahã language, probably because the translator that Sauerland used didn't speak good Pirahã. Everett & Gibson (2019) provide better Pirahã translations of the target materials. In any case, this problem is less of an issue than other problems with Sauerland's design and interpretation, to be discussed here.

Sauerland (2018) provided many of the details of his raw data, so we can visualize his results in Figure 4, where each shaded square represents the answers given by a Pirahã participant in his critical experiment and a control experiment. In this figure, the *y*-axis indicates the number of embedded interpretations out of nine that a particular participant obtained, and the *x*-axis indicates the number of correct interpretations of the nine control materials (to be described below) that the participant got. There are 16 shaded squares: one for each participant, including two who happened to give the same pattern of subordinate and control responses.

We can see in the figure that there are more participants higher up on the *y*-axis, indicating more responses favoring the subordinate interpretation. Sauerland ran a statistical test that suggested that this proportion was greater than chance, and he concluded that Pirahã has recursive syntax.

2.4.1 Flaws in Sauerland’s design and interpretation

There are two major flaws with Sauerland’s reasoning here. First, we need to ensure that the participants understood the task. And second, contrary to Sauerland’s assumption, a subordinate interpretation can come from either an embedded or a non-embedded syntax: there is no necessary connection between the two. I address each of these two issues below in turn.

First, Sauerland doesn’t speak Pirahã. So he needs some control to ensure that the participants understood the task. In order to address this potential issue, Sauerland included a set of nine control sentences, that had correct answers. One such example is given in (9):

- (9) a. Spoken by speaker 1 (Toe):
 ce kahápe kahe’ai igeuo
 I have.been moon there
 ‘I have been to the moon.’
- b. Spoken by speaker 2:
 Toi he gái-sai ce kahápe heesé igeuo
 Toe 3sg say 1sg have.been sun there
 i. co-ordinate interpretation: ‘Toe talked, and I have been to the sun.’
 ii. subordinate interpretation: ‘Toe said ‘I have been to the sun’.

These materials are just like the target materials, except that Speaker 2 now refers to a different location that Speaker 1 (Toe) is purported to have visited. So while Toe talks about the moon in his statement, Speaker 2 refers to the sun as

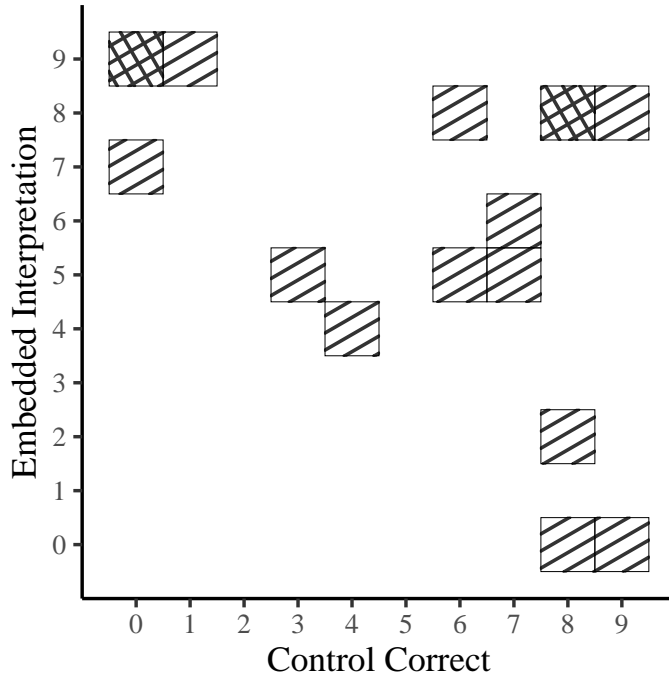


Figure 4: Each shaded square represents the answers given by a Pirahã participant in two sub-experiments conducted by Sauerland (2018). On the y -axis are the number of embedded interpretations out of nine. On the x -axis are the number of correct interpretations of the control materials, also out of nine. There are 16 shaded squares one for each participant, including two who happened to give the same pattern of subordinate and control responses.

For Sauerland's data to be supportive of his hypothesis, participants would need to be in the upper right corner of the graph: most passing the control test, and most showing evidence of an embedded interpretation. Most participants are not in the upper right corner. Even if the data had been in the upper right corner, this would not be evidence of syntactic embedding, as discussed in the text.

Data reported as presented in Sauerland (2018), replotted using R R Core Team (2023), Wickham (2016), Wickham et al. (2022). (Thanks to Moshe Poliak for this picture.)

the place that someone might have visited. Now, Statement 2 is false, no matter whether the participant gets the subordinate or coordinate interpretation. So participants need to have rejected all of these sentences uniformly.

The results from the control experiment are presented on the x -axis: people who understood the task were the ones who got more control questions correct: the rightward people on the x -axis. Note that there are many participants who get fewer than 6 or 7 of the control questions correct. It's unclear what these participants thought was intended by the materials. But whatever they thought, we should not be analyzing their data. Hence, Sauerland should only be analyzing participants' data who understood the controls (e.g., at least 6 of 9 correct): which is only 10 of the 16 participants: the ones on the right. When one analyzes these data, the participants are at chance at interpreting the experimental items in the subordinate reading.

So contrary to what Sauerland says, his Pirahã participants don't actually reliably get the embedded interpretation. The ten participants who understood the task were completely at chance in that interpretation.

The second problematic issue in Sauerland's paper is that he assumes that answering "true" to the target materials necessitates a self-embedded syntactic structure. This is not the case. Alternatively, it could be that the meanings of the materials are biased towards a subordinate *meaning*, whether or not there is self-embedded syntax. We can test this hypothesis in English by giving people similar materials in English, but critically with no syntactic embedding. Everett & Gibson (2019) did this with 20 participants on English translations of Sauerland's materials, as in (10):

(10) John: "I have been to the stars."

Bill: John said something. I have been to the stars.

The question is who does "I" refer to in Bill's sentence: John or Bill? Sauerland thinks that we can only get a referent to "John" through embedding in the syntax, such as "John said that I have been to the stars." But in spite of the lack of syntactic embedding in the materials all participants answered "true" most of the time (98% of trials), getting the embedded meaning interpretation, in spite of no embedded syntax. Hence, people think "I" refers to John almost all the time in (10), even with no embedded syntax

So, in spite of a non-embedded syntax, people get the embedded meaning, contrary to Sauerland's assumption. We don't need recursive syntax to get the embedded meaning interpretation. It appears that Sauerland's materials were biased towards an embedded meaning, independent of the syntax. Indeed, the Pirahã

participants answered with a non-embedded interpretation far more often than English speakers did. It's hard to know why that might have been. My guess is just that the materials and task are confusing for the participants. They generally didn't know what they were supposed to do, and answered semi-randomly across people.

2.5 Pirahã is not even exceptional in its syntactic structure

Overall, my conclusions from Futrell et al. (2016) and Sauerland (2018) are that we have no evidence for self-embedded syntax in Pirahã. This null result does not establish that there is no self-embedded syntax in Pirahã, but (a) several attempts to elicit it have not succeeded, (b) it is not obviously present in naturalistic corpora, and (c) it is not present according to the primary linguist who worked there.

It is worth observing that many other researchers have suggested that languages other than Pirahã might also lack self-embedding in the syntax. Pullum (2024 [this volume]) documents several languages that had been provided as evidence of similar claims by researchers well before Everett, including Iatmul, Gunwingu, Kathlamet, Mohawk, and some Pama-Nyungan languages. And more recently, Gil (2009), Jackendoff & Wittenberg (2014) and Gil (2024 [this volume]) discuss Riau Indonesian, suggesting that this language may lack syntactic self-embedding; and Jackendoff & Wittenberg (2014) discuss how newly-formed sign languages may have similar properties (Goldin-Meadow 2005, Sandler et al. 2005).

Against this backdrop, Hauser et al.'s (2002) claim is somewhat bizarre. How could they have proposed that "recursion" was known to "all language users" if so many languages have been argued not to have it? One possibility is that they were simply unaware of the typological data already reported in the field. But it is interesting to consider how their proposal would have been different if they had engaged this prior literature, and tried to find a universal that was empirically attested in all human languages.

3 The irrelevance of recursion/syntactic self-embedding to theories of grammar

Finally, I return to the main point of this brief paper: Why did Hauser et al. (2002) focus on "recursion" (self-embedding in the syntax) anyway? The claim of Hauser et al. (2002) is that being able to generate an *unbounded* number of sentences is a critical feature of human languages, which gives rise to *discrete infinity*, which they think is a crucial feature of human languages. But why should

generating an unbounded number of sentences be a feature of a human language? This is not a feature that any language user can take advantage of. So clearly “usefulness” is not what Chomsky and colleagues have in mind as a design feature of human language (contrary to e.g., a current claim in language: that language is evolved for efficient use e.g., Gibson et al. 2019).

Furthermore, the existence of potential exceptions to this claim (such as Pirahã) suggests that human languages need not generate an unbounded number of sentences in order to be useful as communication systems. Alternatively, maybe the useful feature of compositional (combinatorial) rules in human language is compression: the fact that having a grammar with generalizations over categories enables us to convey our ideas more efficiently. With categories of forms (words/morphemes), and rules to combine them, we can convey far more meanings than if we associate each complex meaning with an independent form. Compositionality can evolve in a linguistic system that is trying to be concise/learnable while having lots of meanings (Kirby 2000).

Note that even with a finite language, we can convey an unfathomably large number of meanings. For example, suppose that there were 5,000 nouns in the lexicon, among other words. Suppose that there were 100 different syntactic sequences 20 words long, with 10 nouns in each sequence, such that each noun could go in any noun position as in (11). This gives at least $5,000^{10} \times 100 = 10^{39}$ sequences, even ignoring the flexibility of all the other words in the sequences (represented as “ x_i ” for each). (See Müller (2023) for a similar point.)

- (11) 100 sequences of 20 words, each with 10 nouns in a different set of positions across sequences; the sequences shown are arbitrary sequences from the set:

Sequence 1:

$N_1 x_1 N_2 x_2 N_3 x_3 N_4 x_4 N_5 x_5 N_6 x_6 N_7 x_7 N_8 x_8 N_9 x_9 N_{10} x_{10}$

Sequence 2:

$N_1 N_2 x_1 N_3 x_2 N_4 x_3 N_5 x_4 N_6 x_5 N_7 x_6 N_8 x_7 N_9 x_8 N_{10} x_9 x_{10}$

Sequence 3:

$N_1 x_1 N_2 N_3 x_2 N_4 x_3 N_5 x_4 N_6 x_5 N_7 x_6 N_8 x_7 N_9 x_8 N_{10} x_9 x_{10}$

⋮

Sequence 100:

$x_1 x_2 N_1 N_2 N_3 x_3 N_4 N_5 x_4 N_6 x_5 N_7 x_6 N_8 x_7 N_9 x_8 N_{10} x_9 x_{10}$

There are 10^{10} neurons in the brain and 10^{50} atoms in the earth. These are inconceivably large numbers. There is no need to appeal to infinity/unboundedness to explain human language: large finite sets are sufficient to motivate a compositional grammar. Recursion or syntactic self-embedding is irrelevant to this argument.

Finally, the unboundedness of sentence length is an odd property for Chomsky and colleagues to propose as the most critical part of human grammar. “Arbitrarily” long (or deeply nested) sentences are never actually realized, due to performance constraints. How could the critical property of human-like language be something like unboundedness, which isn’t ever seen? It’s a bit like claiming that the defining feature of a car is that it can in principle go any speed, even though we only ever actually see it go two hundred miles per hour. This kind of theorizing is confused and simply can’t be right. The critical aspects of human language will inevitably turn out to be features which are empirically observed in languages, not abstractions about infinity that no human mind actually realizes.

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Chapter 2

Daniel Everett on Pirahã syntax

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Daniel Everett's generalizations about the syntax of the Brazilian indigenous language Pirahã in 2005 provoked not just a linguistic dispute but also an international campaign of vilification and abuse against him. Yet many other languages have been claimed to have the properties he attributes to Pirahã (basically, absence of devices like hypotaxis and clausal coordination). Nevins, Pesetsky, and Rodrigues attempt to represent Everett as having dishonestly concealed earlier evidence of hypotaxis in the language. They are not successful. Later attempts by others to exhibit self-embedding in Pirahã syntax fare even worse. The issue has little general importance for linguistics, since nothing important about language or humanity hangs on whether an upper bound on sentence length exists. In pursuing the matter, Everett's accusers have done him a gross injustice.

1 Everett's dangerous idea

The war on Daniel Everett's reputation and research began soon after the fall of 2005, when he gave a two-part language tutorial session on the Brazilian indigenous language Pirahã at the annual meeting of the Linguistics Association of Great Britain in Cambridge, England (September 1–2), and published an article entitled "Cultural constraints on grammar and cognition in Pirahã" in the August–October issue of *Current Anthropology* (CA). The publisher of CA, the University of Chicago Press, put out a news release about the article which led to some newspaper stories. The surprising result was that in the following years Everett was subjected to bitter attacks impugning not just his work but his integrity and character. The attacks emerged first within the linguistics community, but have come to the attention of a much wider public, particularly among admirers of Noam Chomsky.



The Pirahã are an uncompromisingly independent tribe of indigenous Amazonian people living a subsistence-level low-technology lifestyle on the banks of the Maici river in Amazonas state. They hardly interact with mainstream Brazilian society at all, and show no interest in reading, writing, counting, history, politics, or religion.

Their language appears unrelated to any other now spoken, and they have remained resolutely monolingual in it for at least 200 years, despite occasional contacts with other indigenous people, and acquaintance with three generations of American missionaries, and sporadic and superficial contacts with mainstream Brazilian river traders. A very small number of Pirahã men have a smattering of Portuguese and can act as interlocutors for Pirahã villages that come into occasional contact with Portuguese-speaking Brazilian river traders. Sakel (2012) calls them “gatekeepers”, and provides some interesting data on their very rudimentary Portuguese (she also notes some use of a local pidgin based on the Tupian language Nheengatu). But the women speak only Pirahã, and the gatekeepers basically shelter the vast majority of the Pirahã community (including most of the men) from needing even a minimal competence in Portuguese.

The Pirahã language is linguistically unusual in several ways, from its tiny phonemic system and unusual phonology to its complete absence of numerals and pure color terms. But although Everett’s statements on these points raised some linguists’ eyebrows,¹ they did not provoke anger. What did, and what motivated the surprising events described in Section 2 below, was sentence structure. This might seem an unlikely trigger for angry diatribes and libelous allegations (at least for anyone who did not know the history of generative syntax chronicled in Harris 2021).

It is highly relevant that all production of Pirahã is oral: though an orthography has been devised, no member of the community has shown any interest in learning to read or write. And oral discourse in the language shows no signs of such familiar syntactic phenomena or devices that writers use in constructing long sentences. Everett reports that there are no signs of multiple coordination (*It takes [skill, nerve, initiative, and courage]*), complex determiners (*[[[my] son’s] wife’s] family*), stacked modifiers (*a [nice, [cosy, [inexpensive [little cottage]]]]*), or – most significant of all – reiterable clause embedding (*I thought [you already knew [that she was here]]*). These are the primary constructions that in English permit sentences of any arbitrary finite length to be constructed, yielding the

¹See Dobrin & Schwartz 2021 for an interesting discussion of the ways in which knowledge is based in fieldwork, and how differing assumptions about things like how to devise glosses contributed to the conflict between Everett and his critics on the quantifier issue.

familiar argument that the set of all definable grammatical sentences in English is infinite.²

Linguists versed in syntactic typology were not the ones who expressed shock at the syntactic facts: similar claims had long been made about other languages, sparking no particular controversy. The anthropologist Brent Berlin, commenting on the CA paper (p. 635, one of eight invited responses published with the article) expresses no surprise about the absence of subordination, and quotes a remark by Foley (1986: 177) about the Papuan language Iatmul, where “Linking of clauses is at the same structural level rather than as part within whole.”

The late Kenneth Hale (1934–2001), a long-time MIT faculty member, argued as early as the mid 1970s that the Australian language Warlpiri could not even be said to have phrase structure, which would necessarily entail it did not have syntactically subordinate clauses. Hale’s work, together with that of R. M. W. Dixon, founded a rich subdiscipline of work on Australian languages, particularly the Pama-Nyungan family. The literature is too large for a proper survey here, but suffice it to say that examination of the example sentences presented in works on Pama-Nyungan languages such as Hale (1976), Nash (1980), Dixon (1981), Austin & Bresnan (1996), and Pensalfini (2004), one finds no sign of any embedded complement clauses. Sentences seem to consist solely of word-level constituents, word order often being astonishingly free. There are signs of what might be non-finite secondary predications at main clause margins which could perhaps be called “functionally dependent” but “structurally unembedded” as Austin & Bresnan suggest (1996: 228, esp. n. 13), but there is none of the clause subordination familiar from English and other languages of the sort Benjamin Lee Whorf called “Standard Average European”.

The relevant literature goes far beyond the work on Pama-Nyungan. More than four decades ago the syntactic typology specialist Talmy Givón (1979: 298) wrote in very general terms about languages of “preindustrial, illiterate societies with relatively small, homogeneous social units” in which “subordination does not really exist”. Kalmár (1985: esp. pp. 157–159), citing Givón, elaborates further, giving several earlier references and raising the interesting possibility that Canadian Inuktitut is in the process of developing subordinate clauses for the first time in writing on serious subjects.

Mithun (1984) studies the noticeable avoidance of subordination in highly agglutinative languages employing polysynthesis in their verb structures. She fo-

²The soundness of the argument even for English can be questioned: Pullum & Scholz (2010: 115–124) argue that the claim of an actually infinite number of sentences cannot be sustained. But we can set that theoretical point aside here, concentrating on more concrete matters like whether the language permits embedding of clauses within clauses.

cuses on Gunwinggu (= Kunwinjku, citing 1951 and 1964 sources), Kathlamet (from a 1911 source), and Mohawk (from her own contemporary informant work), and observes that they all resist resorting to subordination, some almost completely. Evans & Levinson (2009: Section 6) take the view that quite generally in Bininj Kun-wok (of which Kunwinjku can be regarded as a dialect variant) there is no clausal embedding, and morphological embedding is possible only to one degree. They also note (p. 442) that Kayardild (another Pama-Nyungan language) allow subordination, “but caps it at one level of nesting”: the subordination cannot be employed to put clauses inside clauses inside clauses and thus make sentences arbitrarily long.

Mithun offers an interesting conjecture about why even one-level subordination is avoided in such languages: in oral-only languages it should perhaps not be seen as implying any shortcoming or lack on their part, but rather an indication that once languages are written, the necessarily slower composition and reception of the written form leads to the development of new syntactic tools “to compensate for the loss of mechanisms inherent in skillful oratory” such as intonational phrasing (p. 509).

Many other instances could be cited of linguists commenting long before 2005 on languages in which arbitrary sentence extensibility seems not to be possible. And not just languages of hunter-gatherer cultures but also languages of early antiquity in Europe and Asia: comments about the lack of true hypotaxis can be found in literature on early Akkadian, Old Chinese, Homeric Greek, and Proto-Uralic.

The late Wayne O’Neil (1931–2020), an MIT faculty member like Hale, published a paper in 1977 arguing that early Old English also showed no signs of clause embedding. Writers would just tack an additional clauses on the end of a main clause, very loosely attached (very much as in Pama-Nyungan). Once Old English speakers were able “to take advantage of the leisure for the composition and decomposition of sentences that being able to read and write afforded them”, O’Neil says, “they took advantage of it in the simplest possible way ... by simply adjoining sentences to sentences, sometimes without even deleting the shared nominal” (O’Neil 1977: 210). The implication is that before Old English was written, subordination was basically absent from the language.

The claims referenced in the last half-dozen paragraphs may or may not be correct in their detailed analytical claims; I am not trying to evaluate them here. My point is merely that they provide descriptions of languages in which it looks as if it would not be possible to construct sentences of arbitrary length, and they have been sitting uncontroversially on library shelves for decades. It is peculiar

that things changed so dramatically in 2005, and that the reaction was so extreme, given that Everett was merely making a point about Pirahã that had been repeatedly made before about other languages.

What had changed? The answer is that a paper co-authored by Marc Hauser, Noam Chomsky, and W. Tecumseh Fitch had been published in the prestigious general scientific journal *Science*: Hauser et al. (2002), henceforth HCF. The paper contains a lot of evolutionary biology and zoology, and it is reasonable to assume that the first-named author did most of the writing. Fitch was an associate in Hauser's lab at Harvard, and Chomsky may have been added more as a co-signatory, without having a role in detailed work on the paper's content (this attributional matter is not irrelevant in the light of the findings of scientific misconduct against Hauser five years later; see footnote 11 below).

HCF included an informally phrased conjecture about what Chomsky calls "Universal Grammar" (UG). The conjecture was that the SOLE aspect of linguistic structure attributable to a biologically rooted "faculty of language in the narrow sense", unique to *Homo sapiens*, is a special cognitive capacity for unbounded combining of mental syntactic representations through repeated applications of a posited binary set-formation operation called "Merge".³ To motivate this idea for a general scientific readership, HCF pointed to a putatively self-evident fact about human language (p. 1571):

The core property of discrete infinity is intuitively familiar to every language user... There is no longest sentence (any candidate sentence can be trumped by, for example, embedding it in "Mary thinks that ..."), and there is no non-arbitrary upper bound to sentence length. In these respects, language is directly analogous to the natural numbers ...

Notice the phrase "every language user", which suggests we are talking about every language of biologically normal human beings anywhere on earth. Note also HCF's claim that the human "faculty of language in the narrow sense" must

³In HCF and a voluminous subsequent literature these matters are discussed in terms of "recursion". I will avoid the use of this term (which HCF nowhere defines) because linguists' use of it is a morass of confusion, as Lobina (2014) correctly points out. In mathematical logic, "recursion" refers to either definition by induction or computational routines that invoke themselves (Soare 1996: esp. 286–289), and "recursive" is used of sets to mean "having a decidable membership problem". Linguists use "recursion" to refer either to self-embedding in phrase structure, or to iterated application of the "Merge" operation, or to HCF's conjectured mental syntactic combinatory capacity, and they use "recursive" as a predicate of rules or grammars. I focus instead on the relatively clear issue of WHAT KINDS OF EXPRESSIONS THE GRAMMAR PERMITS.

“construct an infinite array of internal expressions from the finite resources of the conceptual-intentional system” (p. 1578).

The content of the quotations above is entirely in line with Chomskyan ideas, though it is plausible to assume that Hauser drafted much of the article’s text. The claims in HCF simply restate more emphatically a view that stemmed from Chomsky’s earliest work and had been standard fare in linguistics textbooks for decades. Nearly half a century before, Chomsky (1956: 113) had claimed that the key purpose of a grammar was to project a finite corpus “to an infinite set of grammatical sentences”, and over the next decade this became a part of the usual motivation for generative grammar. Ronald Langacker (1968: 31), for example, was merely elaborating on it when he wrote that “The set of well-formed sentences in English is infinite, and the same is true of every other language”, adding the standard argument that given a sentence of any length you can construct a longer one by embedding it as a *that*-clause. HCF was merely echoing such statements.

Two years before HCF, Lasnik (2000: 3) had put things even more assertively in a syntax textbook, calling the availability of infinitely many sentences a “central” universal of language:

Infinity is one of the most fundamental properties of human languages, maybe the most fundamental one. People debate what the true universals of language are, but indisputably, infinity is central.

And six months before Everett’s *CA* article was published, Sam Epstein and Norbert Hornstein (2005) cited HCF in a letter (intended for publication in *Science* but published in *Language* instead) defending the Chomskyan program and asserting that “human language is a highly structured formal combinatorial system and, in addition, the number of discrete well-formed sentences generated by the system is infinite”. They continued (p. 4):

This property of discrete infinity characterizes EVERY human language; none consists of a finite set of sentences. The unchanged central goal of linguistic theory over the last fifty years has been and remains to give a precise, formal characterization of this property and then to explain how humans develop (or grow) and use discretely infinite linguistic systems. [Emphasis in original – GKP.]

This differs from earlier claims only in being even more strident and explicit.

The trouble for Everett was that by the mid 2000s, endorsing HCF’s view of the biological basis of language had become something of a test of loyalty to the

Chomskyan mainstream conception of syntax. Everett's simple descriptive observation (with its many precedents in unnoticed earlier literature) had become an ideologically dangerous idea.

Some attempts were made to answer it by reinterpreting HCF in a way that could allow Everett's claims to be true without being relevant. The tactic is to neutralize the dangerous idea by asserting that only a vastly weaker hypothesis was ever really at issue. The main attack on Everett in the refereed literature, Nevins et al. (2009b), briefly mentions such a reinterpretation, claiming that under theories of the sort HCF assumed, "what is at stake is in fact the GENERAL ability to build phrases that contain phrases as subparts" and nothing more (pp. 366–67, fn. 11). This retrospectively interprets HCF as saying merely that phrases may contain other phrases. That must involve Merge applying to objects formed by Merge, and that can be called "recursion", vindicating HCF.

There are two problems, though. First, HCF's actual claim about languages was never simply that some phrases can contain certain other phrases (which could be entirely compatible with an upper bound on sentence length). The reference to a literal infinity of sentences quoted above ("There is no longest sentence") is crystal clear. Second, the notion that phrases may contain other phrases is absurdly weak: no one ever doubted it, and no one could think it merited publication in *Science*.

Chomsky has nonetheless essayed a retreat to an even weaker thesis (or at least a less empirically accessible one), which does not say anything about languages at all. He has maintained in various interviews that HCF was merely suggesting that there was a genetically inherited mental capacity of our species that WOULD permit humans to learn languages with arbitrary sentence length, IF they chose to use it. Whether or not speakers of attested languages show signs of using it is, Chomsky now claims, a total irrelevance. Speaking to a 2016 interviewer, Chomsky stated that we can dismiss the evidence of Pirahã syntax because "if some tribe were found in which people wear a patch over one eye and hence do not use binocular vision, it would tell us nothing at all about the human faculty of vision".⁴

Hornstein (2019: 792–794) expounds this view at greater length for anyone who didn't get the memo the first time. He distinguishes "Greenberg universals", to which evidence about languages can be relevant, from "Chomsky universals", which apparently await future advances in neurophysiology for support or refutation. Unfortunately, putting it this way reduces to nothing more than saying

⁴"Chomsky: We are not apes, our language faculty is innate." Interview with Filomena Fuduli Sorrentino, *La Voce di New York*, 4 October 2016, online at <https://lavocedinewyork.com/en/2016/10/04/chomsky-we-are-not-apes-our-language-faculty-is-innate/>

that there must be some special combinatorial ability (HCF’s “faculty of language in the narrow sense”) built into our brains somehow. The view makes no testable predictions except that some sort of linguistic ability will exist in normal humans; but we knew that when we arrived at the lab.

In the interview with Filomena Sorrentino mentioned above, Chomsky makes an additional revealing remark. Sorrentino asked him, “Is there something especially interesting about the Pirahã language?” and he said:

The interesting properties of Pirahã have been studied in depth for many years in a wide range of languages, most prominently by Everett’s mentor, MIT linguist Kenneth Hale, one of the leading figures in the study of indigenous languages, who has produced many important studies of these topics from the 1960s.

There are some straightforward untruths here – Chomsky’s MIT colleague Kenneth Hale, though admired by Everett and everyone else who knew him, never served as “Everett’s mentor,” since Everett’s MA and PhD theses on Pirahã had been completed before the two men met, and Hale never worked on Pirahã at all – but notice that Chomsky seems to be acknowledging the existence of a language with no apparent syntactic embedding. As mentioned above, Hale did point out in the 1970s that Warlpiri lent no support to any theory of hierarchical constituent structure, which would imply the absence of subordinate clause constituents, and at that time Chomsky saw no reason to attack him for it. It was only his pique at seeing HCF contradicted that motivated his going on the offensive against Everett.

Everett (2005) was really just drawing the attention of syntactic theorists to a pre-existing conflict. For decades linguists had been drawing motivation for generative grammars from the proposition that all human languages had infinite numbers of grammatical sentences. Pirahã provides a particularly clear and much publicized case of a language lacking the key syntactic constructions that could support the truth of such claims. For those aggressively committed to the totality of Chomsky’s program, especially those knowing little of the syntactic literature from two or three decades earlier, this message had to be addressed by attacking the messenger.

The public part of the war on Everett began with a long paper about his work first circulated in 2007 and ultimately published by *Language* in 2009. It was written by David Pesetsky of MIT, Andrew Nevins, then at Harvard (now University College London), and Cilene Rodrigues, then at Emmanuel College, Boston (now the Pontifical Catholic University of Rio de Janeiro). I will refer to this trio as NP&R.

NP&R's paper (Nevins et al. 2009b) contains lengthy discussion of a topic about which I will say hardly anything: the extent to which, and the ways in which, culture can influence grammar. Everett holds that a single feature of Pirahã cultural life – their focus on immediate experience rather than remote considerations like the distant past, the far future, or the abstractions of mathematics or philosophy – predicts a whole slew of properties of their language. I doubt it, as do NP&R. But it is not their disagreeing with Everett that I will be concerned with here. In Section 3 I will turn to the rather meager results of their search for false syntactic claims in Everett (2005), but first I review some of the ancillary actions they and others took, and the way they instigated and promoted a remarkably vicious attack on Everett's character and integrity in the years that followed. I will survey the events only briefly in the next section, without attempting to be exhaustive.

2 Character assassination and career disruption

The obvious course of action for linguists who felt Everett's CA paper must be mistaken would have been to engage with him collaboratively to find out more about relevant properties of the Pirahã language. This was not the path chosen by NP&R. Their paper was written without contact with either Everett or anyone else who knew the Pirahã language. This made it wholly an exercise in textual exegesis. And it did not stop at addressing factual claims; from the start it employed thinly veiled inferences and accusations of prejudice, dishonesty, and even research misconduct.

The suggestion NP&R made was in essence that Everett's early descriptive writings on Pirahã did offer evidence of subordinate clauses (along with various other things like numerals, quantifiers, and color names), so his 2005 position was a suspiciously unsupported and possibly mendacious retraction of earlier views.

Despite mentioning the idea that HCF had only ever intended a weak claim about phrases containing other phrases (pp. 366–67, fn. 11), NP&R only made that point in passing; their central aim was to argue that in 2005 Everett was telling lies about CLAUSAL embedding, and that one could learn this by simply looking at his work of a quarter-century before, where he did tell the truth. In the refereed paper they published in *Language* (2009b) they could only adumbrate the claim of dishonesty, but in less constrained channels they and others were less guarded: emails, tweets, blogs, remarks to journalists, and posts on Facebook can slip the surly bonds of scholarly decency.

The attack mounted by NP&R, and taken up by other anti-Everett linguists, was not the worst that a social scientist ever suffered; the libeling of anthropologist Napoleon Chagnon and geneticist James Neel by Patrick Tierney (2000) was surely worse.⁵ But the trashing of Daniel Everett runs a fair second for nastiness.

Tom Bartlett of *The Chronicle of Higher Education* heard about it from linguists that he interviewed in 2012. His account of linguists' behavior (Bartlett 2012) is not edifying, but fully accords with my knowledge and experience of the events. He speaks of a linguistics discipline "populated by a deeply factionalized group of scholars who can't agree on what they're arguing about and who tend to dismiss their opponents as morons or frauds or both". Other disciplines have disputes too, he admits, but even so, "linguists seem uncommonly hostile". If anything, Bartlett somewhat understated things; the following subsections refer to documentable incidents that he did not even mention.

2.1 The BCS lecture

In the fall of 2006 Professor Edward Gibson arranged for Daniel Everett to give a lecture on Pirahã syntax in the Brain and Cognitive Sciences department (BCS) at MIT. David Pesetsky, of MIT's Department of Linguistics and Philosophy, contacted Gibson by email. Details of the interaction are disputed,⁶ but Gibson reports Pesetsky as apparently thinking that Everett held reprehensible views about the Pirahã people, mentioning a claim that the Pirahã talk like chickens and act like monkeys. Gibson knew the latter remark. It was from a page headed "Pirahã: The People" on the University of Pittsburgh website,⁷ and reported a contemptuous remark by Brazilian merchants who traveled the Maici river and occasionally traded with men from Pirahã villages. Everett wrote: "The local traders say they 'talk like chickens and act like monkeys'". He was quoting, not endorsing the characterization; he despised the ignorance of the people who repeated

⁵Tierney falsely alleged that Chagnon and Neel had deliberately exacerbated a fatal measles epidemic among the Yanomamö people in pursuit of some kind of eugenics experiment. For a time anthropologists Leslie Sponsel and Terence Turner persuaded the American Anthropological Association to support these charges and condemn Chagnon and Neel. See Dreger (2011) for detailed research on the whole sordid story of this affair, and a vindication of Chagnon and Neel. Tierney is now regarded as totally discredited.

⁶Pesetsky asked Gibson to assure him that he was not forwarding the email exchange to anyone else, and Gibson gave that assurance. Gibson has since honored Pesetsky's wish to keep his emails private. When I asked Pesetsky to show me the emails, he refused, so I have only Gibson's broad paraphrase of them as my source.

⁷In 2007 it was still accessible at <http://amazonling.linguist.pitt.edu/people.html> but it did not survive Everett's subsequent moves to other universities and seems not to have been preserved by the Wayback Machine archiving site.

the saying. Gibson pointed out that an unendorsed direct quotation entailed nothing about Everett's views, but when the first draft of NP&R's paper was circulated about three months later,⁸ it contained a statement that the authors felt a "general discomfort with the overall presentation of Pirahã language and culture" that Everett gave, and in a footnote (p. 51, fn. 74) it repeated the quote from the river traders.

The extent of NP&R's hostility to Everett's views and suspicion about his relations with indigenous Brazilians became much more explicit on Tuesday 28 November 2006, when Gibson sent out a formal announcement of Everett's lecture to the mailing lists for linguists and BCS people at MIT and Harvard. Immediately Andrew Nevins (who had never met Everett, and refused when Gibson later suggested a meeting) sent out a scathing email from his Harvard account to the same lists about the expected content of the talk.⁹ The subject line was "enough is enough" and it opened by saying:

although david, cilene and i are working on a paper about the linguistic features of piraha, i thought some of you should see some of the more obvious counterexamples to everett's cultural claims before his talk at mit on friday, especially since we may not be allowed to ask questions without being cut off.

He then gave a link to Everett's "Pirahã: the people" and said: "have a look at this archived web page from just over 6 years ago. Did the Piraha change since then, or did Everett?" – an indication that NP&R were going to try to show that Everett was not just wrong, he was lying about facts he had previously acknowledged. After giving a few links to Brazilian anthropological literature, Nevins ended with a sarcastic parody of advertising copy:

You, too, can enjoy the spotlight of mass media and closet exoticists! Just find a remote tribe and exploit them for your own fame by making claims nobody will bother to check!

This struck me as like an intrusion into linguistic science of the sort of attack ads typically seen in political election campaigns. I commented on it in a dis-

⁸LingBuzz, 8 March 2007, https://ling.auf.net/lingbuzz/000411/v1.pdf?_s=AES_1bvQN0ZRFPhy

⁹At the time I had a Radcliffe Institute email address that David Pesetsky had kindly added to the MIT visitors' email list to keep me informed about colloquia during a sabbatical at Harvard, so I was an accidental recipient of Nevins's email. He had tried to reach the MIT Brain and Cognitive Sciences list as well as the lists for the two linguistics departments, but found it closed to external senders.

cussion of the issue on Language Log the next day,¹⁰ speculating on whether the attack might be motivated by a combination of Chomskyan orthodoxy, liberal hypersensitivity regarding ethnic minorities, and academic prejudice against missionaries.

The talk attracted a large audience. Nevins, Pesetsky, and Rodrigues all attended, and so did Marc Hauser, the lead author of HCF. Hauser was well acquainted with Nevins, who regularly attended Hauser's lab meetings at the time. Ironically, seven months after Nevins's email about "claims nobody will bother to check", Harvard investigators began to check some of Hauser's claims about primate behavior, and within four years he had been found responsible for serious research misconduct and had lost his professorship and quit academia.¹¹

2.2 Refusal of research permits

In 2007, Everett received an unexpected phone call from the distinguished journalist Larry Rohter, who had been South American bureau chief for *The New York Times* since 1999. Rohter was in the office of the director (*presidente*) of FUNAI (Fundação Nacional do Índio, later renamed Fundação Nacional dos Povos Indígenas), the Brazilian government agency charged with overseeing the welfare and protection of the country's indigenous people. He had in his hands a letter written to FUNAI by Cilene Rodrigues. Rohter read the Portuguese text to Everett over the phone.

The letter expressed objections to Everett's linguistic research and his representation of Pirahã culture. It may also have expressed the view that he was not a suitable person to be permitted to work with Brazilian Indians. I have not seen the letter, and Rodrigues did not respond when I asked her for a copy of it, but Rodrigues's role in the interaction with FUNAI is confirmed in an article in *The*

¹⁰'Fear and loathing on Massachusetts Avenue,' on Language Log, 29 November 2006, online at <http://itre.cis.upenn.edu/~myl/languagelog/archives/003837.html>

¹¹In July 2007 investigators entered Hauser's lab while he was away, seizing computers, video records, and documents. By August 2010 they had found him solely responsible for "eight instances of scientific misconduct", including "problems involving data acquisition, data analysis, data retention, and the reporting of research methodologies and results". After a year's leave of absence, Hauser learned that he would not be allowed to return to teaching at Harvard, or maintain a laboratory, or apply for grants. He resigned effective 1 August 2011. Later a separate investigation by the federal government's Office of Research Integrity found in September 2012 that he had fabricated data, manipulated results, and wrongly described experiments supported by several federal grants (see DHSS notice 77 FR 54917, 09/06/2012). Gross (2011) provides a detailed discussion of the Harvard investigation and its aftermath.

New York Times,¹² which reports that “She declined to elaborate on the contents of the letter, which she said was written at Funai’s request and did not recommend any particular course of action”, and that “asked about her overall opinion of Dr. Everett’s research, she said, ‘It does not meet the standards of scientific evidence in our field.’”.

A few years earlier, Napoleon Chagnon’s enemies had managed to persuade FUNAI to deny him permission to visit the Yanomamö people in Brazil (see Dreger 2011). Something similar now appeared to happen to Everett. The next time he applied for permission to bring some researchers to the Pirahã territory (which, ironically, he had originally assisted FUNAI in demarcating in order to protect the Pirahãs’ right to their land), he found that he was denied. He was later able to get permission from the local FUNAI office to visit the area merely as an aide and interpreter to a film team during the making of the 2012 documentary film *The Grammar of Happiness*,¹³ but his applications to do grant-supported field research on the language met with negative decisions.

Everett flew to Brasília to discuss the situation, accompanied by the doyen of Amazonian research, the late Aryon Rodrigues (1925–2014), who had been a mentor to him during his doctoral studies. They had set up a meeting with the national director of FUNAI, Márcio Meira, but Meira did not show up. Instead he sent a deputy had no power to make executive decisions. Everett was thus cut off from visiting the people he had known intimately for more than thirty years.¹⁴ Among other things, this was a material loss for the Pirahã, because every time Everett arrived in their village he would bring medicines and other valued items.

2.3 Chomsky’s “charlatan” insult

In early 2009 Noam Chomsky was interviewed about the dispute by *Folha de S. Paulo*, the the largest-circulation newspaper in Brazil, and with evident irritation he told the interviewer (see the issue of 1 February 2009):

Ele virou um charlatão puro, embora costumava ser um bom linguista descritivo. É por isso que, até onde eu sei, todos os linguistas sérios que trabalham com linguas brasileiras ignoram-no.

¹²“How Do You Say ‘Disagreement’ in Pirahã?” by Jennifer Schuessler, *The New York Times*, 21 March 2012.

¹³On YouTube at <https://www.youtube.com/watch?v=5NyB4fIZHeU> and also via SLICE at https://www.youtube.com/watch?v=_LAR6eeiVtY

¹⁴Everett lived in Pirahã villages for 10 days in 1977; 3 weeks in 1978; 6 weeks in 1979; 8 months in 1980; 4 months each year from 1981 to 1985; a total of 12 months during 1986–1988; a total of 36 months during 1989–1999; 20 months during 1999–2001; and three months during 2001–2009, a total of just over 100 months.

[“He became a pure charlatan, although he used to be a good descriptive linguist. That is why, as far as I know, all the serious linguists who work on Brazilian languages ignore him.”]

The petty abuse of the first sentence is followed by a piece of dishonesty: since Chomsky has never worked on Brazilian indigenous languages and has never discussed any detailed work by those who have, he has no knowledge of the wider community of Amazonianists (many of them missionaries, others secular linguists or anthropologists in a variety of universities in Europe, Australia, and the Americas), and therefore has no grounds for assessing Everett’s standing among Amazonianists. The truth is that Everett’s expertise has never been questioned by the linguists with whom he has worked, or by any of the roughly twenty researchers who have spent time with him among the Pirahã to do research, or by any of the few outsiders who (like Steven Sheldon) have actually made progress on learning the Pirahã language.¹⁵

Chomsky continued with a clearly unverifiable claim about Everett’s private thoughts and hopes:

Everett espera que os leitores não entendam a deferença entre a GU no sentido técnico (a teoria do componente genético da linguagem humana) e no sentido informal, que dis respeito às propriedades comuns a todas as línguas.

[“Everett hopes that the readers do not understand the difference between UG in the technical sense (the theory of the genetic component of human language) and the informal sense, which concerns properties common to all languages.”]

Chomsky is alluding to his reinterpretation of HCL’s “recursion” claims as having never been about languages, but only about the genetically transmitted human ability to acquire language. He is claiming that Everett wanted to fool *CA* readers into paying attention to sentence structure when really he knew the focus should have been on genetics and neurophysiology.

But HCF never provided any genetic or neurophysiological facts about the human language capacity that Everett could have focused on. As Everett noted in a response to NP&R, if the “genetic component” is the issue on the table, then Chomsky’s claim seems virtually empty: humans simply have whatever special

¹⁵Chomsky had perhaps forgotten that Everett had mentioned the lack of syntactic embedding in Pirahã during a personal conversation with him at MIT 25 years earlier; see Everett (2007: 12, fn. 7). I return to this briefly in Section 3 below.

thing it is that permits them to acquire and use language (see Everett 2009: 439). Since he was motivated by what HCL actually said (“There is no longest sentence”, etc.), he concentrated on “properties common to all languages”. That isn’t charlatanry.

2.4 Rodrigues’s overt accusation of racism

Later in 2009, Rodrigues increased the rhetorical temperature some more. She explicitly alleged in a magazine interview with the German journalist Malte Henk that Everett held racist beliefs: “Everett ist ein Rassist. Er stellt die Pirahã auf eine Stufe mit Primaten” [“Everett is a racist. He puts the Pirahã on a level with primates”].¹⁶ By “primates” she clearly means apes and monkeys, unless she has forgotten that all humans are primates.¹⁷

As Bartlett (2012) remarks, “When you read Everett’s two books about the Pirahã, it is nearly impossible to think that he believes they are inferior. In fact, he goes to great lengths not to condescend.” He does indeed. He stresses their sharp intelligence, ingenuity, strong group identity, rich social life, and ability to grasp complex discourse. He lived with them, hunted with them, raised his three children among them, talked with them endlessly, and learned from them during periods of residence totaling well over eight years. His many accounts of interaction with them (most engagingly in Everett 2008) often evince admiration, and never for a moment suggest he sees them as racially inferior beings.

But accusations of racism are potent weapons in contemporary intellectual and political debate, whether grounded or not – more powerful than any points about syntactic analysis could be.

2.5 The fraud libels

While working on his 2012 article, Tom Bartlett asked Nevins for some comments on the war on Everett. Nevins refused to be interviewed, but emailed back: “it seems you’ve already analyzed this kind of case!” – appending a link to an earlier Bartlett story about Diederik Stapel.

The implied defamatory claim here is extreme. Stapel is famously an admitted fraudster. He voluntarily returned his PhD certificate to the University of Amsterdam because he acknowledged that his scientific misconduct had been “inconsistent with the duties associated with the doctorate”. So far 58 of his papers in

¹⁶*GEO* magazine (Gruner + Jahr, Hamburg, Germany), January 2010, p. 59.

¹⁷In an email to Everett, Rodrigues denied ever making the statement, but Malte Henk stands by his claim about what she said to him on the record; see Everett (2013: 13).

social psychology have been retracted on grounds that the data were either manipulated or – in at least 30 cases – simply invented out of thin air. Stapel would invent whole tables of data with no empirical basis at all, and published many reports of experimental studies that were never conducted. Nevins is equating Everett’s eight years of immersive fieldwork and data analysis with the proven scientific misconduct of a man described in *The New York Times* (26 April 2013) as “the biggest con man in academic science”.

At the time Nevins sent his message to Bartlett, Everett was a dean at Bentley University and happened to be chairing an investigation into allegations against a professor of accounting: Professor James E. Hunton, who ultimately resigned in December 2012. By 2016 at least 37 of Hunton’s papers had been retracted under suspicions of wholesale invention of data and publishing reports of studies that had never been conducted.¹⁸ Bentley, therefore, had a well-functioning procedure for dealing with research misconduct, which could have been used against Everett if anyone had come up with a scintilla of evidence about fraud or other research misconduct.

Tom Roeper of the University of Massachusetts, Amherst, also directly and publicly accused Everett of fraud. Speaking about Everett on camera to the makers of *The Grammar of Happiness*, he said: “I think he knows he’s wrong, that’s what I really think.” With a knowing smile, he added: “I think it’s a move that many, many intellectuals make to get a little bit of attention.”¹⁹ Roeper’s claim is not just that Everett is wrong, but that he KNOWS he’s wrong, and is telling lies “to get a little bit of attention”.

2.6 Illegality accusations

In Brazil, the allegations started to reach further than simply positing dishonesty. Rumors were spread that for decades Everett had been working illegally, never obtaining the required permits for working in Indian areas. Denny Moore, an American linguist resident in Brazil, made forceful allegations along these lines to me in personal conversation and subsequent email (May 2019) and made further remarks on the topic in a Facebook comment in January 2024.

The suggestion that Everett had never complied with the full legal requirements is implausible on its face, because if it were true then his failure to obtain

¹⁸See *Retraction Watch*, <https://retractionwatch.com/2016/05/12/former-accounting-prof-adds-4-more-retractions-total-exceeds-37/>

¹⁹For a bookmarked location of Roeper’s remark in the SLICE release of the film, retitled as “Decoding Amazon: life of the Pirahã”, go to https://youtu.be/_LAR6eeiVtY?t=1323

a FUNAI permit after Rodrigues's letter of 2007 would have been of no importance. Everett arrived in Brazil in 1977 and was granted permanent resident status under an agreement between the Brazilian government and the Summer Institute of Linguistics (SIL), so he can visit the country without a visa whenever he wishes. But doing research on the Pirahã reservation without a FUNAI permit would be illegal. The only reason Everett has not been able to do any field research among the Pirahã since 2009 is that he strictly respects the law – as one would expect, given the crucial necessity for him to have access to indigenous Amazonian areas.

In 1977 all SIL missionaries were allowed to live among indigenous populations (Desmond Derbyshire had been with the Hixkaryana under such terms since 1955 when I met him). In 1978 the government canceled the contract with SIL and all missionaries had to leave indigenous lands. At that point Everett became a graduate student at the State University of Campinas (UNICAMP), and in that capacity, with the help of Aryon Rodrigues, he received written authorization from the director of FUNAI to return to the area, and spent a year living in a Pirahã village with his American wife Keren (now Keren Madora) and three children – not a visit that could have been accomplished furtively.

Eventually FUNAI reached an understanding with SIL that allowed all of its members to continue working in indigenous villages, not as missionaries but in order to do linguistic research and translate morally uplifting works into indigenous languages. That blanket permission for SIL members covered Everett after he completed the PhD at UNICAMP, until 2001. During that period he never needed to fill out the permit application forms used by university academics, whether Brazilian or foreign, which is why (as suspicious Brazilian researchers have found) searches in the public record for his applications via that channel come up with no results.

In 2001 Everett left SIL. Since then, when doing grant-supported research as a faculty member at the University of Pittsburgh (1988–1999) or the University of Manchester (2001–2006), he has entered the country on the basis of his permanent resident status (contrary to some allegations, he has never entered Brazil on a tourist visa), and he obtained permission for visits to indigenous areas through close contacts with FUNAI.

There are different ways for permanent residents to work: they can apply to the national office of FUNAI, or go through a local FUNAI office in the appropriate region provided Brasília does not object. They can also visit at the request of an indigenous group, which FUNAI is required to accept. One way or another, Everett has always had the needed permits, and two national-level directors of FUNAI (including the much-respected Apoena Meirelles) visited Everett while

he lived with the Pirahã, which would hardly have happened if he was an illegal foreign interloper. He has a letter from FUNAI thanking him for his work, and a short article praising his work appeared in a magazine in 2012²⁰ and was archived on the FUNAI website.

There was an occasion in 2007 when Everett was with the Pirahã along with several students and a local FUNAI official with a grudge against him reported that they were there illegally. A heavily armed team of military police made the long river journey through a rainstorm to get to the relevant Pirahã village and arrest him. Everett greeted them in fluent Portuguese, showed them his permanent residence document and his letter from the local FUNAI office. The policemen relaxed, and posed smiling for a photo with members of Everett's team. A few days later in Porto Velho, he was called in by the FUNAI office there over the same incident, and again satisfied the organization that he had done everything legally.

Everett is not and never has been the subject of any civil suit or criminal indictment for illegal presence in an indigenous area. Yet allegations that he is a notorious lawbreaker continue to be spread by linguists in Brazil. The strong antipathy felt by many Brazilian academics to North American missionaries may be partly to blame, since Everett is still thought of as associated with that role, more than two decades after he left SIL.

2.7 The Nevins/Carvalho/Rössler video

A conference was held at the Federal University of Rio de Janeiro in 2013 that was devoted entirely to work arguing that Everett was wrong. Everett heard about the planning for it, and offered to attend the conference at his own expense, but he was told he would not be welcome. During the same period (August 2013) Nevins took the opportunity to work with Emerson Carvalho and Eva-Maria Rössler to produce a video²¹ which seems to have the primary purpose of further damaging Everett's reputation. It is represented as an interview with two representatives of "the leadership" of the Pirahã (in truth they live an anarchist socio-political life with no political leaders). The main speaker throughout the video is Jose Augusto Diarroi, nicknamed "Verão" by Portuguese speakers because of his SIL contacts (*verão* means "summer"), who falsely represents himself as member of the Pirahã community. His father was Pirahã, but his mother was not, and he was raised elsewhere, never acquiring more than a smattering of the Pirahã language. Sitting beside him is a native Pirahã speaker whose name is given as Yapohen

²⁰Marcelo Moraes Caetano, "Indagado pelos Pirahã", *Revista da Cultura* 61, August 2012, p. 33.

²¹Online since 2013 at <https://www.youtube.com/watch?v=J3jW14cPRMg>

(not a possible Pirahã name) but is actually Hiahoái. Very few Pirahã utterances are heard in the entire interview, and none are glossed in the subtitles.

Augusto tells tales about Everett engaging in activities seemingly drawn from the worst stereotypical charges against bad missionaries, claiming that Everett had terrorized the people he lived among, threatening them that God would kill them all if they did not come to Jesus and convert to being “true believers”, and so on. Nevins’s voice can be heard saying things like “Wow!” from time to time. If any of what he says were true, Augusto would not be one to tell about it, because he never lived in a Pirahã village during any time when Everett was there.

At certain points Augusto attempts to elicit some contributions from Hiahoái, who is visibly reluctant to speak, and says nothing for a long time. When he is eventually prompted to say a few things in Pirahã, Augusto pretends quite unconvincingly to translate them, turning a few seconds of Pirahã into several minutes of Portuguese. What he represents as translations are total fabrications. A version of the video with transcription supertitles of the Pirahã utterances was uploaded by Miguel Salinas in 2019.²² See Everett & Gibson (2019: 781, fn. 3) for brief discussion of some of this video, with examples of the mistranslations.

2.8 Cancellation at Oxford

The work that NP&R have put into representing Everett as a disreputable person and untrustworthy scholar has not had significant material effects on his career: he has served successfully as a department head, dean of arts and sciences, and acting provost, and unlike Hauser or Hunton he remains a tenured full professor to this day. Nevertheless, NP&R have created a kind of folklore, a vague shadow of disrepute, which continues to have effects. Mud sticks, if you throw enough of it. One of Everett’s daughters reports having met people in Brazil who say, “Oh, you’re the daughter of that racist guy.”²³ And substantive professional consequences do result from this atmosphere of negativity.

For example, on 12 March 2017 Everett offered to give a talk to the linguists at the University of Oxford the following September – at no cost to Oxford because he was planning to visit the UK anyway. The planned lecture was not to have been about Pirahã syntax, incidentally, but about paleoanthropology and the emergence of language in early humans. His offer was greeted with enthusiasm by the head of the linguistics faculty, Professor Aditi Lahiri, who promptly let her colleagues know the good news. But within hours her acceptance was withdrawn in a rather awkward email message.

²²Online at <https://www.youtube.com/watch?v=xEeAufXg8fc>

²³Interview with Liz Else and Lucy Middleton, *New Scientist*, 19 January 2008, p. 44.

The next day Everett learned the reason: two junior faculty had objected by email as soon as they learned of the tentative plan, citing potential “reputational damage” to Oxford if Everett were to speak there.²⁴ It is hard to believe someone would think a visiting speaker could be so toxic that his mere appearance would inflict reputational damage on Britain’s oldest university, often ranked number one in the world. But this is the sort of strange fruit the long campaign against Everett has borne.

2.9 The double review of *Recursion Across Domains*

The conference in Rio de Janeiro in 2013 resulted in a book entitled *Recursion Across Domains* (Amaral et al. 2018). The central aim of the conference and the book was to publish studies saying Everett was wrong, and he was never invited to submit a reply to its criticisms. But the editors of the Linguistic Society of America’s journal *Language* invited Everett together with his collaborator Edward Gibson to write a review of the book (it appeared as Everett & Gibson 2019). When this became known to Everett’s opponents, the editors promptly came under pressure to alter their decision. After some consultation they made the unprecedented decision to give the book two review articles in the same issue. Several potential reviewers who were thought likely to take a more anti-Everett and pro-Chomsky line were sounded out but declined. Finally Norbert Hornstein agreed to take on the task.

Hornstein (2019) admitted with admirable frankness (p. 791) that he knows nothing at all about the empirical content of the book – topics like the syntax of South American languages and experimental developmental psycholinguistics. In fact he says: “Facts usually make me itchy... My allergies will lead me to pass lightly over many of the specific empirical findings in what follows.”. His main qualification was clearly that he could be relied upon to support the Chomskyan line, and that he did. (See Section 5 below for a discussion of one chapter from the book that Hornstein naively accepted as sound.)

Further pressure on the editors of *Language* induced them to do one additional thing regarding the same book that as far as I can see was unprecedented: *Language* (like most scholarly journals) does not publish aggrieved responses to book reviews submitted by authors whose work is criticized. But Cilene Rodrigues sent in a letter of protest about the Everett and Gibson review, which had said that her work did not exhibit “high scientific standards”. The editor (Andries Coetzee) initially resisted the idea of publishing it (and told Everett and Gibson that it would

²⁴This was reported to Everett by the late Yorick Wilks (1939–2023) in an email, 13 March 2017, which I have seen. Wilks stated that he had seen the objectors’ emails but did not name them.

not be published without their having right of reply), but he was eventually persuaded to print it, and it appeared in *Language* 96.2 (2020), 221–223, without a reply (Rodrigues 2020). A short editorial clarification concerning one sentence in the Everett and Gibson review was also printed. Thus *Recursion Across Domains* ended up being the subject of four different items in the pages of *Language* when the usual maximum for any book is one.

2.10 Recent literature overviews

The work NP&R have done to damage Everett’s reputation has been ample to color the general impression a newcomer to the dispute will pick up. The superbly detailed survey of Amazonian languages by Aikhenvald (2012) takes the line of treating the issues as unfit for discussion, declaring that “there is neither consistency nor plausibility to the quasi-analytical statements which have been made concerning this language [Pirahã], or its culture, during the past fifteen years. I refrain from quoting these sources” (p. 411, n. 91). She thus avoids any discussion of the polemics of the post-2005 literature. In fact she cites nothing on Pirahã dated later than 1986.

Janet Chernela, an anthropologist specializing in Amazonia, recently tried to survey the whole dispute in an article for *Annual Review of Anthropology* (Chernela 2023). She seems to think she has provided a balanced summary, but her treatment of the relevant literature is hopelessly skewed against Everett. She never even mentions the existence of *Handbook of Amazonian Languages*, and hence never refers to Everett (1986b), unquestionably the most important descriptive document in the whole dispute. She cites Nevins et al. (2009b) without ever mentioning that it was followed by a detailed response (Everett 2009) in the same issue of *Language*, nor the rebuttal to that by Nevins et al. (2009a), nor the final rejoinder to that by Everett (2013). She very briefly mentions the incompetently uncritical review article by Hornstein (2019), but seems unaware of the vastly more expert critical one by Everett & Gibson (2019).

Admittedly, reading all of the post-2005 work just cited would be an exhausting business – anyone who doesn’t come out of reading it feeling dazed and confused just hasn’t been paying attention. But the skewing of Chernela’s coverage is quite extraordinary. It is possible that she fell victim of a major downside to accessing literature online: anyone who had *Language* 85 no. 2 in their hands could not fail to see that Nevins et al. (2009b) is immediately followed by Everett’s 37-page response, but if Chernela simply heard about the former and downloaded a PDF of it she might well have had no idea the latter existed.

However, she has less excuse in the matter of the two reviews. She cites Hornstein (2019) in connection with Chomsky's claim that "variation between languages – while possibly interesting for other purposes – is irrelevant to the nature of the FLN" (p. 140). But its first page carried an editor's footnote explaining that "This issue of *Language* contains two review articles focusing on the volume *Recursion Across Domains*", and adding: "Since the topic of this volume (recursion) is one of central interest (and some controversy) in current linguistic theory, we thought it important to publish reviews from scholars who will bring differing perspectives to the topic", and so on. Those differing perspectives do not come through in Chernela's account.

She makes some patently erroneous and unfounded claims, like that NP&R "reanalyzed data collected among the Pirahã by Everett's predecessors" (p. 140). NP&R did nothing of the sort, and do not try to represent themselves as having done it. Steven Sheldon, whose residence among the Pirahã antedated Everett's, did produce some transcribed texts, which are utilized by Futrell et al. (2016), but NP&R appear not to have known about them. NP&R (2009b: 391) do cite a table of six pronoun forms from a paper by Sheldon, but the paper (Sheldon 1988) appeared two years after Everett's main descriptive work on the language was in print.

In another inexplicable piece of invention, Chernela asserts that "Much of Everett's field methodologies involved structured interviews using a recorder" (p. 143), and she asserts that his work "flies in the face of Boasian anthropology" because it fails to "interpret cultures and languages on the basis of each society's own logic and values rather than through a universal yardstick" and "understand language as a social phenomenon in which meanings cannot be understood apart from context". But Everett's work involved interacting more closely with the community than any other outsider has ever done or was ever competent to do. He lived in the community and participated in its life for eight years. His children became fluent in the language and often played with Pirahã children all day. He constantly strived in his work to "interpret cultures and languages on the basis of each society's own logic and values". Throughout Everett (2012) it is clear that language is being seen as intimately linked to culture, and Boas is copiously discussed in Everett (2016).²⁵ Like NP&R, Chernela never met Everett or even emailed him. She seems to have decided up front that he was to be her representative of the typical desk linguist asking elicitation questions, not the sensitive anthropological investigator attuned to culture, values, and meaning.

²⁵Chernela mentions the existence of both these books (p. 144), but only in passing, and she misstates the title of the first.

The general pall of negativity that has been cast over Everett's work may be responsible for some of Chernela's bias. Like NP&R, she worked without any contact with Everett or anyone else who had ever lived with the Pirahã and learned their language. It was an anthropologist, Bambi Schieffelin, who suggested to Chernela that she might write the article, and neither of the two people thanked in her acknowledgment note for reading the paper in draft (p. 146) is a linguist. She does no linguistic analysis; she simply browsed some of the recent literature and came away with the broadly negative view of Everett's work that NP&R were intent on establishing as the default.

The end result is not too surprising given the intellectual climate that the campaign of hostilities created. Linguists should be ashamed of this ghastly parody of science, with its rumors of racism substituting for scientific discussion, and career sabotage replacing rational criticism. But what makes things worse is that it was under-informed from the start. To see why Everett in the early 1980s was trying to provide evidence of subordination in Pirahã, we need to look at certain events predating all of his descriptive work, but the digression is a relevant one.

3 Overlooked prehistory

In 1975, Daniel Everett was 24 and had just completed a Diploma in Foreign Missions from the Moody Bible Institute in Chicago. He and his wife were making plans to enter service as missionaries and bible translators for the Summer Institute of Linguistics (SIL) in South America.

Four thousand miles away, I was a 30-year-old lecturer in linguistics, completing my first year at University College London. I had spent 1973–74 at King's College, Cambridge, learning typology from Ed Keenan and Bernard Comrie, and spent the summer of 1974 at the LSA Linguistic Institute at U Mass Amherst learning from Chomsky, Halle, Keyser, Perlmutter, and Postal.

In 1976, barely done with writing my PhD dissertation on rule interaction in classical transformational grammar, I was asked if I would take on the supervision of a prospective PhD student: a 54-year-old SIL missionary named Desmond Cyril Derbyshire. He had had no college degree; before he became a missionary he had been a chartered accountant in Durham, England. I'm not sure whether my senior colleagues believed the work of a middle-aged missionary would amount to much, but fortunately for me they allowed him to enroll, and I agreed to be his *de facto* advisor (*de facto* because the university did not allow someone of my lowly rank to be a doctoral supervisor). He turned out to be perhaps the finest scholar I ever worked with.

3.1 Discovering Amazonian languages

By the time I met Derbyshire he had done nearly 20 years of work on a Cariban language I had never heard of, spoken on a northern tributary of the Amazon. In a lecture on constituent-order typology I presented arguments (set out in then-forthcoming article, Pullum 1977) that there was no convincing evidence for any language in the world having an object-initial basic constituent order (OVS or OSV). The only surface orders for the major constituents of the clause permitted by universal grammar seemed to be SOV as in Hindi, SVO as in English, VSO as in Irish, and VOS as in Malagasy. Derbyshire raised a hand from the back row and reported that he had been working on a language that he believed strongly preferred OVS as the order in transitive clauses.

The language was Hixkaryana. We arranged to meet after class so that I could learn something about its clausal syntax. Derbyshire had actually published a preliminary study of it back in 1961, when I was in high school (Derbyshire 1961), and it included a remark (using the terminology of Kenneth Pike's largely forgotten tagmemics framework) that "the goal always precedes, and the actor usually follows, the predicate tagmeme". In post-Greenberg terms, that meant OVS. But there had been no discussion of this language in the subsequent literature.

I gave Derbyshire some ideas on how he might confirm that he really was dealing with an OVS language: there was the possibility that (for example) Hixkaryana was just an SOV language in which the subject was occasionally shifted to clause-final position in special discourse contexts. There were substantial stocks of data to consult: a collection of texts transcribed from native speakers and published in Brazil ten years before; a Hixkaryana version of the entire New Testament, checked throughout by native speaker consultants, in press in Brasilia; and plentiful supplies of other data collected during Derbyshire's twenty years of fieldwork, including a remarkable diary privately composed by a native speaker who had learned to write the language.

Text from all sources supported Derbyshire to the hilt. My belief that universal grammar precluded object-initial basic constituent orders was inescapably wrong. Hixkaryana was a rather rigid OVS language: always OV, with auxiliary after the lexical verb, and the subject clause-initial only infrequently, when specifically focused or contrasted with something else (see Derbyshire 1985: 74).

Derbyshire and I began work on publicizing what appeared to be the then new and surprising fact that there was definitely at least one clear case of an OVS language. I worked with Derbyshire on preparing a squib for publication in *Linguistic Inquiry* (Derbyshire 1977). And I suggested to him that his doctoral

work might permit him to also undertake a monograph for the *Lingua Descriptive Series* (LDS) that was being planned by Bernard Comrie and Norval Smith.

The LDS monographs were required to adhere to a format carefully designed to facilitate comparative research. The instructions for contributors were published as a special issue of *Lingua* (vol. 42, no. 1) as the *Lingua Descriptive Series Questionnaire* (Comrie & Smith 1977, henceforth *LDSQ*). It set out a systematic section-numbering scheme for organizing descriptions in the series.

I showed Derbyshire my copy of *LDSQ* as soon as I received it, and he not only took up the task of writing an LDS monograph, but worked efficiently enough to produce the inaugural one (Derbyshire 1979), a superb description which would have amply justified the award of a PhD – but in fact he also produced a distinct work to offer as his PhD dissertation under the title *Hixkaryana Syntax*, which presented the description somewhat differently and added a second part on typology and discourse syntax plus eleven appendices on phonology and morphology (it was published later as Derbyshire 1985).

The significance of *LDSQ* to this story becomes clear in the light of what its detailed instructions said about subordinate clauses. It specified that Section 1.1.2 of the description was to be headed “Subordination”. Subsection 1.1.2.1 was to state whether there are “any general markers of subordination, e.g. word order, particles (in what position?), verb modification, etc.”, and 1.1.2.2 was to cover “Noun clauses” – the full finite subordinate clauses that Jespersen calls content clauses. Section 1.1.2.2.3 was to deal with declarative content clauses (“indirect statements”), 1.1.2.2.4 was to treat interrogative ones (“indirect questions”), and so on. This had more significance than we then realized.

Derbyshire made some further visits to Brazil and began learning more about what other SIL linguists had found. We began to pick up reports of other OVS languages, plus one or two that seemed to be OSV. I obtained a grant from the UK Social Science Research Council to support Derbyshire’s work, not only on the syntax of *Hixkaryana* but also on these other reported languages. I learned a lot about the history, geography, ecology, and demography of Amazonia, and the appalling treatment of its indigenous inhabitants, and together Derbyshire and I prepared a paper entitled “Object initial languages” giving brief accounts of a dozen object-initial languages (it was later published in *IJAL* as Derbyshire & Pullum 1981). This led to our planning what became the four-volume *Handbook of Amazonian Languages* (*HAL*).

The relevance of *HAL* is, of course, that around 1983 or 1984 Derbyshire commissioned a chapter for it from the young Daniel Everett. His grammatical overview of Pirahã became the longest chapter in the first volume (Derbyshire & Pullum 1986, henceforth *HAL 1*).

Everett was by this time a PhD graduate of the Universidade Estadual de Campinas in Brazil (the first linguistics PhD in the country), with a dissertation on Pirahã grammar and syntactic theory. Derbyshire was aware that Pirahã was a genetically isolated and notoriously difficult language on which SIL had tried to make headway for a quarter of a century. Two previous missionary linguists had worked on it: Arlo Heinrichs, who did the difficult work of establishing initial contact with the Pirahã and worked with them from 1959 to 1967, publishing a preliminary view of the phonemes of the language (Heinrichs 1964), and Steven Neil Sheldon, who worked on the language from 1967 to 1976 and knows it fairly well. But Everett and his then wife Keren were the first SIL members who learned to speak and understand the language fluently. Everett's translation of the *Gospel of Mark* (Everett 1986a) was the first piece of bible translation SIL had ever achieved for the language.

To guide Everett and the other contributors of the grammatical sketches in *HAL*, Derbyshire and I had produced an analytical table of contents, much briefer than the questionnaire for the LDS but inspired by it. We reproduced it in *HAL 1*, pp. 31–32. And (the crucial point) Section 14 was to be headed “Subordinate clauses”. Everett had in fact already seen *LDSQ* as soon as it appeared, and was already assuming that he had to say things about subordinate clauses.

It should not be too surprising, then, if Everett diligently strove to find and exemplify subordinate clauses, looking for all the usual grammatical furniture that speakers of European languages and syntacticians at MIT would expect sentences to exhibit. NP&R represent it as suspicious that he would say in 1983 and 1986b that there were subordinate clauses and then say in 2005 that there weren't. But he was effectively being directed to say something about subordinate clauses by both of the two sets of instructions he was using as guidance.

Looking back now, what surprises me is that Derbyshire and I did not rethink our guidance, and change the question to “Are subordinate clauses found in the language?”; by the early 1980s we knew what Hixkaryana had taught us about the topic of subordinate clauses. Derbyshire followed *LDSQ*'s directions closely, so linguists do not have to wonder about what the subordinate clauses are like in any language with an LDS monograph; they can just turn to Section 1.1.2 and find out. Here is what Derbyshire says about Hixkaryana (p.21):

1.1.2. Subordination

Subordination is restricted to nonfinite verbal forms, specifically derived nominals (or, pseudo-nominals that function as adverbials – see 1.1.2.2.6).

Hixkaryana, then, had no content clauses at all. And turning to Section 1.1.2.3, “Adjective clauses (relative clauses)” – I’ll use the latter, more modern term – we find that in Section 1.1.2.3.1 the marking of relative clauses was to be described; in 1.1.2.3.2 the description should say whether there is a distinction between restrictive and non-restrictive relative clauses; and other subsections ask about their word order, etc. Here is the relevant passage:

1.1.2.3. Adjective clauses (relative clauses)

There is no construction of the adjective clause (relative clause) type. There are various means used to obtain the same effect as such a clause: simple nominalization; placing NPs together in a paratactic relationship, with intonational break; descriptive sentence, usually involving an equative clause (see 1.2.1.1.4); or some combination of these means.

So relative clauses did not exist in Hixkaryana either.

LDSQ also requires that 1.1.2.4 should cover “adverb clauses”, i.e. clauses functioning as modifiers of location, manner, purpose, cause, condition, result, or degree (1.1.2.4.2.1 – 1.1.2.4.2.7). On these, Derbyshire says:

1.1.2.4. Adverb clauses

The nearest equivalent to adverb clauses is what I have called adverb pseudo-clauses, for the same reason that I use the term ‘pseudo-clause’ in connection with nominal constructions (see 1.1.2.2.6). These adverb pseudo-clauses are either (i) postpositional phrases with a derived nominal as head of the phrase, or (ii) constructions whose nuclear element is a pseudo-nominal, without a postposition...

Thus Hixkaryana also lacks finite clauses serving adjunct function; it uses noun phrases (NPs) or phrases headed by adpositions (postpositional ones, henceforth PPs).

One other relevant thing Derbyshire reports (Section 1.3, p. 45) is that “There are no formal means in the language for expressing coordination at either the sentence or phrase level”. The English coordinators *and*, *but*, and *or* have no direct equivalents.

To summarize, everything one can immediately think of that might be used as the basis of an argument that sentences could be of arbitrary length in Hixkaryana is ruled out. Hixkaryana could have been mentioned among the languages I discussed in Section 1 for which the possibility of an infinite sentence inventory had been questioned in the literature long before 2005.

3.2 Everett's 1986 grammatical sketch

Everett's description of Pirahã (1986b), a revised English version of the descriptive part of his PhD dissertation occupying 125 pages of *HAL 1*, is considerably more than a sketch. It gives Section 14 (p. 262) a longer introduction than other descriptions in *HAL*, postponing exemplification for the more detailed subsections that followed. He mentioned topics like nominalization, parataxis, and the expression of temporal and conditional adjuncts, and but mostly commented on the complex verb morphology of the language, which allows for new verbs to be formed by including more than one verb root in a single word. Everett calls this "verb incorporation", mentioning the phenomenon known in relational grammar as clause union, but what he calls verb incorporation lacks two defining features of clause union: the amalgamated verb roots are invariably understood with the same predicand, and (significantly) he mentions that evidence of "underlying bisententiality" is absent

Everett states unequivocally that "There is no preclausal complementizer such as English *that* in Pirahã" (p. 262). In the early 1980s it was of course very natural to look for a "complementizer": Everett was strongly interested in government-binding theory (his dissertation title includes the words "and the theory of syntax"), and he wanted to show how transformational grammar would apply to Pirahã. But there was no COMP node to be found, because there were no finite complement clauses for them to introduce. This means the familiar right-branching nested English constructions that we invariably exhibit to undergraduates in our syntax classes (*A knows that B said that C thinks that P*) cannot be paralleled in a single Pirahã sentence.

Having noticed this, Everett voiced his suspicions to Noam Chomsky in conversation. Directly after receiving his PhD, before *HAL 1* was published, he received a fellowship enabling him to spend a year (1984–85) as a visiting scholar at MIT, where he had a conversation that he describes as follows (Everett 2007: 12, fn. 7):

I talked to Chomsky about my idea that there seemed to be very little evidence for embedding of any kind in Pirahã, apart from these *-sai* examples which I was beginning to question. We discussed it briefly and Noam gave me some ideas for further testing the idea. Mark Baker, writing his PhD under Noam at the time, mentioned to me one day as we were having lunch that Noam was really intrigued by the idea that a language might not have embedd[ing] (Mark said something like "You really got Noam's attention with what you told him about Pirahã" ...).

Chomsky, then, had heard about the apparent lack of embedding in Pirahã from Everett himself, twenty years before the *CA* paper, and was quite interested.

Everett adds: “I had a growing suspicion that my 1982 analysis was wrong, based ... on artificially and exclusively elicited data” (I return later to the highly significant issue of data elicitation), but he says he “did not take the time to work out an analysis with no hypotaxis at all until 2004, when working at the Max Planck Institute in Leipzig”.

4 Subordination and nominalization

NP&R were well aware that there were Amazonian languages that seemed to use nominalizations to do the work that English would do with subordinate clauses. They make this relevant point:

As is well known, it is quite common for embedded clauses to look more “nominal” than their main-clause counterparts, due to a partial or complete suppression of tense, aspect, or agreement distinctions found in the verbs of main clauses. Koptjevskaja Tamm (1993) adopts from Stassen (1985) the term DERANKED (vs. BALANCED) for reduced embedded clauses of this sort. Koptjevskaja-Tamm offers many examples of languages that (either exclusively or quite generally) use deranked constructions with nominal properties for complement-clause embedding. (Nevins et al. 2009b: 370)

They cite languages like Adyghe, Ancient Greek, Classical Latin, Inuktitut, Quechua, and Turkish as illustrating such “deranking”, and add that “deranked embedded clauses appear to be common among Amazonian languages”, citing Derbyshire (1987) and several descriptions from *HAL*, among them Wai Wai, Macushi, and the *HAL 1* chapter on Apalai (Koehn & Koehn 1986).

What they don’t mention is that they are just repeating this point from Everett (2005: 629). It is Everett who cited Koptjevskaja Tamm’s book. And that book is about nominalizations, not subordinate clauses. If we “rank” constituents by reference to main clause features such as tense, nominalizations could be regarded intuitively as “deranked” compared to content clauses. But nominalizations are NPs, not clauses. *We were unaware that the enemy had destroyed the city* has a subordinate clause in it, but *We were unaware of the enemy’s destruction of the city* does not. After the publication of Chomsky (1970), generative grammarians ceased even trying to derive nominalizations transformationally from clauses.

What’s more, linguists still do not know how to draw a clear line between embedded clauses and nominalizations. It is clear even for English. There are several constructions that can (at least approximately) express the semantic content

of a clause in a less assertion-like or prominent way. Some express the downgraded material in a clause-like constituent that lacks certain main clause properties such as tense or agreement; but others, like Hixkaryana, have only very rough semantic parallels to clause structures, exhibiting both the structure and the distribution of NPs. Consider the following English expressions related to the declarative main clause *I ate it*:

- (1) a. that I ate it
[finite content clause]
- b. for me to eat it
[infinitival clause]
- c. me eating it
[“ACC-*ing*” construction]
- d. my eating it
[“POSS-*ing*” construction]
- e. my eating of it
[event nominalization, genitive determiner NP as agent]
- f. the eating of it
[event nominalization with definite article]

Uncontroversially, (1a) is a transitive content clause, and most modern linguists would call (1b) a transitive clause too. And (1f) is certainly a simple definite NP. But in between there are other constructions. The trouble starts with (1c). Linguists differ radically on where clauses stop and NPs begin. The morphology of the head in (1c) and (1d) is no help: the *-ing* verb form is called the “gerund-participle” in *The Cambridge Grammar of the English Language* (Huddleston & Pullum 2002) because no verb in English distinguishes the form used in the progressive aspect (*I am eating it*) from the form used in (1c) and (1d). Morphology therefore does not help draw the line between clauses and NPs (after all, many words ending in *-ing*, though derived from verb roots, do not belong to verb lexemes at all).²⁶

The generative literature on these constructions has considered arguments based on a wide range of phenomena; Pullum (1991) gives a systematic survey of the data. Calling (1d) an NP accounts nicely for the way it can be the object

²⁶An unhelpful irrelevance, which I will ignore, is that many prescriptive usage authorities insist that (1c) is a deprecated form that should be corrected to (1d). I take this view to be untenable; the more scholarly usage manuals reject it, noting the free variation between them found throughout English literature.

of a preposition, as in *She disapproved of my eating it*. Jackendoff (1977: 222–223) accordingly takes that view; so does Pullum (1991); and so does Blevins (2005), despite having criticisms of Pullum (1991) and citing others who disagree with it.

Kiparsky (2017), however, carefully argues for treating (1d) as a clause – but a clause with the unusual property of needing (in Chomskyan terms) to be assigned case, as NPs are. That is essentially what Stowell (1981) also advocated. It agrees with Jackendoff, Pullum, and Blevins that (1d) has the external syntax of an NP, but differs by assigning it the root node label that clauses have.

Neither Pullum nor Kiparsky is very clear on the status of (1c), the so-called “ACC-*ing*” construction. Blevins argues firmly that it too is an NP. However, Rodney Huddleston convinced me, a decade after I wrote Pullum (1991), that it is a clause, and also that the “ACC-*ing*” and “POSS-*ing*” constructions are too similar in both external and internal syntax to make it plausible that one is a clause and the other is not. So *The Cambridge Grammar* treats both (1c) and (1d) as non-finite subordinate clauses differing only in the superficial case-marking of the subject. My earlier view disagrees with my later view, and I am still not entirely sure which is right. (I was lucky enough not to face an inquisition by NP&R accusing me of trying to dishonestly cover up my earlier view.)

There is much more generative literature on “ACC-*ing*” and “POSS-*ing*” constructions than I can discuss here, but the bottom line is that six decades after the earliest generative studies of English nominalization and subordination, there is still no sign of broad agreement on where to draw the line between NP and clause constituents. And if linguists are not clear where we should draw the line between clauses and nominalizations in English, we can hardly be confident about answering similar questions in vastly less-studied languages. For Nevins and colleagues to claim they know exactly where to draw the line between clauses and NPs for Pirahã is absurd hubris.

4.1 A few Pirahã examples

NP&R spend 50 pages of *Language* trawling through Everett’s work looking for dishonesty. They blow plenty of smoke but come up with essentially nothing definitive. I’ll discuss just three examples that might appear to be of interest because their English translations contain non-finite subordinate clauses. They can be found in Everett (1986b) Section 14.2.1, headed “Infinitives, participials and gerundives”, pp. 262–263 (just the terms that might be used if the section were describing English).²⁷

²⁷In citing Pirahã I’ll follow Everett’s transcription, except that his orthographic ‘x’ for the glottal stop consonant is singularly hard for a linguist to get used to, so I replace it with the IPA glottal stop symbol ‘?’ in transcribed examples.

- (2) a. Kóʔoi soʔóá ʔibíibihái tiobáhai bíio kai-sai
 Kóʔoi already order.PROX.RELCERT child grass do [+sai]
 b. hi obáaʔái kahaí kai-sai
 3rd see/know.INTENS arrow make [+sai]

For (2a) and (2b) Everett gives English translations containing infinitival subordinate clauses. His free translation of (2a) is ‘Kóʔoi already ordered the child to cut the grass’ (where ‘REL CERT’ is an epistemic mood suffix signaling a report of something relatively certain). His translation of (2b) is ‘He really knows how to make arrows.’ NP&R seize upon these as examples of the subordinate clauses that Everett is supposedly now trying to conceal. But Everett actually took both to be nominalizations correspond to the subordinate clauses in English (an echo of the way Derbyshire had found nominalizations doing the work that English does with subordinate clauses). Both have the verb stem *kai*, which is like French *faire* in covering the meanings of both ‘do’ and ‘make’. The constituents at issue are *bíio kai-sai* (grass-doing) and *kahaí kai-sai* (arrow-doing).

In the 1980s Everett thought *-sai* was a nominalizer, glossing it ‘NOMLZR’, and he continues to gloss it as ‘nominalizer’ in the CA article (where it is misprinted several times as ‘NOMINATIVE’ owing to careless proofreading). This could mean that the examples might have been better translated as ‘Kóʔoi already assigned the child the grass-cutting’ and ‘He really knows arrow manufacture.’ NP&R, of course, have no idea whether the NP analysis is correct, or whether we are looking at subjectless non-finite VPs.

The results of their poring over Everett’s work cannot be construed as adequate support for the claim they want to make – that Pirahã has clause embedding of the sort familiar from the Indo-European languages.

A few pages later Everett gives (in his (290) on p. 278) example (3), which might look more promising as a case of a subordinate clause.

- (3) hi ti ʔapi-sai ʔogi-hiab-a
 3rd 1st go [+sai] want.not.REMOTE

It consists of a 3rd-person pronoun, a 1st-person pronoun, a verb meaning ‘go’ with *sai* suffixed, a verb stem that means ‘want’, the negative suffix *hiab*, and the remote aspect suffix *a* (on which see Everett 1986b: 293–294). In his early work, up to 1986, Everett thought it might best be translated as ‘He doesn’t want me to go.’ NP&R seize upon it as a highly significant case of his having cited a sentence with a subordinate clause in object position preceding a matrix verb of desiring (see their (23) on p. 375). It surely could not be plausibly treated as two successive main clauses in paratactic relationship.

But (3) is problematic in a way that Nevins et al. were unaware of – and here they fell victims to their policy of avoiding all contact with Everett. Looking back at the origin of sentence (3), Everett recalls that he constructed it himself, and asked speakers whether it was acceptable – a use of the problematic “can-you-say” question.

Everett was never able to make much use of questions put to speakers in his language learning. “How-do-you-say” questions (Samarin 1967: 114, Ch. 6, Sakel & Everett 2012: §6.4) were ruled out because he had no contact language in which to ask them.²⁸ Hardly any Pirahã men (and none of the women) have even the crudest smattering of Portuguese (again, see Sakel 2012); no one raised as a native speaker in the Pirahã community seems ever to have subsequently become fluent in Portuguese. Everett does mention that early on he would sometimes be able to point to something and ask “How do you say that?” (Everett 2008: 20) – presumably to elicit a noun; but that won’t do for most concepts.

Later on, when he had attained a basic grasp of the language, he relied a lot on “perambulatory elicitation” (Everett 1986b: 200), which means walking around the village chatting to people. But that still cannot be called upon to elicit some key form that will help resolve some puzzle about syntactic possibilities. When his conversational abilities had improved enough, therefore, Everett sometimes used “can-you-say” questions. These have the advantage of being usable in a fully monolingual situation, given only enough command of the target language to express the question “Can you say *S*?” and pronounce the conjectured candidate utterance *S*. So it becomes possible, at least potentially, to check hypotheses about what is grammatical. But of course you don’t know what you’re going to get.

This mode of proceeding calls for great caution, especially when working with linguistically unsophisticated speakers (which will be most speakers of most languages in the world, of course). “Can you say” questions presuppose that the consultant will understand that the *S* is being mentioned, not used, and that the linguist is not asking for permission to say something, or asking about physical possibility, but rather wants a judgment of concerning grammatical correctness in isolation from context. What Everett discovered in later years was that the Pirahã had regularly been saying “Yes” to his occasional “Can-you-say-*S*?” questions, just to humor him, even if the *S* was decidedly unidiomatic.

Everett was caught out by exactly this behavior in another case. Early in his study of Pirahã he assumed it obviously should be possible to have more than one attributive modifier in the structure of a Pirahã NP, just as in English. In

²⁸Fastidious field linguists shun them anyway, even when a contact language is available. Bloomfield never used them at all, according to Voegelin (1960: 204).

example (268) of Everett (1986b: 273) he cited (4) as the largest NP he had in his corpus (and I give his 1986 glosses):

- (4) kabogáohoi biísi hoíhio ʔitaíʔi
barrel red two heavy
'two heavy red barrels'

The two modifiers might suggest modifiers can be stacked in NP. But he had made several errors with (4). The example wasn't really in his corpus in any strict sense. He expressed unease even when citing it, acknowledging that the example "is rather artificial" and "was not taken from textual material but rather was separately elicited". He later became convinced that the example is ungrammatical. Just as he discovered that *biísi* (based on *bií* 'blood') means 'bloodlike' rather than 'red', and *hoíhio* doesn't mean exactly 2 but rather 'a couple' or 'a bit' (in a vague sense that implies roughly 2 or 3 with count nouns), he also learned that it was another case of informants who said things were fine as a way of being tolerant of his imperfect grasp of their language: they would nearly always assent to his "can you say" questions. When he finally persuaded a speaker to give him the straight truth on whether (4) was acceptable, he was told: "Pirahã don't say that. You can say that. You are not Pirahã" (Everett 2009: 422).

The same sort of thing seems to have happened with (3). Since Everett never recorded anything like it in spontaneous use, he recently decided to seek a second opinion on it from Keren Madora (the only outsider who has lived with the Pirahã longer than Everett, and the only other outsider who is truly fluent in Pirahã). Formerly married to Everett, today she still lives very near the Pirahã area and is in regular contact with speakers. Her opinion (email, Madora to Everett, 10 January 2023) was that he is correct, (3) is ungrammatical. Pirahã speakers never spontaneously say anything like (3).

Highly relevant information concerning the suffix *-sai* was published in 2010 but was not available in 1986 or 2009. New empirical evidence indicates that *-sai* is not a nominalizer at all. Two of the only linguists outside of SIL who have worked directly with Pirahã speakers in a context where they could get reliable translations, Jeanette Sakel and Eugenie Stapert, constructed some test sentences by concatenating two Pirahã clauses translatable as 'it's raining' (*piiboibai*) and 'I don't go' (*ti kahápihiaba*), intended to suggest the meaning 'If it's raining, I don't go', and suffixing *-sai* to either the first clause or the second. They then asked nine speakers (seven women, two men) to simply repeat back what they'd said. They found that the informants' responses might have *-sai* on the first clause, or the second, or both, or neither, regardless of which input sentence they were given.

Their conclusion (see Sakel & Stapert 2010: 5–6) is that *-sai* “does not appear to be a marker of subordination, as originally claimed by Everett (1986)” (and they mean that it is not a marker of nominalization either). Everett agrees, and now believes it may be an optional marker for sentences conveying discourse-old information. Its random placement in sentence repetitions would be as expected if its old-information signaling role only made sense in a discourse context: speakers charged with repeating two sentences with no context apparently recalled vaguely that there was a *-sai* in there somewhere, but didn’t necessarily remember where.

What does it mean for sentences like (2a) and (2b), if *-sai* might not be either a nominalizer or a subordination marker after all? I’m not sure. And I don’t think anyone really is. But when looking at attested Pirahã examples, with their short clauses and unclear syntactic linkages, it is definitely useful to recall the perceptive remarks of Liberman (2006) on *Language Log*, published before either the Nevins boycott move or the first draft of NP&R’s paper, about sentences in conversational English as recorded by novelists with a good ear for colloquial speech. Liberman gives examples from Elmore Leonard. One character is quoted as saying things like ‘We get to a phone, we’re out of the country before morning.’ In the context it is clear that the intended meaning is conditional. One can imagine such a speaker saying, ‘It’s raining, I don’t go.’ Everett cites very similar examples of what he then thought were conditional clauses. For example (Everett 1986b: 265, ex. (241)):

- (5) Paió hi abópaisai ti ?ii oáboíhai.
 Paió hi ab-óp-ai-sai ti ?ii oá-boí-hai
 (name) 3sg turn-go-ATELIC-COND I thing buy-come-NEAR-CERTAIN
 ‘Paió comes back, I’m gonna buy something.’

I am not in any way suggesting that everything is now resolved and the picture is clear. Far from it. We have no truly reliable principles to use in order to decide whether some Pirahã construction is more analogous to *if he returns* or *him returning* or *his returning* or *his return*. All sorts of unclarities remain. Everett acknowledges having made errors in both elicitation and analysis; in 1986 he thought *-sai* was a morpheme forming subordinated constituents of some kind, probably nominalizations that played the role subordinate clauses would play in European languages, but after the convincing work of Sakel and Stapert he no longer thinks that. It has been definitely confirmed that *-sai* sometimes appears on what in English might be a subordinate clause but also sometimes appears on what in English would be a main clause.

In 1986 Everett also thought there were two *-sai* morphemes, differing in tone, but subsequent F0 measurements by Miguel Oliveira have revealed no statistically significant tonal difference (a rough set of slides presenting the results was made available as Oliveira & Everett 2010). Everett now thinks there is just one *-sai*.

Given the present state of our knowledge, we certainly cannot say that NP&R have refuted Everett's thesis about Pirahã sentences never exhibiting clause embedding. One might perhaps argue that the case is still open, but not that NP&R examined the matter and settled it – which is what far too many linguists (Chomsky included) have been lazy enough to assume. Simply citing Nevins et al. (2009b) without getting into any of the details is not sufficient. Those who are truly intent on trying to support the ungracious claim that Everett lied are going to have to start learning Pirahã.

4.2 The crucial issue of embedding depth

There is a vital point about nominalizations that NP&R either failed to notice or chose not to mention. What we really need to know, if we are to address the only issue that makes this discussion sensible, is whether a Pirahã nominalization (or non-finite clause or whatever) can be embedded inside another, and the result inside another, and so on, to arbitrary depths. NP&R struggle to find even a single case of a fully clear subordinate clause in Everett's early work (and they never venture to propose a structure for even a single sentence), but they certainly never even touch on the matter of showing embedding that can be reiterated to arbitrary depth. Nothing they say suggests that subordination in Pirahã (if it has any) can give rise to sentences of arbitrary length. And that is what any serious notion of "recursion" has to be about.

In Standard English, after more than a thousand years of literacy (which O'Neil 1977, Givón 1979, Mithun 1984, and Kalmár 1985 suggest might be a crucial consideration) now has fairly rich nominalization resources: even a clause like *A knows [that B said [that C thinks [that D predicts [it will rain]]]]* can be paired with a cumbersome NP analog like *A's knowledge of B's statement about C's opinion concerning D's prediction of impending rain* with roughly the same content.²⁹ But are such multiple embeddings of NPs constructible in every language? I have never been able to see a way in which the nominalization resources of languages like Hixkaryana, Apalai, or Pirahã could be used to replicate any such internally

²⁹The reader might like to consider whether one could construct a nominalization that exactly captures the content of the husband's thought in Bruce Eric Kaplan's well-known *New Yorker* cartoon (26 October 1998), where a man earnestly assures his wife: "Of course I care about how you imagined I thought you perceived I wanted you to feel."

ramified NP constructions. The most that NP&R have to suggest is that in one or two Pirahã examples there may be depth-1 subordination of a non-finite secondary predication, but they really only have what look like adsentential modifying phrases appended to a clause. They cannot exhibit Pirahã evidence supporting the claims of so many linguists that iterated embedding in human languages is always allowed to unbounded depth. That is the claim Everett was challenging.

5 Hallucinated PP self-embedding

The work presented in *Recursion Across Domains* (Amaral et al. 2018) is of astonishingly low quality, replete with glaring mistakes. The review by Everett & Gibson (2019) provides a selection of the evidence, concentrating most on Pirahã, on which the authors had worked together in the field. For the second review that *Language* commissioned, the editors certainly found the right man for the job: Hornstein (2019) faithfully repeated Chomsky’s theoretical position on “recursion”, elaborating the rhetorical escape-hatch arguments (see Section 1 above), and then proceeded to uncritically endorse all data-oriented contributions in the book regardless of their merits. Thus he reported that by using a truth-value judgment experiment Uli Sauerland (2018) had managed to “provide pretty dispositive evidence that Pirahã allows sentential embedding under ‘say’” (p. 796). In truth Sauerland’s statistical analysis has vitiating flaws, and when his experiment is run on English speakers it does not produce the results that would be needed to support his claims anyway (see the analysis by Everett & Gibson 2019: 781–784, who took the trouble to review the use of statistics and test his experimental design on English speakers, and the more detailed critique by Gibson (2024), Chapter 1 of this volume).

I will not attempt a general survey of the material in *Recursion Across Domains* here, but I will just address a particularly incompetent chapter about Pirahã PPs. Neither of the *Language* reviews mentioned the stunning error, and presumably none of the referees for the book noticed it either.

The chapter by Filomena Sandalo, Cilene Rodrigues, Tom Roeper, Luiz Amaral, Marcus Maia, and Glauber Romling da Silva (2018) claims that Pirahã syntax allows PPs to be embedded inside other PPs, and reports experiments purportedly showing that native speakers have no difficulty in processing and interpreting such phrases. The authors assume (as is clear from their (15) on p. 285) that the English phrase *the coin on the paper on the chair on the board* has a right-branching structure with a single NP constituent containing all the PPs as modifiers of successively embedded NPs: *chair on the board*, *paper on the chair on the board*, and so on.

They claim that Pirahã has precisely analogous phrases, with two differences. First, Pirahã lacks determinatives such as the English definite and indefinite articles; accordingly, it makes sense to ignore articles in the English structure shown below – it simplifies the tree structure considerably. And second, Pirahã PPs are postpositional. The right-branching structure for English diagrammed (without articles) in Figure 1 is claimed to have an analogous left-branching structure in Pirahã with the terminal string ‘*tabo apo tiapapati apo kapiiga apo gigohoi*’. (Sandalo et al. mistranscribe all of these words, but I set that aside that for now.) Taking into account “the fact that Pirahã is a head-final language”, they assume that an English structure in Figure 1 – where I omit determiners to save space – has an exact analog in Pirahã, which they depict (in their (19) on p. 287) as shown in Figure 2.

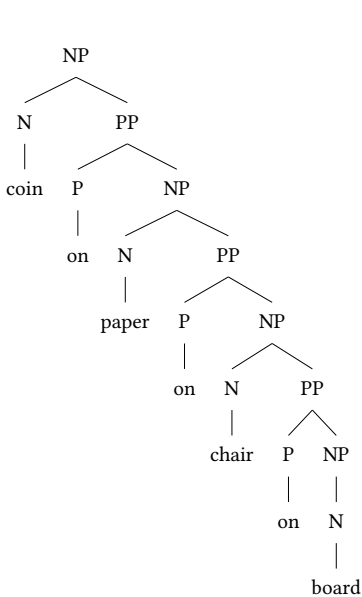


Figure 1: PP modifiers of NP in English.

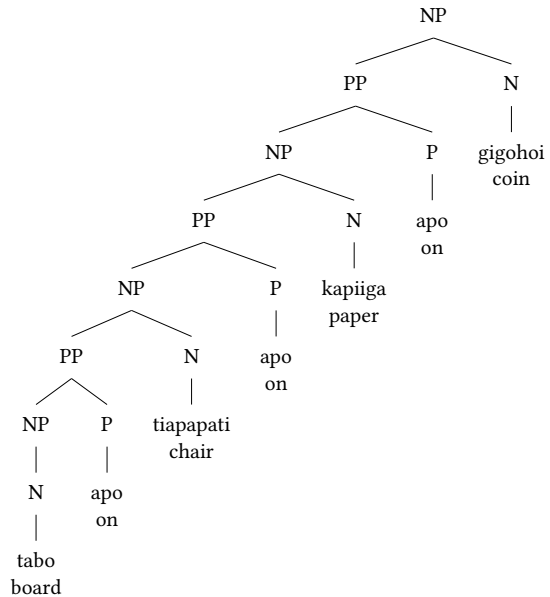


Figure 2: Sandalo et al.’s (2018) Pirahã PP structure.

Sandalo et al. have overlooked a crucial syntactic fact. Pirahã is NOT a uniformly head-final language. As Everett noted forty years ago, in the noun phrase “modifiers follow, while possessors normally precede, the phrase head” (Everett 1986b: 272). He lays out the sequence of elements in the NP as follows (p. 273):³⁰

³⁰See also Everett (1983: 132ff). Pirahã has no true numerals in the sense of names for the natural numbers, but presumably its vague quantity-related items like *báagiso* or *?aibá* ‘many’, *?ogii* ‘a lot’, and *?oihi* ‘few’ take that slot in the NP.

- (6) (POSSESSOR) + (PRO.CLITIC) + N + (MODIFIER) + (NUMERAL) + (DETERMINER)

The vital point is that modifiers follow the head in NPs. So if there were noun-modifying postpositional PPs embedded in NPs within other such PPs, the result would be nothing like the fictive left-branching tree in Figure 2. In fact there's a good reason that languages with nouns postmodified by PPs don't allow iteration of the construction: it yields center-embedding of the sort that poses major difficulties for human sentence processing – the kind seen in English center-embedded sentences like ?? *The children the women the soldiers left saved protested*.

The purported phrase Sandalo et al. are trying to diagram would actually come out as in Figure 4, where I correct the transcriptions and word identification as well as the structure.

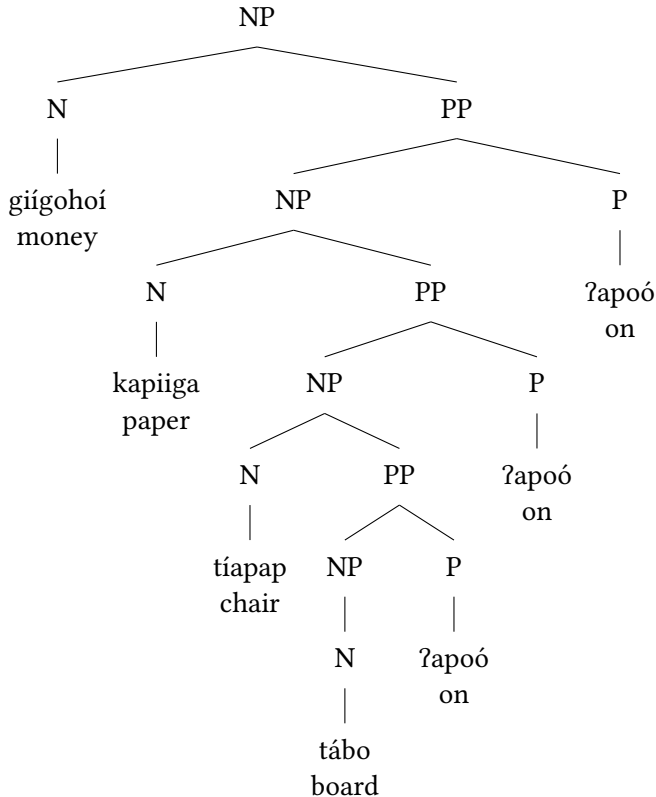


Figure 3: Expected structure if Pirahã had nesting of PP modifiers in NPs

No one has ever suggested that PPs like in Figure 4 are encountered in Pirahã discourse, and no such structures were presented to Sandalo et al.’s hapless informant.³¹

It is difficult to guess what must have gone on in their experimentation (they stress that it is to be regarded only as a pilot study). They claim to have found that a native speaker named Iaoá understood their pronunciation of the purely fictional phrase (Figure 2). Given that the word they write as *tiapapati* seems to be the imperative verb *tiapapaáti*, meaning ‘sit down’ (Everett & Gibson 2019: 786–787), Iaoá would have heard them as saying something that meant roughly ‘Sit on the board. On top. On the paper. Money.’ The corrected string is given in (7):

- (7) tábo ʔapoó tíapap ʔapoó kapiiga ʔapoó giigo-hoi
 board on chair on paper on money

The most likely guess at how Iaoá or any native speaker would have parsed this would be as a list of successive PPs and a final NP, as in Figure 4.

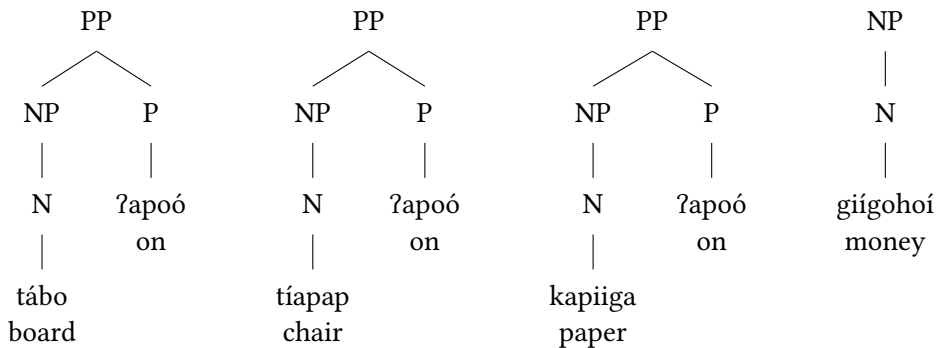


Figure 4: Most likely native-speaker parse of (7)

Convinced that they had identified nested PPs in Pirahã, Sandalo et al. (2018: 289–292) proceeded to construct some test sentences paired with pictures of alligators on mats on rocks on beaches, and claim to have used them to produce

³¹Later they give a second similar structure for NPs containing PP modifiers which is best ignored. Their (21) on p. 287 has nodes labeled “PP*” dominating other nodes with that label. On p. 284 they say they are using “notation adapted from traditional Kleene* system” [sic], but Stephen Kleene’s star notation symbolizes a unary operation mapping a set of strings to its reflexive and transitive closure under concatenation. It makes absolutely no sense in a node label.

evidence for interpretation of nested PPs. They claim a picture of an alligator on a mat on a beach was reliably distinguished from a picture of an alligator on a mat beside another alligator on a beach. Further discussion of this experiment is not really feasible; their account is too ill-informed and confused, replete with botched transcriptions, mistaken glosses, misidentified words (*tahoasi* is glossed as ‘mat’ when it actually means ‘beach’ – the word for ‘mat’ is *paahóisi*), and so on.

In another experiment they tried to get Iaoá to play a “game” involving coins being put on a paper that was on a chair on a board, or on a paper on a chair, or on a paper on a board. They note (p. 294) that where they supplied a string like “gigohoi kapiiga apo tiapapati apo tabo apo” (intended to be *giigo-hoi kapiiga ?apóó tíapap ?apóó tábo ?apóó*, glossed ‘coin paper-on chair-on board-on’), when Iaoá repeated the string “he switched the order of the PPs in the sentence”, yielding what they wrongly transcribe as ‘tabo apo tiapapati apo kapiiga apo gigohoi’ (‘board-on chair-on paper-on coin’). This was a sign of something gone terribly wrong: Iaoá was unable to come anywhere near repeating what they thought was a single NP in his language. But in an almost unbelievable fit of wishful thinking (hope springs eternal in the human breast), they interpret this as “spontaneous evidence” in favor of their hypothesis! It seems more likely that Iaoá scarcely knew what was going on, but took their attempted PPs to be independent phrases, not successively embedded modifiers in an NP, and repeated them back in LIFO (last in, first out) order.

There is also a very simple semantic observation that may play a role in interpreting the events that they take as vindication of their hallucinated PP embedding claims. We normally take the ‘on’ relation between medium-sized physical objects to be transitive. Any coin on a piece of paper on a chair is also a coin on a chair. Any alligator on a mat on a beach is an alligator on a beach.

The most plausible conclusion from Sandalo et al.’s bungled experiments is that Iaoá parsed the fictive PPs individually, and then (with the sharp general intelligence Everett has always noted among the Pirahã) simply guessed what the linguists wanted him to do.

6 Sentence-length extensibility more generally

As promised earlier, I have avoided the impenetrable thickets of confusion found where linguists use the words “recursive” and “recursion”; I have focused instead on the clearer issue of syntactic devices that can in principle allow the construction of sentences of arbitrary length.

The issue does not have the fundamental importance that some have seen in it. Linguistic creativity is not tied to any claim about an infinitude of sentences, since human linguistic creativity (as Everett has often stressed) resides mainly at the discourse level. Nor is it tied to the ability to grasp concepts. Absence of propositional attitude verbs in a language, for example, does not entail speakers' inability to engage in metacognition. Everett deftly illustrates how a complex proposition with a logical form like *[if [P and Q]] then R* does not need to be expressed in one sentence when he titled a conference paper: "You drink. You drive. You go to jail. Where's recursion?" (Everett 2010).

Everett's opponents seem to assume that linguistic life with only simple main clauses would hardly be worth living. But there is no reason to regard a language lacking unbounded sentence extensibility devices as less useful or expressive than a language. Kornai (2014) argues that the information-carrying complexity of a finite language can actually be greater than that of an infinite one.

One way of stressing the difference between finite and infinite languages, often touched on in undergraduate textbooks, depends on pointing out that for a finite language the grammar could be given in the form of a simple list of sentences. But that was never a very sensible point to harp on. From the complexity of verbs alone (Everett 1986b: 288–301) it is apparent that the set of Pirahã sentences would be way too vast even to be compiled, stored, or accessed by either a brain or a currently imaginable computer, let alone to be of real online use either cognitively or computationally. The grammatical complexity of Pirahã would still pose the usual problems for the theory of language acquisition: inducing generalizations from exposure to data would have to be involved, not just memorizing complete utterances. As Gibson (2024), Chapter 1 of this volume argues, what's important is compression of information (Kolmogorov complexity), not infinitude.

Whether the set of all sentences in a language is finite or not is in any case inherently difficult to settle, for a number of reasons, and would remain so even if all of Everett's specific claims about Pirahã syntax are accepted.

First, the lexicon has to be stipulatively fixed at some finite number N of words, though we have no clue about what N might be because new words (e.g. personal names) are being coined all the time, and the interaction of agglutinative word formation and lexicalization in languages like Turkish or Inuktitut makes it implausible that there is any such N at all.

Second, the notion "sentence" needs a clear definition; syntacticians casually assume it is a well-understood primitive term, but it is not easily defined at all. Separating a passage of spoken language into sentences in a way that a different linguist would replicate is very difficult, and beset with problems raised by

false starts, parenthetical interruptions, direct quotations, appositional expansions, rhetorical repetitions, whatever semicolons represent in writing, and asyndeton (coordination without coordinator words, as in Dickens's *It was the best of times, it was the worst of times, it was the age of wisdom, it was the age of foolishness ...*).

Third, with regard to hypotaxis (subordination), Pawley & Syder (2000) argue that it hardly occurs at all in spontaneous speech, even in English, once we set aside a limited number of high-frequency partially customizable schemata like *I think ___* or *It depends whether ___*, and similar formulas. This would presumably be all the more true for languages spoken in cultures where no one writes or reads. A few folk tales or epic poems might have a broadly fixed (or even faithfully memorized) traditional form, but most language use will be informal chatting, and Pawley and Syder claim that spur-of-the-moment construction of hypotactic sentences will be rare to nonexistent.

There are other phenomena that could introduce difficulties: NP apposition, roughly definable as adjacent iterated NPs with the same reference and syntactic function (Karlsson 2010 cites an attested five-NP example in Swedish); intensificatory or iconic repetition of attributive adjectives (*a big, big, big problem*) or adverbs (*I really, really mean it*) or VPs (*They hit me and hit me and hit me ...*) or NPs (*cows, cows, ... cows, as far as you could see*). Such possibilities are seldom noted in reference grammars. Only study of large corpora of texts will tell us whether such iterable sentence-lengthening constructions are found in the syntax of an exclusively oral language like Pirahã.

How might we even estimate the likelihood that Pirahã truly has no unbounded syntactic resources for sentence lengthening? A beautiful and oddly neglected paper by Widmer et al. (2017) addresses this question. Widmer and colleagues suggest some additional methods that could be employed to figure out the probability of a language lacking such resources. They identify five ways in which NPs in Indo-European languages can be lengthened by embedding other NPs inside them: stacked genitive determiners, adjectivization-derived modifiers, modifiers with head marking, adpositional modifiers, and simple noun juxtaposition (I assume apposition is to be included under the latter heading). They show that Indo-European languages have repeatedly developed such devices and also lost them through syntactic change over the past few thousand years.

Through a clever calculation they then assess how likely an Indo-European language is to end up at a given time with at least one such device in its NP syntax, concluding that it is very high indeed: they estimate that with probability ~ 0.98 , any Indo-European language, at any given point in its history, will have at least one grammatical device for arbitrarily expanding NPs. As an explanatory

conjecture, they suggest that for some reason the human processing capacity finds it helpful for there to be some such mechanism provided by the grammar.

However, they add (p. 822): “With regard to sentence-level syntax, it remains an open question whether syntactic recursion or simple conjunction is preferred.”. To settle it, “a larger sample of data would be needed”. We cannot know what the answer is, or how likely it is that any arbitrary language in the world (not just in the Indo-European family) would have some kind of iterable sentence-lengthening syntactic device available at all times in its history. But suppose the probability of languages having such features were as high as ~ 0.99 . It would still be expected, given the 7,000 languages attested in the world today, that there might be 70 languages or more in which such devices are absent. The literature on ancient languages and languages of preliterate cultures has thrown up quite a few candidates, as discussed in Section 1. Pirahã just happens to be the clearest case – and the one that kicked the hornets’ nest politically.

7 Conclusions

No one should claim, in the present state of our knowledge, that we have a good understanding of the syntax of Pirahã (or for that matter any other language, even Standard English). The corpus study of Pirahã syntax by Futrell et al. (2016) is a sterling effort at utilizing what materials we have (specifically, parsing texts collected by Steven Sheldon in an effort to find evidence of subordination), but in many ways it just underlines how woefully unclear things are. Much more work has to be done.

That work will not be accomplished without collaborations that involve people who (i) have no advance commitment to particular results or empirical claims and (ii) are prepared to spend time paying close attention to everyday usage in the Pirahã speech community. That will mean extended residence in Pirahã villages, and consultation with people who have substantial experience with the language.

Such people exist. Steven Sheldon worked on the language from 1967 to 1976, and knows it well. Caleb Everett, Kristene Diggins, and Shannon Russell all learned to speak and understand the language when living in Pirahã villages as children, and their parents Daniel Everett and Keren Madora are outsiders with unprecedented fluency. Madora has studied the language in depth since 1977 and still lives close to the Pirahã villages; Everett spent a total of about eight and a half years with the Pirahã between 1977 and 2006, and made various visits thereafter, becoming fully fluent in the language. He translated the *Gospel of Mark* into it (Everett 1986a). Yet NP&R decided to work without having a single conversation with any of these people.

This represents a sadly missed opportunity. If linguists like NP&R had applied their analytical theoretical abilities to the available data in a collaborative spirit, drawing on the knowledge of active speakers of the language (particularly Everett himself), new linguistic insights might have been gained. That chance has been lost, probably forever. They have wrecked their credibility by making it so obvious that from the start they aimed simply to bring Everett into disrepute. All that linguistics ended up getting out of their work was an uninformed retrospective document review. They have divided linguists into two irreconcilable warring camps, and made the entire discipline of linguistics look, as it did to Tom Bartlett, like a snakepit of hostility.

Like any scientists, linguists have a duty to maintain ethical standards and intellectual open-mindedness – even when someone is claiming Chomsky was wrong about something, or when the popular press tries to fluff up a science story into something earth-shaking or theory-trouncing and publishes absurd overstatements.

Certainly it was ridiculous hyperbole for *New Scientist* (18 March 2006) to call Everett’s account of Pirahã “the final nail in the coffin for Noam Chomsky’s hugely influential theory of universal grammar”. If we’re honest we’ll admit that Chomsky does not have enough of a detailed theory of universal grammar to constitute a full coffinload. Nor do his opponents have solid enough empirical accounts of language acquisition to nail down the lid of such a casket anyway.

It was similarly absurd for the *Chicago Tribune* (10 June 2007) to suggest that Everett’s work is analogous to a high-school physics teacher finding “a hole in the theory of relativity”; but we all know that sort of thing often happens when popular news media try to cover science. Providing better and clearer hype-free accounts of our work to science journalists will be an enduring burden, but one that we all have to shoulder. Calmly, and with some understanding of the fragile and difficult business of popular journalism.

I can well imagine how irksome it has been for Chomsky to see overblown hype about a putatively theory-shaking discovery in the jungle repeated in scores of news sources. But that doesn’t justify the petty spite of his “charlatan” remark to *Folha de S. Paulo* in February 2009, or his assertion that “Daniel Everett’s contributions are basically nothing” in a 2021 video interview.³²

Over the past four decades, Everett can be fairly said to have done more for Amazonian linguistics than any other linguist now living. His detailed descriptions of Pirahã and Wari’ are lasting contributions, as is his energetic promotion and encouragement of descriptive work on other Brazilian languages. His basic

³²<https://www.youtube.com/watch?v=UBla-h36yWA>

claim about Pirahã syntax not permitting unbounded sentence length is very probably true. He did not deserve the years of hot-tempered public allegations and insults (or the worse incidents of insult, hate mail, and shouting in his face that he does not publicly report). A sector of our field seems to have lost its moral compass over this issue.

It speaks well of Everett that never in all the years since 2005 has he responded to his tormentors with insults or abuse: he argues points of fact, but he refrains from accusing his enemies of scientific misconduct, devious motives, or self-interested mendacity. For that, and much more, we should salute him.

And as regards the validity of the accusations hurled at him by his many opponents, none of them familiar with the lives and spoken language of the Pirahã, I quote in conclusion the opinion of a young Brazilian anthropologist writing recently about Pirahã culture (Felizes 2023: 59):

A relação de Daniel e Karen Everett com os Pirahã é algo que perdura até aos dias atuais. Durante mais de quarenta anos de convívio – permanente ou esporádico – conquistaram a reputação de grandes amigos, de saberem bem a língua, de serem exímios contadores de histórias e de se tornarem importantes aliados, a quem os Pirahã geralmente recorrem para resolver potenciais conflitos ou aprender coisas sobre o mundo dos brancos.

[Daniel and Keren Everett's relationship with the Pirahã is something that has endured to the present day. During more than forty years of coexistence – permanent or sporadic – they gained the reputation of being great friends, of knowing the language well, of being excellent storytellers and of becoming important allies, to whom the Pirahã often turn to resolve potential conflicts or learn things about the white world.]

That is the view formed by an independent third party with a personal commitment to studying the life of the Pirahã, some who has spent time in Pirahã villages, made the acquaintance of Keren Madora [formerly Everett], and witnessed the consequences of the Everetts' 46 years of friendship with the Pirahã at first hand.

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I have known Daniel Everett and his work since 1983, and gratefully acknowledge his openness and candor during our discussions of this topic. During four decades of friendship we have agreed on many things and disagreed on many others. In writing this paper I have confirmed claims about specific people's actions

and statements from suitable sources or personal reminiscences as far as that was possible. I thank many friends who have spent time reading drafts, answering my questions, helping me to verify facts, saving me from errors, and making useful suggestions. Among them are Judith Aissen, Ash Asudeh, Peter Austin, Jim Blevins, Bernard Comrie, Peter Culicover, Hope Dawson, Lise Dobrin, Ted Gibson, John Goldsmith, Randy Allen Harris, Lloyd Humberstone, Brian Joseph, John Joseph, Ed Keenan, Bob Ladd, Pim Levelt, Bob Levine, Noah Ley, Joan Malting, John McWhorter, Philip Miller, Stefan Müller, Georgia Morgan, David Nash, Johanna Nichols, Steven Piantadosi, Steven Pinker, Jerry Sadock, Jeanette Sakel, Rich Thomason, Sally Thomason, Tom Wasow, Rebecca Wheeler, Melinda Wood, and Annie Zaenen. Correspondence with David Pesetsky and Cilene Rodrigues helped me to improve the accuracy of certain claims in Section 2. Errors may remain, and they are solely mine. Presentations of some of the content of this paper were made during 2023 at MIT, George Mason University, the Max-Planck Institute for Psycholinguistics in Nijmegen, and the 2024 meeting of the North American Association for the History of the Language Sciences.

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Chapter 3

How language began: A theoretical interpretation

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In his book Everett (2017), Daniel Everett argued that linguistic communication did not originate with *Homo sapiens sapiens* but rather began two million years earlier with *Homo erectus* [HE]. The linguistic system proposed by Everett for HE is not as complex as modern language but is more than adequate for the demands of HE's sociocultural and technological needs. This paper presents an analysis of the linguistic system of HE in terms of a theory of grammar, namely Role and Reference Grammar (Van Valin 2005).

1 *Homo erectus* and language: A long-standing puzzle

While full-blown language is generally agreed to be a property of *Homo sapiens sapiens*, there is tantalizing evidence that points to earlier humans as having some kind of communication system much more sophisticated than animal systems yet not as complex as modern language. Everett (2017) makes the case for human language to have originated well before the advent of *Homo sapiens sapiens*, some two million years earlier with *Homo erectus*. Unlike the risible Prometheus story invented by Chomsky, there are solid grounds for concluding that Upright Man had a communication system which was much more sophisticated than animal call systems, if not as complex as modern language. Everett reviews these in detail and presents convincing evidence for this view. One strong piece of evidence is the geographical distribution of *Homo erectus*: groups migrated out of Africa not only into Europe and the Middle East but all the way to Southeast



Asia and China. So-called “Peking Man” was *Homo erectus*, as was “Java Man”. The trek into Southeast Asia involved crossing substantial bodies of water, and this required building and operating watercraft. It is inconceivable that such journeys could be accomplished by groups with no way to convey complex thoughts and ideas among themselves, especially when it came to building and operating watercraft.

The discussion will proceed as follows. The first section introduces the complexity hierarchy of grammars assumed by Everett. The second section presents a sketch of a Role and Reference Grammar [RRG] (Van Valin 2005, Van Valin & LaPolla 1997) analysis of the system which *Homo erectus* might have utilized. The third section concerns the role of information structure in communication systems as simple as these and the implications for conclusions about *Homo erectus*. The fourth section briefly touches on how a system like that of *Homo erectus* could have evolved into a grammar higher on the hierarchy of grammars introduced in Section 2. Conclusions follow in Section 6.

The analysis to be presented is agnostic with respect to whether the communication system attributed to *Homo erectus* was manual or oral or some combination of the two modalities.

2 Complexity of grammars: Everett’s hierarchy

One of the immediate difficulties in discussing the possible linguistic abilities of early humans is the obvious fact that they are very different from those of modern humans, and accordingly, if the question is phrased “did early humans have language?”, where “language” is understood as being like contemporary linguistic systems, the answer is obviously “no”. Everett avoids this trap by talking instead of grammars and proposing a hierarchy of grammars in terms of their formal complexity: $G_3 \Rightarrow G_2 \Rightarrow G_1$. The simplest grammar, G_1 , is dubbed a “linear grammar” by Everett, because it permits only sequences of expressions without any embedding of one expression in another. An example is given in (1).

- (1) Output of a G_1 linear grammar: “Man see deer ... Deer big ... Man spear deer ...”¹

Each of the expressions is a simple proposition, which may be juxtaposed linearly with other simple propositions to form more semantically complex utterances.

¹It is irrelevant for this discussion whether the word order is Actor-PRED-Undergoer, Actor-Undergoer-PRED, or PRED-Actor-Undergoer.

The intermediate grammar in the hierarchy, G_2 , is labeled a “hierarchical grammar”, because it allows the embedding of one expression inside another, e.g. a modifier inside of a reference phrase. This is exemplified in (2).

(2) Output of a G_2 hierarchical grammar: “Man see big deer ... He spear it ...”

Instead of having a separate proposition *deer big*, the information regarding the size of the deer is incorporated into the referring expression *deer*.

G_3 is a “recursive hierarchical grammar”, the complex grammatical system characteristic of most, but not necessarily all, modern languages. Everett suggests that most likely *Homo erectus* had a G_1 grammar, and that will be the focus of this discussion.

3 A Role and Reference Grammar analysis of G_1 grammars

On p. 198 of Everett (2017) there is a partial RRG tree diagram illustrating some of the structural complexity in a G_3 grammar. That tree represents only one of the four projections of the clause posited in RRG: there are constituent, operator, information structure, and prosodic projections. Only the first two are represented in Figure 1 below.

The constituent projection contains lexical items functioning as predicators, arguments, adjuncts and modifiers. The layered structure of the clause consists of the NUCLEUS, the syntactic unit housing the predicating element, typically, but not necessarily, a verb. The nucleus plus the semantic arguments of the predicator constitute the CORE of the clause. The English verb *give* has three semantic arguments, yet there are only two arguments in the core in Figure 1. The third semantic argument is a WH-expression, and occurs displaced in the PRE-CORE SLOT, the normal position for displaced WH-expressions in many languages. Locative and temporal adjuncts normally appear in the core-level periphery, since they specify where and when the event coded in the core takes place. In this example the temporal adjunct *yesterday* functions as a frame-setting topic and occurs in the PRE-DETACHED POSITION. The CLAUSE contains the core plus the PRE-CORE SLOT and the core-level periphery, while the SENTENCE includes the clause, the clause-level periphery, and the pre-detached position.²

The auxiliary *did* is not attached to the constituent projection, because it is not lexical but rather grammatical in nature: it expresses two important operators, tense and illocutionary force. It is, therefore, attached to the operator projection.

²Some languages have a POST-CORE SLOT and/or a POST-DETACHED POSITION.

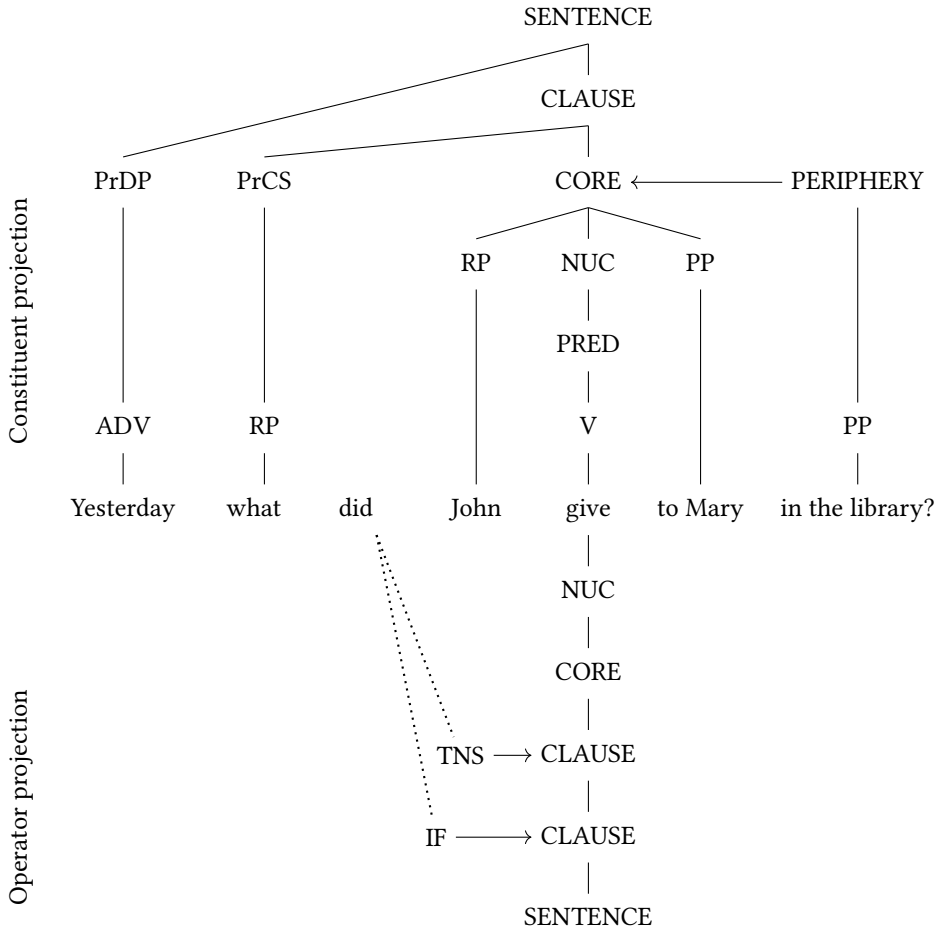


Figure 1: Clause structure in a G₃ language

3 How language began: A theoretical interpretation

What would a sequence of utterances in a G_1 language look like? Consider the following mini discourse in (3).

(3) Near river ... I see deer ... (It/deer) big ... (I) spear (it/deer) ...

It would have the following structure.

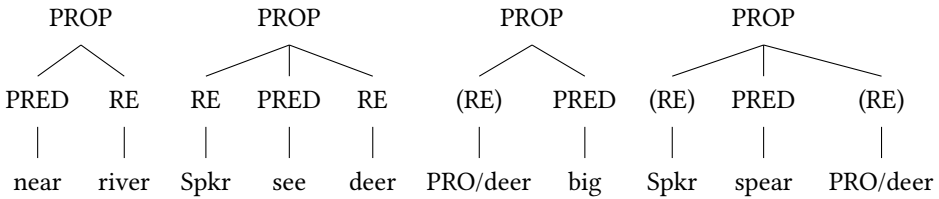


Figure 2: Structure of utterances in G_1 language

The first thing to note is the lack of syntactic categories. There are no grounds for attributing syntactic categories or syntactic structure to these utterances. The categories are all semantic: RE is REFERRING EXPRESSION and is not phrasal; PRED is PREDICATOR; and PROP is PROPOSITION. A proposition consists of a predicator and its arguments. There are no adjuncts modifying the proposition or any of its constituents. When a location needs to be mentioned, for example, it is expressed as an independent locative proposition, analogous to the independent attributive proposition involving the referring expression *deer*.

The equivalent of lexical modifiers, as illustrated in Figure 2, would be represented as independent propositions. What about non-lexical, i.e. grammatical, modifiers? It is highly unlikely that there are any grammatical modifiers of this kind found in a G_1 grammar of the type posited for *Homo erectus*. Hence there would not be an operator projection in the representation of utterances. However, there are two operators which are found in the grammar of every G_2 and G_3 human language and must have been part of any possible *Homo erectus* G_1 system: negation and illocutionary force. Negation is essential for reasoning as well as for important speech acts like negative imperatives and warnings. The ability to make assertions, ask questions and give commands is an essential part of any human communication system. It is for these reasons that RRG claims that negation and illocutionary force are the only universal operators. Both can be expressed through non-grammatical means: illocutionary force can be signaled prosodically, while negation can be expressed gesturally. Hence they would not motivate an operator projection in the structures.

4 Information structure, argument realization, and cooperation

In the hypothetical G_1 example in (3) and Figure 2, after the first mention of a referent, there are three possibilities for subsequent mentions: (1) repetition of the referring expression, (2) using a PRO form, or (3) simple omission, as is often the case in many G_3 languages today. Option 1 requires no special machinery; it is the most redundant. Option 2 is the least likely, since the development of PRO forms seems to be more likely a trait of the advanced systems. The most interesting option is (3). It was argued in Van Valin (1990) and Van Valin & LaPolla (1997), following Kuno, Bolinger and Bickerton, that information structure plays a central role in the analysis of intrasentential pronominalization, regardless as to whether it involves overt PRO forms or zero anaphora. For example, a referent cannot be realized as zero if it is part of the actual focus domain of the clause but can be if it is part of the background. So in the earlier example, it would be nonsensical to introduce *the deer* using zero coding. Hence overt occurrence vs. omission would likely not be beyond the means of *Homo erectus*. Thus possibility (3) is very much an option.

If *Homo erectus* is sensitive to some aspects of information structure, then this has significant consequences for the issues raised at the outset of this discussion. It was argued in Van Valin (1993), following Kempson (1975), that the notions of topic and focus, which are fundamental to information structure, are ultimately derived from Grice's Cooperative Principle and the maxim of quantity, which are general (i.e. not domain-specific) rational principles of human behavior. Cooperation is a hallmark of language users, and despite the fact that it is certain that *Homo erectus* did not wield the Cooperative Principle in the same way as modern G_3 language users do, it nevertheless was a necessary part of *Homo erectus* cognition. An example where cooperation would be vital is trying to reach islands separated from them by a significant body of water; cooperation is essential in the construction and operation of the primitive watercraft on which they traveled and on which their lives depended.

5 The transition from G_1 to G_2

A G_2 grammar would differ from a G_1 grammar in significant ways. To begin with, the combination of adjunct modifiers and referring expressions yields REFERENCE PHRASES, which are necessarily syntactic, because a reference phrase potentially consists of two or more units that are not of the same semantic type, e.g.

[_{RP} [_{PRED} big] → [_{RE} deer]]. In the same vein, the cooccurrence of syntactic reference phrases in a proposition triggers a reanalysis of the proposition as a syntactic entity, a core. In addition, the occurrence of adjunct modifiers taking a propositional unit as an argument, e.g. *I see big deer near river* (i.e. *near'* (river, [_{Spkr} see big deer])), further motivated the reanalysis, as the predicate+argument(s) unit is now functioning as an argument and filling a slot that could also be filled by a syntactic entity, namely a reference phrase (e.g. *Big deer near river*). The predicator underwent reanalysis as a syntactic nucleus due to, among other things, the occurrence of syntactic entities as the predicator, e.g. 'Spkr good hunter'. Thus, the introduction of embedding had profound implications, because it created semantically mixed units which led to the reanalysis of the fundamental semantic entities as syntactic, as illustrated in Figure 3.

The two most salient changes are the transformation of the attributive predicator *big* into a part of the referring expression *deer*, thereby creating a syntactic reference phrase, and the reanalysis of the locative proposition *by the river* into a propositional modifier. The result is more compact expressions with modification relations directly coded.

6 Conclusion

In this brief note I have sketched out what an RRG analysis of a G_1 linguistic system which could have been employed by *Homo erectus* might have looked like, based on the account given in Everett (2017). Dubbed a “linear grammar” by Everett, it would specify a linear string of propositions, as in Figure 2, which would be semantic in nature. There is nothing to motivate the positing of syntactic categories or structure. Of particular interest is the role of information structure, which gives evidence that Upright Man had a rudimentary understanding of Grice’s Cooperative Principle and at least the the maxim of quantity, since it underlies the important notions of topic and focus.

There is little agreement among researchers investigating primate cognition as to whether non-human primates have shared intentionality, i.e. the ability to recognize con-specifics as being intentional and mental agents. It is clear, however, that early humans, including *Homo erectus*, had shared intentionality. They were, so to speak, “Gricean apes”.

The transition from a semantic G_1 to a syntactic G_2 was briefly discussed. It was argued that the introduction of embedding into the grammar led to a transformation of the grammar from being essentially semantic to being primarily syntactic.

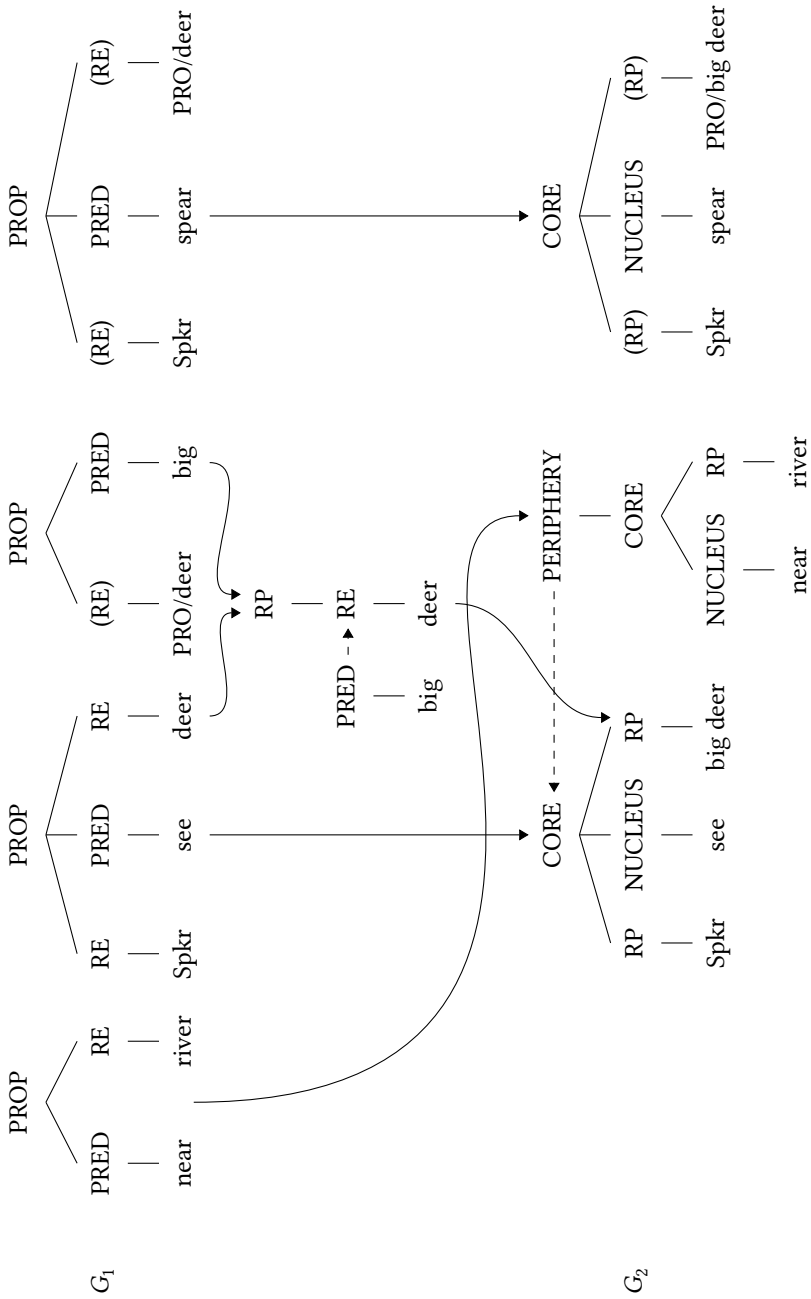


Figure 3: The transition from G_1 to G_2

Thus, Everett's proposals regarding the linguistic abilities of *Homo erectus* together with the well-motivated theoretical constructs of RRG yield important insights into how language began.

Abbreviations

IF	illocutionary force	PROP	propositional
NUC	nucleus	RE	referring expression
PrCS	pre-core slot	RP	reference phrase
PrDP	pre-detached position	SPKR	speaker
PRED	predicator	TNS	tense

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Chapter 4

Cohesive rhythms: Choral narrative in Ega

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This contribution combines functional, structural and acoustic explanations of a domain “beyond words”: how speech rhythm provides narrative cohesion and group bonding by caller entrainment with a choir during an orature event, recorded during fieldwork, in Ega, an endangered language isolate in south central Ivory Coast. The event in question is an oral narrative, a parable related in an interactive scenario by the village orator in two roles: as narrator, with feedback from his designated responder, and as caller in chanted interaction with the audience. First, a qualitative characterisation of coupling functions of speech rhythms is given, continued with a cyclical dynamic interaction model with specific rhythmical properties. The qualitative analyses are followed by a phonetic analysis based on annotation mining as a bridge leading to the quantitative signal processing of rhythm formants, and back to a qualitative interpretation of frequency properties of spectral peaks as resonant rhythm formants. While previous approaches have either selected functional or structural or acoustic analyses and representations of rhythm, the present study shows how these three approaches, taken together, can cohere and explain how speech rhythm patterns can be both physically grounded and functionally interpreted in context.

1 Speech rhythms in village orature

The present case study in the documentation and description of Ega, a language isolate of south-central Ivory Coast, is about the physical properties, structure and meaning of rhythms in interactive discourse. The study explores a communal orature event in Ega and its underpinnings in the rhetorical and poetic rhythms



of speech. The genre of the event is a parable told through interaction among *narrator-responder* and *caller-choir* participants, where the audience is the *choir*, and with variability of mutual rhythm entrainment in different phases of the narrative. The study relates closely to the emphasis in Sakel & Everett (2012) on contributions of multidisciplinary methods to fieldwork, dialogue description and documentation of language and culture.

The data consist of the sound track of the video recording of the parable, which is narrated by an accomplished orator, the *chef du village*, to his fellow villagers in the Ega-speaking region of Ivory Coast (Rossini & Gibbon 2011). The general framework for the case study is description and documentation of an endangered language (Gibbon et al. 2004), with the aim of understanding some of the discourse skills of the community by combining brief functional and structural accounts as background for a contemporary prosodic phonetic analysis, in search of both hermeneutic interpretation and causal physical grounding for speech rhythms.

The video data were recorded on 6th March 2001, during fieldwork on the Ega language in Gnieguédougou village, Ivory Coast, an enterprise which was cut short by the Ivorian civil war. The data are consequently rare and sparse, and quantitative phonetic analysis has therefore to be subordinated to qualitative interpretation and structural modelling, and to a final qualitative interpretation of the quantitative results in terms of initial assumptions and predictions. The direction taken in this contribution has the character of a travelogue about discovering discourse rhythms, their functionality, their structure and their physical form.

The language Ega (ISO 639-3 *ega*) is an endangered Niger-Congo tone language isolate in south central Ivory Coast. The endangerment of the language results from several factors, including the gradual failing of intergenerational transmission due to the dominance of French in schooling and influence from the enclaving Eastern Kru dialect related to Lakota Dida (ISO 639-3 *dic*) as spoken in neighbouring Divo.

Ega has been tentatively assigned to the Kwa subgroup (Bole-Richard 1983, Connell et al. 2002, Salfner 2004). A sceptical view is taken in Blench (2015, 2017) of the closeness of the language to other Ivory Coast Kwa languages or other neighbouring Niger-Congo subgroups. Ega community members in the social media see the Ega as autochthonous, surrounded by invading ethnic groups. Ega is regarded with some suspicion as a “secret language” by Dida speakers, whose name for the community and the language is Diès, a term resented by many Ega as a colonial invention. The outsider status of Ega in relation to other Ivory Coast Kwa languages is shown in Gibbon (2014), using unsupervised clustering of

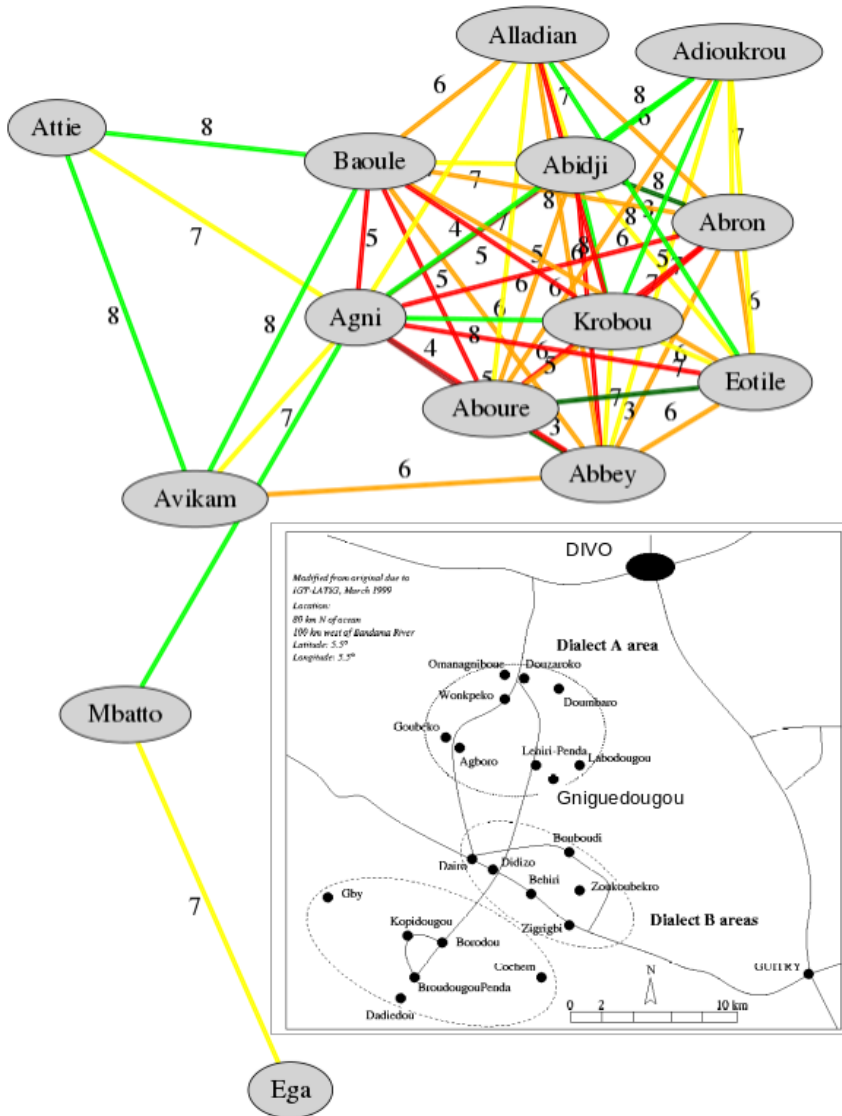


Figure 1: Unsupervised clustering of Ivory Coast Kwa languages derived from phoneme feature vectors (source: Atlas Linguistique des Langues Kwa, Héroult 1983). Colours and line lengths represent Hamming distances between phoneme inventory vectors. Insert: Sketch map of the Ega enclave in south-central Ivory Coast.



Figure 2: Recording scenario with narrator (right), responder (left) and audience (Gibbon 2023). (Video frame by permission of the participants.)

pairwise phoneme inventory distances between Kwa languages,¹ based on legacy data from the *Atlas des langues Kwa* (Hérault 1983). The visualisation in Figure 1 shows the linguistic isolate status of Ega, which matches its geographical and social isolate status as an enclave in a Kru area.

The orature session takes place in a typical Niger-Congo village scenario involving an orator with two roles: as *narrator* of the parable, with a designated backchannel *responder*, and as the *caller* who elicits *choir* responses from the audience in interactive chanted segments of the session, cf. Figure 2. The participant role relations can be summarised as follows:

[[[narrator-caller, responder], audience-choir], observer-fieldworker]

The parable concerns a faithless young bride-to-be who left her promised bridegroom and is on her way to the next village to marry her new-found future husband. She is fruitlessly warned by a twittering bird with a prophecy about her impending end at the hands of her formerly betrothed suitor, who had worked for free for her father to win her hand. Alas, the girl did not understand. The bride and the suitor met along the way, he reproached and threatened her, she spat at him, whereupon the prophecy came true. The parable closes with the moral that one should learn other languages like the fieldworkers are doing, meant as

¹<http://wwwhomes.uni-bielefeld.de/gibbon/DistGraph/distgraph-kwa.html>

a kind gesture by the orator. But the real linguistic punchline is more subtle: the birdsong, with *caller* and *choir* turns, is actually in Dida, the enclaving Eastern Kru language, not in endangered Ega.

The approach taken in the present study is to concentrate on this one orature session and explore the rhetorical and musical rhythms which characterise the interactive narration; for a more extensive treatment of similar orature, cf. Gibbon (2022a). In the following section, a brief qualitative linguistic outline of the functions and forms of the narration is given, as a basis for interpreting phonetically analysed rhythms. Section 3 continues with a hybrid qualitative-quantitative analysis of the speech recording annotated with turn-taking roles and Section 4 provides a detailed spectral analysis of the session in the frequency domain, following the principles of Rhythm Formant Theory. In Section 4, conclusions are drawn about orature rhythm, its functions, structure and forms.

2 The ubiquity of rhythms: function, form and sound

Rhythms are a key topic in many disciplines, not only in the study of music and speech, but in fields from astronomy and oceanography through econometrics to medicine, with cardiology and neurology in the forefront (Lewalter & Lüderitz 2010, Huang et al. 1998). The algorithms used in the analysis of temporal regularities and irregularities in these fields are closely related to the low frequency (LF) spectral analysis approach of this contribution, and equally closely related to the algorithms of popular smartphone song-recognition applications (Wang 2003). The concept of rhythm which underlies the present approach is also related to the account of expectations created by sequences of similar pitch accents in English (Dilley 2005) and can be formulated as follows, with *natural rhythms* understood in the traditional sense of temporally regular sequences of beats:

A natural rhythm is perceived, and can be measured, when a series of similarly structured events occurs with a specific frequency at approximately equal intervals in time and motivates a prediction of a further similarly structured event after a similar interval.²

²It may be helpful to note that the expectation created by a rhythm is conceptually related to the econometric concept of *Granger Causality* in time series, though without necessarily implying a rhythm: “A time series X is said to Granger-cause Y if it can be shown [...] that those X values provide statistically significant information about future values of Y”. (https://en.wikipedia.org/wiki/Granger_causality)

In speech, music and dance, however, rhythm has communicative functionality which goes beyond the physical patterns of natural rhythms and occasions *synchronisation* or *rhythmic entrainment* among the interlocutors (Cummins & Port 1998, Inden et al. 2001, Rathcke et al. 2021):

A behavioural rhythm fulfils the conditions for producing a *natural rhythm* and furthermore entrains perceptual synchronisation in its hearers.

In phonetics the main paradigm which uses this methodology is *speech modulation theory* (Ohala 1992, Traunmüller 1994, Todd & Brown 1994, O'Dell & Nieminen 1999, Barbosa 2002, Galves et al. 2002 pp. 323-326, Tilsen & Johnson 2008, Inden et al. 2001, Tilsen & Arvaniti 2013, Gibbon 2021), in which beat *frequency*, *magnitude* and *bandwidth* are the main properties of rhythms, and rhythms are generated by coupled oscillators and decoded in the frequency domain by a variety of spectral analysis algorithms. The present study uses a further development of Modulation Theory, Rhythm Formant Theory (RFA) (Gibbon 2021), in which rhythm-characterising low frequency (LF) spectral peaks between approximately 10 Hz and 0.01 Hz are interpreted functionally as LF rhythm formants, by analogy with the spectral peaks of high frequency (HF) phone formants. The relevant aspects of RFA are outlined in Section 3.

2.1 Linguistic, rhetorical and poetic rhythms

Phonological and phonetic descriptions of rhythm tend to be concerned with the very brief timing patterns of “linguistic rhythm”, in the words of Liberman & Prince (1977), of syllable, word and phrase sequences. In phonetic terms, the periods of such rhythms tend to be between about 100 ms for shorter syllables and 1 s to 3 s for phrases and sentences, corresponding to beat rates or frequencies between about 10 Hz and 0.3 Hz.

However, as studies in interactional linguistics show (Couper-Kuhlen & Selting 2018), much longer prosodic domains are called for in registers such as conversation, story-telling (as with the Ega parable), or speeches. For example, the duration of the complete Ega orature event is 300 s, and very long-term LF rhythms with rhetorical functionality span the entire event.

As a poetic feature, rhythm is a configurative indicator of cohesion, which can be present in different types and degrees with different functionalities throughout a poetic event (Wagner 2010). The poetic discourse of the selected parable calls for description in terms of the metalocutionary dimension of *coupling* (Levin 1973): the iteration of functionally relevant sound features which create

a layer of ritualised cohesion superimposed over the grammatical structure of locutions, by means of iterative *alliteration*, *assonance* and *rhyme*, and by the iterative patterns of performed rhythms, conventional metres, stanzas, refrains or the strophe-strophe-bridge-strophe patterns of popular music. Of all the available coupling parameters, only rhythm, as a variable iterative cohesion indicator, which may or may not align closely with grammatical structures or conventional metres, is picked out for discussion in the present case study.

The multiple long-term rhythms of discourse are referred to in the present context as *rhetorical rhythms* or *discourse rhythms*. Their multilevel intonation counterparts are sometimes referred to in the literature as “minor tone groups”, “major tone groups” (Trim 1959) and, in longer time intervals, as “minor paratones” and “major paratones” (Yule 1980). The “prosodic hierarchy”, a key theme in phonology, is only addressed indirectly in this study; hierarchical structure figures not in terms of a hierarchy of strict constituent inclusion (cf. the stress hierarchy of Chomsky & Halle (1968) and the prosodic hierarchy of Selkirk (1984), which are closely associated with grammatical structures, but in terms of a spectral scale of periodicities of different frequencies which are measurable in the physical speech signal.

2.2 Metalocutionary functions

The central question in the present section, which requires some background discussion, concerns rhythms as a meaningful component of utterances. A related and more frequently discussed issue is the analogous question of speech melodies as meaningful components of utterances, though these are not discussed here. The starting point for capturing the meanings of rhythms is the traditional concept of parallel information channels in spoken language: the rhythms and melodies of prosody, and the sounds, syllables, words and phrases of locutions. The parallel channel approach has been shared by most traditional intonation and stress textbook models for more than a century (for overviews see Gibbon (1976) and Arvaniti (2022)), by speech engineering approaches (Morikawa & Fujisaki 1976, 't Hart & Cohen 1973, Dutoit 2001, Fujisaki 2004) and, in phonology, with the exception of Chomsky & Halle (1968), by the popular metrical and autosegmental models (Barnes & Shattuck-Hufnagel 2022).

The core meanings of locutions are propositional (Austin 1962), while the core meanings of prosodic forms are metalocutionary (Gibbon 1976, 1980) and refer indexically to semantic features of the utterance and pragmatic features of the utterer:

- *Semantic metadeixis* with respect to utterance locutions, in that rhythmic beats and beat sequences (such as pitch and duration accents, nuclear, contrastive and emphatic accents and sequences of these) *denote* (or: *refer to* or *point at*) the real-time physical temporal locations of locutionary components which relate to information structure (as deictic gestures point in real-time and real space at components of the environment). Traditional terms for these metadeictic functions are *culminative* for accents, *configurative* for rhythmic and melodic groups, and *delimitative* for boundary effects.
- *Pragmatic indexicality*, with respect to the utterer, with interpersonal indications of attitudes, emotions, beliefs, turn exchange control, as characteristics of the speaker such as sex, age, health, social status, physical and social proximity or distance (Gibbon 1976, Hirschberg & Pierrehumbert 1986).

Pragmatic indexical metalocutionary meanings are also conveyed in the locutionary channel itself by subjective adverbs, discourse particles and other lexical items with appraisive pragmatic features.

Lexical prosodic forms, such as phonemic and morphosyntactic tones and lexical pitch accents, have direct contrastive and syncategorematic functions and are tightly synchronised with head syllables of locutionary lexical items. In addition to some direct influence from segments, particularly consonants, tonal forms are partly independent of syllable components in the sense that they may undergo sandhi processes such as terracing and tonal assimilation without reference to locutionary properties (Gibbon 1987, Jansche 1998). These lexical prosodic conditions also characterise the three phonemic and morphosyntactic level tones of the Ega language (Connell et al. 2002).

Supralexical prosodic forms such as metadeictic pitch accents and intonation boundary and contour patterns are less tightly synchronised with the heads of the items they denote. Accentual and boundary features, also in tone languages such as Tem (Togo, ISO 639-3 *kdh*) (Tchagbale 2001), Ega, and also Mandarin (Duanmu 2007), may include speech tempo deceleration with syllable lengthening, pitch upstep and downstep and increased pitch range, not only lexical tonal features such as relative pitch height, pitch range and pitch contour. In Ega narration, multimodal features of manual gesture, facial gesture and posture, as shown in Figure 2, are part of the metalocutionary complex (Rossini & Gibbon 2011), but are not within the scope of the present contribution.

Speech rhythms in the Ega parable distinguish different kinds of prosodic cohesion in the narrative exchanges between the *narrator* and his *responder* on

the one hand, and interactive choral exchanges between the *caller* and the *choir*, on the other hand, as the acoustic phonetic analyses in the following sections show. The rhythms of the narrative exchanges have the function of structuring cohesive propositional groups, while the rhythmic entrainment of the choral exchanges has an additional function of creating explicit backchannel agreement and social bonding among the participants along a functional scale between mutual entrainment to shared rhythmic patterns at the one end of the scale and relatively arrhythmic patterns at the other end. The present data tend to show the former case, with cohesive rhythms associated with narration structure by an individual speaker, and with entrainment and bonding created by rhythms contributing to the aesthetic and social value of interactive choral chanting.

2.3 Cyclic models for multiple rhythms

The aim of the present section is to present a viable *dynamic functional model* for the static participant model of [[[*narrator-caller, responder*], *audience-choir*], *observer-fieldworker*], in the sense that transitions between states in the dialogue are modelled, and map to long-term rhythms in the interlocution.

In structural terms, a rhythm is an iterative sequence of beats or waves which can be modelled by an iterative finite machine (finite state automaton, FSA), with multiple cycles, and which functions as a system of coupled oscillators (Cummins & Port 1998, O'Dell & Nieminen 1999, Barbosa 2002). A little justification for using cyclical linear models is in order, since more powerful but less realistic models tend to be preferred in linguistics. However, many grammatical word and phrase structures are right-branching or left-branching, indicating that in computational formal grammar terms a fully centre-recursive Chomsky Type 2 (context-free, phrase structure) grammar is not necessary, and that a Type 3 (linear, regular) grammar is sufficient.

Type 3 recursion can be efficiently implemented as iteration with linear time and finite memory space; this is not the case with general Type 2 languages. This has been clearly demonstrated in the “recursion debate” (Karlsson 2010). General recursion as a property of thought rather than language is proposed by Everett (2017); other scholars (overview in Gibbon & Griffiths 2017) suggest that communication with the more general recursion of thought was made possible partly by ritual rehearsal and (as in mathematics) by the invention of writing, which allows for extended processing time and permits the use of additional memory space in external media.

Another key difference is that for any Type 3 grammar there is an equivalent FSA (or FS transducer, FST, if tree output is also desired), with realistic processing properties of finite memory and linear time as a function of the size of the

input. Cyclical FSAs are ubiquitous in many areas of computational linguistics, especially in morphology, phonology and prosody. It is assumed here, until the contrary is demonstrated, that FSAs and FSTs are the formal devices needed for the description of rhythms.

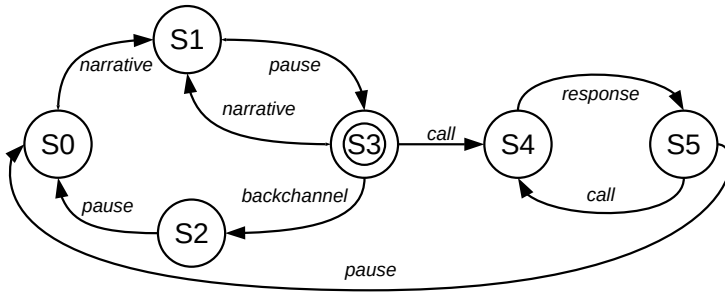


Figure 3: Sketch of discourse grammar for interactive orature as an iterative transition network.

The cyclical linear grammar approach suits the Ega parable very well. In Figure 3 the iterative dialogue flow is represented in transition network format, showing inclusive cyclic patterning, i.e. a system with one cycle fully enclosed inside another (there are many other types but they do not apply in this context). The sequence of iterations can be formalised by means of a weakly equivalent right branching (or left branching) linear grammar.³ However, the transition network format of Figure 3 has greater heuristic visualisation value than grammar rule notation. An equivalent, though less perspicuous, regular expression can also be formulated.

The iterative linear grammar with inclusive cycles is realistic in real time and real memory space. It does not seem necessary to model the development of dialogues like this as becoming ever more deeply embedded and requiring ever more nonlinear increases in time and memory space as the dialogue unfolds. The formal parallels between the dialogue model and cyclical prosodic models suggest that this dialogue grammar should be interpretable physically as a rhythm generating or accepting oscillator with inclusive cycles at multiple frequencies, which

³Right-branching Type 3 grammar ('|' for options):

$S_0 \rightarrow \text{narrator } S_1$
 $S_1 \rightarrow \text{pause } S_3$
 $S_2 \rightarrow \text{pause } S_0$
 $S_3 \rightarrow \text{narrator } S_1 \mid S_3 \rightarrow \text{backchannel } S_2 \mid S_3 \rightarrow \text{choir } S_4$
 $S_4 \rightarrow \text{response } S_5$
 $S_5 \rightarrow \text{choir } S_4 \mid S_5 \rightarrow \text{pause } S_0$

relates easily to cyclical linear models of prosody. This is to be demonstrated in the following sections on the phonetics of rhythm.

The including and included iterations have flexible assignments to locutions, especially in the *narrator* turns and do not necessarily constitute a “strict layer hypothesis” (Selkirk 1984). Four inclusive cycles are defined by the FSA:

1. Local (included) cycles:

narrative-pause cycle within a narrative turn;
narrator-responder cycle, with pauses, intervening within a narrative turn;
chanted *caller-choir* cycle, complementary to the *narrator-responder* cycles;

2. Global (including) cycle with restart of local cycles:

pause return to *narrative* start state.

Cycles, of the kinds shown in Figure 3 do not necessarily reflect a rhythmic pattern; the inverse is true, however: a rhythmic pattern is expressible as a cycle, given additional time and frequency constraints. The *caller-choir* cycle in particular is a strong candidate for resonant oscillation with the coupling function of rhythm, as a chanted or sung context of *caller-choir* pairs.

The task facing the following annotation-based phonetic analysis and acoustic phonetic signal processing is to investigate the empirical physical grounding for the different rhythms in the *narrator-responder* and *caller-choir* sequences.

3 An inductive approach: Rhythmic modulation

3.1 Phonetic preliminaries: Annotation mining

A set of informal descriptive hypotheses are formulated:

H0: The presence of rhythmic beats cannot be identified in the speech signal.

H1: The presence of rhythmic beats can be found by visual inspection of the waveform of the speech signal.

H2: Rhythmic beats can be statistically described.

H3: Rhythmic beats can be dynamically modelled in the time domain.

H4: Rhythmic beats can be modelled in the frequency domain.

H0 represents a sceptical view sometimes found in the literature. The hypotheses H1 to H3 represent goals for the time domain annotation-based analysis and are not incompatible but increasingly detailed. H4 represents the perspective of Rhythm Formant Analysis, to be addressed in the next section. The hypotheses are necessarily informal in view of the tiny data. H1 is immediately confirmed by inspection of Figures 4 and 5 mid panel, and Table 1 provides simple evidence for H2. H3, H4 and H5 are argued for below (cf. Figures 5 and 6).

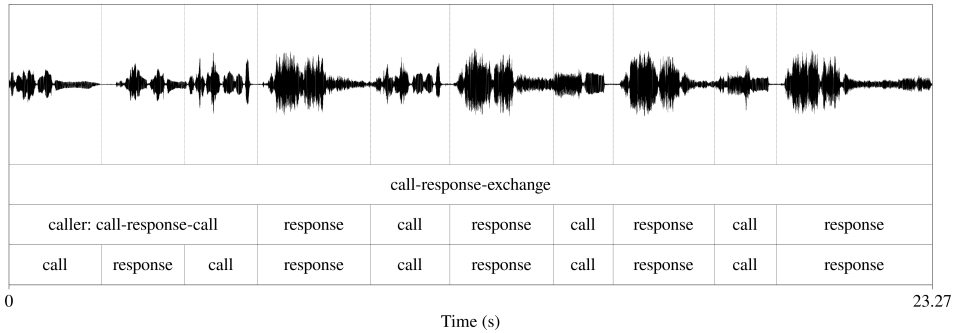


Figure 4: Segment of Ega orature event annotation comprising *caller-choir* iterations (posterised Praat screenshot).

Table 1: Rhythm unit durations and periodicity of *caller-choir* categories

Item	caller-d	caller-p	choir-d	choir-p	caller-choir-d	caller-choir-p
cr1:	2.337	0.428	2.08	0.481	4.417	0.226
cr2:	1.852	0.54	2.838	0.352	4.69	0.213
cr3:	2.004	0.499	2.612	0.381	4.616	0.217
cr4:	1.501	0.666	2.549	0.392	4.05	0.247
cr5:	1.565	0.639	3.931	0.254	5.496	0.182
mean:	1.852	0.54	2.802	0.357	4.654	0.215
SD:	0.305	3.279	0.616	1.623	0.476	
CoV:	0.165	6.061	0.22	4.545	0.1	

The recording was annotated with participant turns, combining qualitative decisions on descriptive categories with time-stamped intervals in which these categories occur (see Figure 4 for a segment of the annotation). Results of the annotation and a basic descriptive analysis are shown in Table 1. The low coefficients of variance for the sequences, together with the alternating durations between the *narrator-responder* and *caller-choir* turn exchanges, are simple time domain indications of long-term rhythmic properties.

3.2 Modulation

The abbreviations FM (frequency modulation) and AM (amplitude modulation) are familiar from radio broadcasting. In general, FM broadcasts use an HF carrier signal of around 100 MHz, modulated by lower frequency audio modulation up to 15kHz, and AM broadcasts use carrier frequencies below 30 MHz with more restricted audio modulation frequencies of 550 to 1720 Hz. The terms FM and AM can apply in any frequency range and are used here advisedly rather than “F0 estimation” in order to emphasise the general theoretical basis for the acoustic speech analysis and to underline the parallel AM and FM channel structure.

Modulation is the modification of a higher frequency carrier signal with a lower frequency information signal. The basic speech carrier signal is the fundamental frequency (F0) produced by phonation in the larynx, but whispering and obstruent noise are also carrier signals. The carrier signal is complex and in the case of F0 at the frequency f also contains harmonics at integer multiples of f : $f, 2f, 3f, 4f, \dots$

The speech carrier signal is modulated directly at source in the larynx by lower frequency FM information signals (tone, pitch accent, intonation) and indirectly by AM air pressure variation (loudness) and oral-nasal filtering (consonants, vowels, certain voice quality features). The speech carrier frequency has an order of magnitude of 10^2 , between about 60 Hz and 500 Hz, depending on gender, age, emotionality and other factors. Low frequency modulations have much lower frequencies, with 50 Hz or less for consonants, around 10 Hz for vowels, 4 Hz for syllables, 1 Hz for words, 0.3 Hz for phrases, very approximately, depending on individual habits, speech rate, style, register and genre. Frequencies of the discourse rhythms of rhetorical structuring and turn-taking are considerably lower, about 0.1 Hz to 0.01 Hz.

FM and AM in speech have specific properties and also different roles in communication: noise, for example, is essentially amplitude modulation. Frequency modulation is less sensitive to noise, explaining why, in teleglossia, communication at a distance, the speech rhythm and melody of a speaker, for example

in the next room, can often be followed, but not the speaker's words, and why whistling, horns and melodic drums are used for teleglossic communication in many parts of the world.

3.3 Rhythm Formant Theory (RFT): Spectral patterning

Rhythm Formant Theory is a further development of Speech Modulation Theory and addresses the task of the hearer (or the phonetician, or the automatic speech recognition application), in demodulating and then analysing the structure of the speech signal. The underlying idea in RFT (Gibbon 2018, 2021, 2022b) is that speech rhythms are not only identifiable as intervals between beats or as beat rate per second in the time domain, but also, and in more detail, as low frequency spectral magnitude peaks, the *rhythm formants*. Over the entire spectral frequency range there are magnitude peaks at specific frequencies, in particular the peak frequencies of the LF AM and FM rhythm formants, the MF (medium frequency) carrier signal frequency (F_0), and the HF peak frequencies of the harmonics of the carrier signal and of the HF phone formants as modulations of the harmonics.

The term *formant* emphasises that, except for the different frequencies, the acoustic definition of LF rhythm formants as spectral peaks is the same as for the HF phone formants which constitute consonants and vowels, which are spectral peaks above and below 1 kHz. The LF rhythm formants are spectral peaks above and below 1 Hz, with frequencies between 0.3 Hz and 10 Hz (periods of 3 s to 100 ms) for the “linguistic rhythm” (Lieberman & Prince 1977) of syllables, words and phrases, and below approximately 0.3 Hz (periods of 3 s or more, in the present analysis 300 s) for rhetorical or discourse rhythms. Rhythm formants in both FM and AM LF spectra are relevant for rhythm analysis (Gibbon 2018, 2021, 2022a,b), but in the present contribution attention is restricted to the AM rhythm formants.

The Rhythm Formant Analysis (RFA) procedure associated with RFT processes the LF information-carrying speech signal, Figure 5 top panel, in 5 steps. First, the AM is demodulated, in the present analysis by taking the absolute values, i.e. full-wave rectification, of the signal. Second, the rectified signal is low-pass filtered at 10 Hz; the low-pass filtered rectified signal can be seen as a possible acoustic correlate of the phonological sonority curve, Figure 5 mid panel. Third, the low-pass filtered rectified signal is analysed by Fast Fourier Transformation (FFT). Fourth, the resulting low frequency spectrum is analysed for rhythm formants, i.e. high magnitude peaks in the spectrum and, fifth, the bandwidth of the formants determines the degree of rhythmicity or resonance at the formant

frequency f_{formant} . Similarly, the FM envelope is demodulated and analysed, Figure 5 bottom panel.

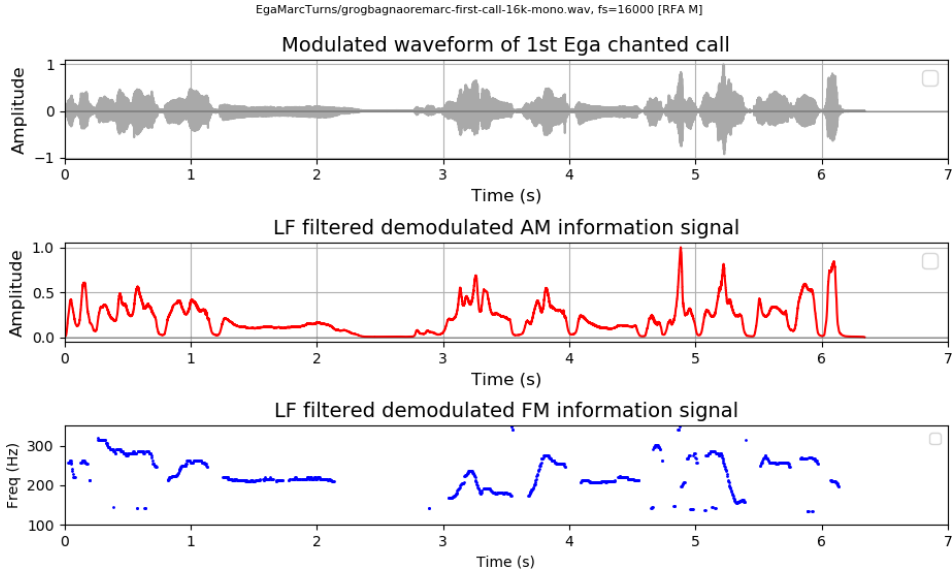
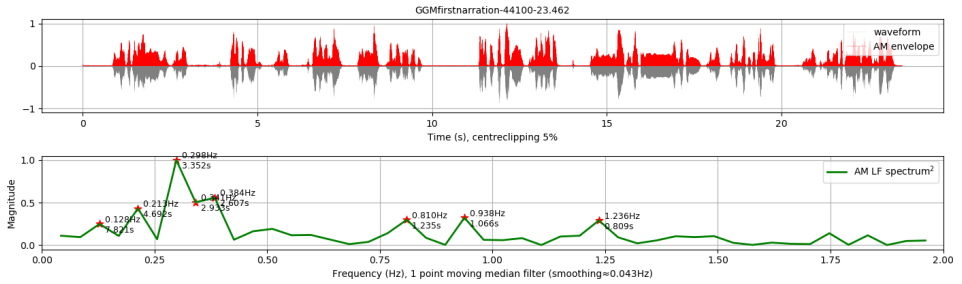


Figure 5: Modulated waveform (upper panel), demodulated LF AM information signal (centre panel), demodulated LF FM information signal (lower panel).

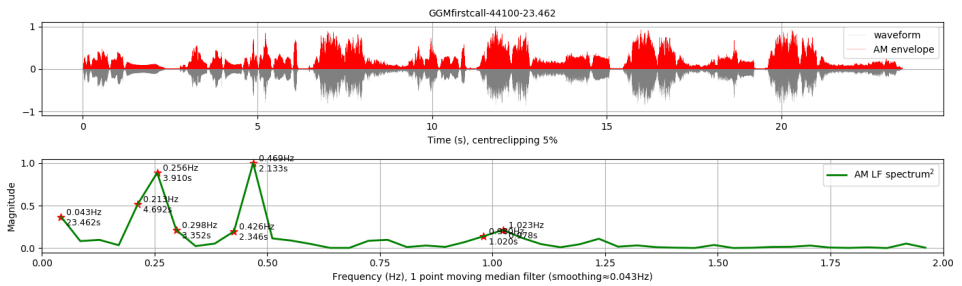
The RFA procedure is applied to the *caller-choir* iteration extract, shown in Figure 4 and the result is visualised in Figure 6. The top panel of Figure 6 shows the waveform and the demodulated AM envelope in the time domain, as outlined in the previous section. The demodulated AM envelope is then transformed by applying the FFT in a window covering the whole *caller-choir* sequence.

Panel A (top) of Figure 6 shows a single, relatively broad rhythmic range between 0.25 Hz and 0.5 Hz, which indicates *narrator* turns of varying length. The smaller peaks above 0.75 Hz relate to shorter phrases, words and syllables of turns, and backchannel turns of the *responder*, which are standardly *sese*, roughly meaning ‘Aha!’, though other particles of astonishment or disbelief occur.

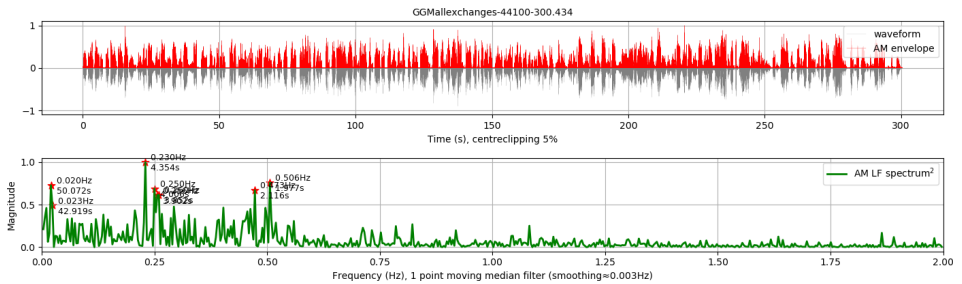
It is also clear on visual inspection of panel B (centre) of Figure 6 that there are two main rhythm formants in the choral chanting, each with narrow bandwidth and thus high rhythmicity or resonance, neither of which covers the same spectral range as the narrative rhythm formant in panel A. The result for the *caller* and *choir* sequences is particularly interesting. Using the annotation time-stamps, it was predicted that a spectral frequency of 0.54 Hz can be found for



(a) narrator-responder exchanges following first caller-choir exchanges (23 s FFT window)



(b) First caller-choir exchanges (23 s FFT window)



(c) Complete parable (300 s FFT window)

Figure 6: RFA: upper panels, waveform and amplitude envelope; lower panels, long-term spectrum of the selected event with conspicuous rhythm formants and smaller higher frequency formants.

the *caller* turns, 0.357 Hz for the *choir* turns and 0.215 Hz for the combined *caller-choir* exchanges, noting that there are outliers in each case. What is found is, in fact, rather close: in the centre panel of Figure 6 is a broader formant around 0.256 Hz and a narrower formant around 0.469 Hz. The first of these includes both the predicted 0.215 Hz and 0.357 Hz frequencies, with *caller* and *choir* merging into one formant region, while the second borders on 0.54 Hz, for the combined *caller-choir* exchanges. The result is approximate, and would need more detailed analysis, but tendentially the prediction H4 is borne out. That the procedure is generalisable to syllable-sized and word-sized rhythms has been shown elsewhere (Gibbon 2021).

Moving on to panel C, the fragment just discussed is not the only such sequence in the story-telling event, and it is fair to predict that the same rhythm formants will be found in other *caller-choir* occurrences in the parable. In fact, if these *caller-choir* sequences are prominent enough, they will be visible in the low frequency spectrum for the entire event in spite of the wide range of other low frequency spectral frequencies, leading to a more specific hypothesis:

H5: The conspicuous rhythmic spectral properties of the first *caller-choir* sequence are repeated in the later sequences, and are sufficiently prominent to be visible in a spectral analysis of the entire story-telling event.

The result of the holistic low frequency spectral analysis is shown in panel C (bottom) of Figure 6. The predicted frequencies are still plainly in evidence in the spectrum of the entire utterance, and, informally, H5 is supported. The spectrum is more ragged, noisier, (or more precisely: more detailed), because of some frequency variation between the different *caller-choir* exchanges, because of the presence of other frequencies from the *narrator-responder* exchanges and because of the much higher spectral resolution resulting from use of a longer FFT time window of nearly 5 minutes.

Taking up the concepts introduced in the discussion of discourse functionality in linguistic stylistics, it is suggested that the regular low frequency rhythms which are detectable in the spectrum are major coupling forms in orature, specifically in the *caller-choir* exchange sequences, with several coupling functions: first, to identify the *caller-choir* type of dialogue act; second, to bind the two parts of the dialogue act syntagmatically; third, to identify instances of coherent sequences of *caller-choir* pairs. This perspective is motivated by previous work on other long-term spoken events such as news-reading, story-reading and poetry recitation (Gibbon 2021, 2022a,b).

3.4 Combining frequency and time domains: The LF spectrogram

The information provided by the long-term LF spectrum is solely in the frequency domain, a generalisation over the whole temporal extent of the story-telling. The spectrum provides no information about possible variation of rhythm formants in time. For additional temporal information, a long-term LF spectrogram containing a sequence of many shorter spectral slices is needed, in which all of the *narrator-responder* and *caller-choir* exchanges can be placed. Figure 7 shows an annotated segment, which includes 8 s of following context for the *caller-choir* exchange, required by the 8 s long FFT window required by the low frequency analysis, and the low frequency spectrogram for the segment.

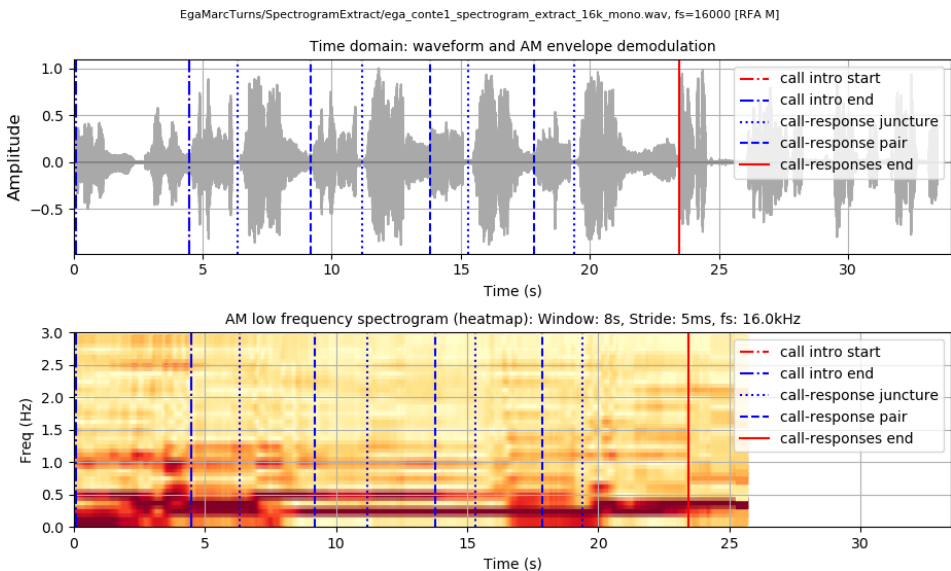


Figure 7: Low frequency spectrogram of the first *caller-choir* sequence of the Ega parable (empty trailing context due to stride length; slight misalignment due to stride rounding).

The sampling frequency of 16 kHz and the spectrogram spectral slice window of 8 s provide adequate frequency resolution for the expected frequencies at around 0.2 Hz (period 5 s) and 0.4 Hz (period 2.5 s). However, the 8 s FFT window comes at a cost, namely very low temporal resolution. This low temporal resolution is compensated by extremely short window strides of only 5 ms. The long FFT window requires the inclusion of at least 8 s of following context in order to capture the whole final signal segment, leaving a blank space in the spectrogram after the last full 8 s window, corresponding to the duration of the FFT

window. The final spectral slice appears at the beginning of the blank interval. However, the massively overlapping stride interval of 5 ms, which is extremely short in relation to the duration of the 8 s spectral slice window, recoups the lost time resolution. The very slight misalignment of the spectrogram bars and the waveform bars in the visualisation is due to uncorrected decimal rounding of the stride duration, but is not important for the present purpose.

The vertical boundary bars in Figure 7 mark the boundaries of exchange types, and are derived from the annotation shown in Figure 4. In the spectrogram the two expected rhythm formants at around 0.2 Hz and 0.5 Hz are clearly visible in the central section as two distinct narrow bandwidth horizontal bars, which merge into a broader bandwidth horizontal bar in the final section, and correspond to the rhythm formants shown in Figure 6b. The *caller* turns are shorter than the *choir* turns and the first and fourth *caller* turns do not share this degree of rhythmicity or resonance with the choir. The third *choir* turn is also somewhat irregular.

The positions of the two *caller-choir* rhythm formants in the timeline are very clear, in contrast to the *narrator-responder* formant. The two rhythm formants are in a quasi-harmonic octave relationship to each other, one being twice the frequency of the other, due to the binary structure of the *caller-choir* grouping. The quasi-harmonic two-formant pattern can be interpreted as a linearly organised prosodic hierarchy, represented by cycles in the oscillator automaton in Figure 3, along the lines of coupled oscillator theory.

So far, the spectrogram of just one *caller-choir* sequence was discussed. A more ambitious validity check is on whether the pattern is repeated in the two following similar *caller-choir* sequences, in which varying *caller* content occurs, but with the same *choir*. The low frequency spectrogram for the entire parable is shown in Figure 8, with narrative and chant episodes marked. The time window for each spectral slice is again 8 s, with the same number of equally spaced strides, but here each stride is 58 ms in duration, due to the 5 min FFT window. Figure 8 starts with the *narrator-responder* cycle, followed by the *caller-choir* chant cycle, *narrator-responder* cycle, *caller-choir* chant cycle, *narrator-responder* cycle, *caller-choir* chant cycle, *narrator-responder* cycle, closing with the moral and the narrator's name. The boundary time-stamps are given in Table 2 in order to facilitate checking with Figure 8.

In summary, the expected dual rhythm formant bars for the three *caller-choir* exchanges are present, at about 30 s, 180 s and 220 s, and provide further informal support for the hypotheses H2, H3, H4 and H5. The intervening *narrator-responder* alternations are complementary to the *caller-choir* cycles, and can be seen to have their own entirely different shorter-term timing properties. It is beyond the scope of the present study to investigate other details. It is sufficient

Table 2: Annotation of the complete parable with narrative and chant episodes

Tier	Label	Start	End	Duration
Episodes	_	0.000	0.146	0.146
Episodes	Narrative	0.146	16.684	16.538
Episodes	Chant	16.684	42.463	25.779
Episodes	Narrative	42.463	175.125	132.662
Episodes	Chant	175.125	197.426	22.301
Episodes	Narrative	197.426	212.238	14.812
Episodes	Chant	212.238	231.901	19.663
Episodes	Narrative	231.901	297.499	65.598
Episodes	_	297.499	300.434	2.935

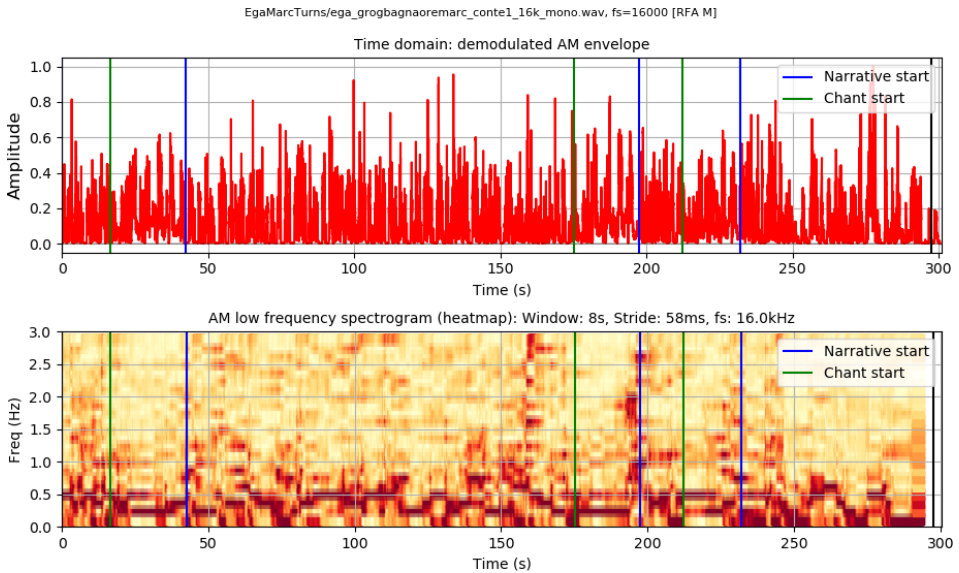


Figure 8: Long-term LF spectrogram of the entire Ega parable; window size 8 s, strides 0.75 s, with episode (narrator and chant turn) boundaries.

for the present study to point out that there are in fact clear spectral differences in segments of the discourse, that they evidently relate to different kinds of turn in the interaction, and that pessimism about identifying rhythm in the physical signal, at least in its discourse functions, is not justified.

4 Summary and conclusion

This contribution explores functional, structural and physical properties of speech rhythms in the interactive narration of a parable in the endangered Ega language, south central Ivory Coast, recorded during a fieldwork project, a small and rare example of traditional orature. The village orator has two roles, as *narrator* of the parable with feedback from his designated *responder*, and as *caller* in chanted interaction with a responding audience as *choir*. The methodology is an interdisciplinary combination of qualitative and quantitative analyses. Relevant functions of speech rhythm are characterised as coupling, a concept from linguistic stylistics, and iterations of matching functional and structural cohesion in the temporal rhythm domain are described with a cyclical linear model. Annotation analysis provides a hybrid qualitative-quantitative bridge to acoustic phonetic signal analysis, and finally spectral peaks which distinguish between different turn and exchange types are interpreted as rhythm formants with a high level of rhythmicity or resonance. The signal processing procedure is based on *Rhythm Formant Theory* and its associated *Rhythm Formant Analysis* methodology and provides physical grounding for rhythm theories, embedded in a background of poetic culture.

In conclusion, it was shown that functional, structural and causal physical accounts can combine to explain the overall picture of how speech rhythms work in interactive orature: *discourse rhythms* have a *coupling* function for the accompanying locutions, are given hermeneutic motivation by functional analysis, and are structured by means of an *iterative linear oscillator* (in the formal grammar sense of “linear”), and realised with *causal physical grounding* as *rhythm formants* with specific frequency, amplitude and bandwidth properties.

Further applications in discourse analysis and in other disciplines such as automatic spoken language analysis, recognition and identification, as well as in spoken language system evaluation and L2 fluency assessment are anticipated. Not all rhythms, whether in syllable, word, and phrase time domains or in the long time domains of discourse turns, can be detected all of the time. But it helps to look in the right places.

Acknowledgments

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Chapter 5

Why is UG such a hard question?

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UG presents a hard problem for scholars. Here, I ask why the question of innate rules of language is so difficult to settle. The problem, I suggest, does not lie in the innateness question itself—whether knowledge, of language or otherwise, is innate knowledge, is a perfectly coherent question. And yet, “innate knowledge” is a notion that is difficult for us to grasp. New experimental evidence suggests that laypeople are systematically biased to presume that knowledge only arises from experience, and this Empiricist bias is rooted in core cognition (e.g., Berent et al. 2022). These results open up the possibility that our troubles with UG arise from this very bias. Whether linguists are indeed biased, and whether these attitudes are anchored in core cognition is unknown. But the possibility that our troubles with UG have innate origins merits close attention.

I am very pleased to offer this essay in celebration of the life and work of Dan Everett. Dan was my linguistics professor at Pitt. Although he was not my advisor, nor was linguistics my major, Dan left a lasting mark on my intellectual development. With his inexhaustible fervor, sharp wit and piercing questions, the redhead professor left us students in awe, silent, dazzled, and a bit frightened.

What was so impressive about Dan wasn't his command of formal theories (at the time, it was autosegmental phonology) – those theories come and go. Rather, Dan saw language as a window into human nature, and he invited us, students, to lean forward and take a peek. So, it is only befitting that, in tribute to my teacher, I broach that subject here.

The topic of my piece is Universal grammar (UG) – the hypothesis that the human capacity for language arises from innate knowledge of linguistic principles (Chomsky 1965). Since UG concerns what's innate in humans, it addresses human nature. But as Dan explained at the time, UG articulates a well-defined scientific



hypothesis that is amply amenable to empirical scrutiny.¹ And yet, nearly sixty years past Chomsky's *Aspects* (Chomsky 1965), the question of whether UG exists (hereafter: the UG question) remains as contentious as ever. Arguably, it's one of the hardest questions in cognitive science.

In this piece, I won't take sides on the UG debate, and I certainly won't seek to settle it. My goal is not to determine whether innate knowledge of language exists. Rather, I ask why the UG question is so difficult for science to settle.

To foreshadow my conclusions, I don't believe that the problem is with the notion of innateness nor do I think the problem is specific to the inquiry into "innate knowledge of language". Rather, I suggest that the problem is with the inquirer.

Supported by recent experimental findings from my lab, I will show that humans are systematically biased in their reasoning about all forms of innate knowledge, UG included. It is these biases, I believe, that render UG a particularly difficult question.

1 "Innateness" is a perfectly coherent question!

Doing science is hard – that much goes without saying. But questions about the mind, especially those concerning innateness, are extra difficult. Debates about innateness just don't go away, and this can be frustrating. For some, the question of innateness seems incoherent (Mameli & Bateson 2011).

I don't think it is. Cognitive innateness, of course, does not lend itself to definition by a set of necessary and sufficient conditions. But so do many other human concepts. And yet, we use such concepts in science, and make good progress. The fact that "game", for instance, cannot be defined (Wittgenstein 1953) has hardly stopped the blooming field of game theory (e.g., Nowak & Krakauer 1999). So, I don't think our troubles with innateness arise from the lack of definitions.

Concerns with innateness also cannot be obviated by the insights from genetics. Critics note that genes and environment interact, and this of course is true of all biological traits (e.g., Ridley 2003). Still, some biological traits emerge spontaneously among members of the species (e.g., having two hands) and others (e.g., a scratch, a severed limb) do not. It is perfectly coherent to ask whether a given trait is largely heritable – is it more like having two hands or a scratch?

To make progress, however, questions about innateness ought to be formulated at a specific level of analysis (Samuels 2004). Although we all agree that

¹The question of "innate knowledge" as discussed here, is amply amenable to empirical scrutiny. As such, the "innateness question" is distinct from the debate regarding merits of Rationalism as a method of inquiry (in philosophy, Rationalism has been frequently invoked to argue for a priori knowledge).

having two hands is an innate feature of humans, this trait (like all others) develops; the human zygote obviously has no hands, yet all healthy human embryos do. By the same token, UG is a cognitive trait, so when we consider its innateness, we ought to explicate it within the cognitive level of analysis. As Samuels (2004) notes, some cognitive traits are the product of cognitive mechanisms, whereas other cognitive traits are not – they are cognitive primitives. The knowledge that “Paris is the capital of France” is obviously the product of learning, but other concepts, such as what is an “object”, arguably aren’t and, as such, are good candidates for “cognitive primitives”. Innate cognitive traits, then, are cognitive primitives; these are cognitive traits that emerge spontaneously in the normal course of development, but they are not the product of other cognitive mechanisms.

Viewed in this manner, the UG question is straightforward: is UG a cognitive primitive, or does it emerge from other cognitive mechanisms – most notably, learning from experience? The answer can be either “yes” or “no” – either UG exists, or it doesn’t. But there is nothing wrong with asking: the question is logically coherent.

And yet, the notion of UG strikes us as “funny” – it doesn’t quite “compute”. But, as we will see next, that sense of unease applies to the notion of innate knowledge, generally – it is not specific to UG. “Innate knowledge” is a notion that is extremely difficult for people to comprehend. The concept of innate knowledge – of any kind – simply strikes people as an oxymoron.

2 Innate knowledge – what a “funny” notion!

When laypeople – adults and children – are asked to evaluate the origins of knowledge, they are systematically biased to assume that knowledge arises from experience. This is the case across multiple instances of knowledge, across multiple manners of probing, and when people consider knowledge of different creatures – humans, animals and even aliens (Berent et al. 2019, Wang & Feigenson 2019).

For example, when asked to evaluate which psychological trait would likely emerge among infants who are raised on a “desert island”, people assert that knowledge will not emerge spontaneously, even when the notions in question are ones that have been documented across cultures, and thus, plausibly innate (e.g., “keeping track of time”, “logical negation”; Berent et al. 2019). The same is obtained when people are asked about the knowledge of infants (e.g., that objects are cohesive) and animals (e.g., the structure of a swamp sparrow’s song),

and when innateness is gauged indirectly, by asking people to predict the onset of traits in development (Berent et al. 2019). People reject that knowledge is innate, and they tend to believe it emerges late in development, even when the traits in question are demonstrably present in young infants or at birth (Wang & Feigenson 2019).

This is not because people uniformly reject all forms of innateness. In fact, when asked the same about other aspects of the psyche – about sensations, motor skills and emotions – people have no problem assuming that these capacities are innate and early emerging (Berent et al. 2019). In fact, they are positively biased to assume that emotions are innate, and they manifest this bias even when they are explicitly told that the emotions in question are learned (Berent et al. 2020b). It is specifically the notion of innate knowledge, then, that seems “funny”. And this is also demonstrably so when people are asked about innate knowledge of language.

In one study, we asked people to weigh in on the origins of language structure (Berent et al. 2019). Participants were presented with two matched vignettes (Figure 1a; emphases are added). Each such vignette presented an explanation for linguistic structure. One explanation attributed structure (specifically, syntax) to abstract rules (simplified, for the lay readers); another attributed structure (syllable structure) to articulatory pressures. In both cases, people were told that the structural regularity in question develops spontaneously, without learning (i.e., innate). Next, we asked participants to evaluate whether these traits will emerge in a “desert island” scenario – among a group of children that are fully cared for, but have had no opportunity to observe language in others. People considered syntactic rules as less likely to be innate (i.e., to emerge spontaneously) than articulatory motor plans (Figure 1b).

Our troubles with innateness, then, are selective: people are biased to assume that knowledge cannot be innate. And if people reject innate knowledge, then it stands to reason that the notion of innate knowledge of language ought to be difficult for people to grasp.

3 Why do we shun innate knowledge?

To understand the scope of our troubles with UG, it is worth considering why people are biased in this particular fashion – why they reject innate knowledge. The “why” question matters because, earlier, I’ve suggested that some scientific proposals are inherently difficult for the human mind – they are hard because they violate principles of core knowledge.

Intuitions about language

Sentence structure

When people come up with descriptions of actions, they are more likely to form sentences with certain word sequences as opposed to others. For example, people are more likely to make sentences like “dogs bark” than “bark dogs”.

This is the case in most languages of the world, including English. Furthermore, **studies have shown** that infants prefer listening to the “dogs bark” over the “bark dogs” pattern. These results suggest that children **develop this preference spontaneously, without learning**.

Scientists think that patterns like “dogs bark” are more frequent because their **abstract structure is simpler**. When people form word patterns, their brain must align the words with an abstract sentence template. The optimal sentence template is “actor-action”. Sentences like “dogs bark” fit this template perfectly, so their structure is easier for the brain to compute than the reverse order. For this reason, people naturally prefer forms like “dogs bark” relative to “bark dogs”.

Articulatory plans

When people come up with names for new things, they are more likely to form words with certain sound sequences as opposed to others. For example, people are more likely to make up a word like “blog” than “lbog”.

This is the case in most languages of the world, including English. Furthermore, **studies have shown** that infants prefer listening to the “blog” over the “lbog” pattern. These results suggest that children **develop this preference spontaneously, without learning**.

Scientists think that patterns like “blog” are more frequent because they **impose fewer demands on the articulatory motor system**. When people produce sound patterns, they must carefully coordinate movements of the lip and the tongue. Lip-tongue sequences (as in “bl”) are coordinated naturally; the reverse order requires more articulatory effort (e.g., an additional articulatory sequence), so it is harder to articulate. For this reason, people naturally prefer forms like “blog” relative to forms like “lbog”.

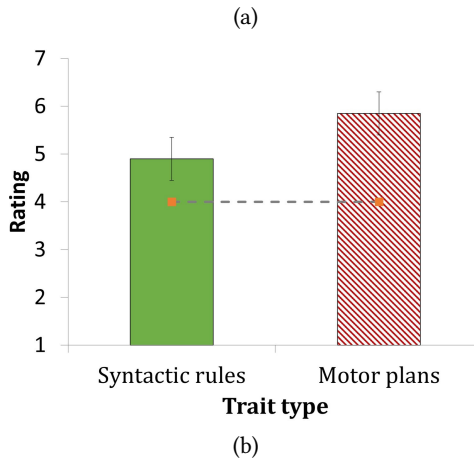


Figure 1: Laypeople’s intuitions about the innate aspects of language (from Berent et al. 2019). Panel A illustrates the materials; Panel B plots the results.

This, however, may not necessarily be the case for Empiricism. Indeed, Empiricism can arise for many other reasons. Perhaps it is our experience with schooling that makes us expect knowledge to arise from learning. Or perhaps it is our fear of moral determinism and the dangers of social discrimination that leads us to reject Rationalism (Pinker 2002). People could also embrace Empiricism because they suffer from “instinct blindness” (Cosmides & Tooby 1994) or “mindreading blindness” (Carruthers 2020).

These proposals can certainly contribute to our troubles with innateness, and they are each justified in their own right. What they fail to explain, however, is the selectivity of our intuitions: why we specifically reject innate knowledge, yet remain open to the innateness of other psychological faculties, even though they, too, are learned (e.g., motor skills, like skating), are arguably more socially worrisome (e.g., emotions like aggression) and are equally amenable to the limits of instinct blindness and the shortcomings of mindreading.

To explain why the notion of innate knowledge is especially difficult – more so than any other forms of psychological innateness – we need to invoke two intuitive psychological principles that are rooted in core knowledge: intuitive Dualism, and Essentialism (Berent 2020, 2021b). Here, I briefly explain how these principles conspire to elicit resistance to innate knowledge. I will next explain what’s wrong with this reasoning. Finally, I will show how these biases are linked to core knowledge.

3.1 A perfect Empiricism storm

Empiricism, I suggest, arises from the collision between two intuitive principles: Essentialism and Dualism. Each of these principles are tacit – they operate largely without conscious awareness, and as such, they should not be confused with the philosophical notions by the same names. And yet, these biases demonstrably interfere with reasoning.

Essentialism is that tacit belief that living things are what they are because of some innate immutable essence that they acquire from their biological parent (Keil 1986, Gelman 2003). Children, for instance, believe that a doggy is brown, like its mother, because of some tiny piece of matter that the doggy inherited from its mother (Springer & Keil 1991). Per Essentialism, then, what’s innate lies deep within the body (Springer & Keil 1991).

Dualism, on the other hand, is an intuitive belief that leads people to consider the mind as ethereal, distinct from the body (Bloom 2005). And knowledge, quintessentially “mental”, appears utterly ethereal. This belief is evident in many

previous studies, suggesting that intuitions about knowledge dissociate, depending on whether the condition targets a person’s mind or the body. When asked to consider a scenario that duplicates one’s body, people assert that the replica will maintain the donor’s physical traits, but not their knowledge (e.g., Hood et al. 2012). But when a manipulation targets only the mind (e.g., the afterlife), here, it is the donor’s knowledge that is most likely to persist (e.g., Bering & Bjorklund 2004). These dissociations suggest that knowledge is considered ethereal, in line with Dualism.

The Empiricist bias arises from the tension between Dualism and Essentialism (Figure 2). Recall that essentialism mandates that what’s innate lies in the body; Dualism, however, mandates that the mind is ethereal. It thus follows that the stuff of the mind cannot be innate. And since people consider knowledge a mental state, i.e., ethereal, the notion of innate knowledge – of language or otherwise – seems impossible, an oxymoron.

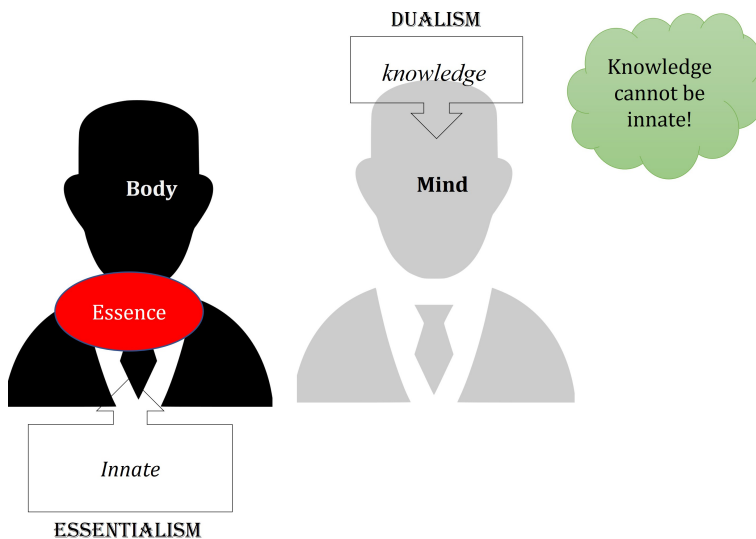


Figure 2: How Dualism and Essentialism conspire to beget Empiricism.

Recent results from my lab bear this theory out by showing that (a) people link innate traits to the body; (b) they consider knowledge ethereal; and (c) that intuitions about innateness and embodiment are linked (Berent 2021a, Berent et al. 2020b, 2021c, Berent & Platt 2021a,b,c).

3.2 It's our logic that is faulty...

Suppose this theory is right, and intuitive psychology indeed biases people towards Empiricism. What's the big deal? Are people actually wrong to endorse Empiricism?

Given that the "innateness wars" are very much ongoing among scholars, this question is difficult to decide. If scholars cannot decide "UG or not UG", how can we qualify laypeople's intuitions as right or wrong?

Obviously, we cannot. Innateness is ultimately an empirical question, and if the empirical facts are contentious, then we cannot determine whether laypeople's judgments are wrong. The real problem with laypeople's intuitions, however, isn't in the specific answer they arrive at (i.e., Empiricism). Rather, it is the logic that guides them that is problematic.

Laypeople assume that (a) "if it's in the body, it's likely innate". This is obviously false – many embodied traits are learned or emerge from experience (e.g., a scratch, Paris is the capital of France, etc.). People also assume (b) "knowledge is ethereal, i.e., disembodied". This, too, has no basis in science. And if intuitions about innateness are driven by such faulty assumptions, then the conclusions that they support are highly suspect. It's the logic of innateness intuitions, then, that is faulty.

4 Are we natural Empiricists?

Let's stop to take stock of the argument thus far. I've argued that (a) Some scientific questions are hard because they violate principles of core knowledge; and (b) Innate knowledge, generally, and UG, specifically, is a question that is difficult for people to grasp, as the principles that guide reasoning are faulty. But how do these faulty assumptions arise – do they emerge from principles of core knowledge? To rephrase the late Lila Gleitman, is Empiricism innate?

Gleitman was obviously joking. There is no reason to assume that Empiricism, or its purported instigators – Dualism and Essentialism – are innate; it is indeed difficult to see what selective advantage they might confer. But while Dualism and Essentialism are not directly innate, they could very well arise from an interaction between innate systems of core knowledge.

Essentialism could plausibly be linked to a number of distinctions that specifically can help identify living things as such, including notions of agency (Setoh et al. 2013) and the distinction between artifacts and plants (Wertz & Wynn 2014). And indeed, Essentialist thinking has been shown to arise spontaneously, even when participants' culture attributes innate physical traits to social and cultural interactions (Astuti et al. 2004).

Dualism, in turn, has been linked to the interaction between knowledge systems. Core knowledge systems are early, putatively innate principles that guide reasoning in specific domains, such as Intuitive Physics, numerical cognition, intuitive biology and theory of mind (Spelke & Kinzler 2007). Two of these systems of core knowledge could lead to Dualism: Intuitive Physics and Theory of Mind (Bloom 2005).

Briefly, Intuitive Physics maintains that objects can only interact by contact, and in the “eyes” of core physics, one’s body is just like a physical object. Yet Theory of Mind leads us to attribute people’s behavior to their mental states – to their beliefs, knowledge and goals.

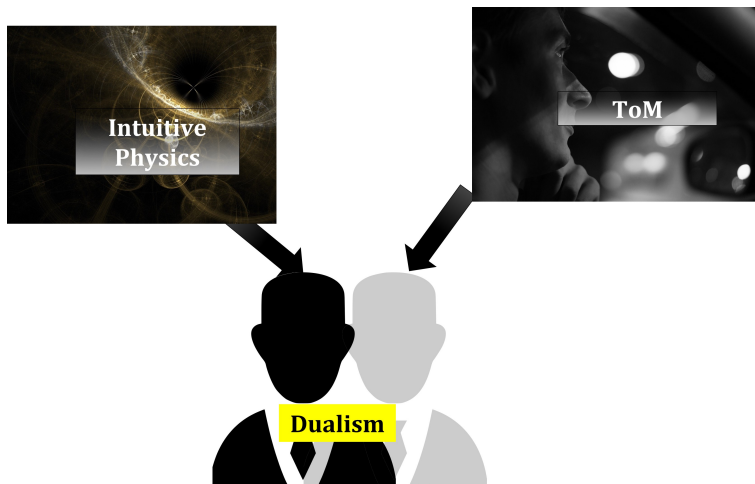


Figure 3: How Dualism arises from Intuitive Physics and Theory of Mind.

The problem, of course, is that what Theory of Mind suggests – that invisible mental states can cause one’s body to move – violates Intuitive Physics. The collision between the two systems might result in tension. To resolve the dissonance, people might assume that those invisible mental states are ethereal, rather than physical. And this is how Dualism emerges (Figure 3).

Recent results from autistic individuals support this proposal (Berent et al. 2022). Autism is known to compromise Theory of Mind. So, if Theory of Mind begets Dualism, then, compared to neurotypicals, autistic people ought to be less Dualist (and instead, lean towards Physicalism – they should view the body and mind as alike). And if Dualism further begets Empiricism, then autistic people should also veer away from Dualism and towards nativism. This is exactly what was found.

Thus, while it is unlikely that Dualism and Essentialism are innate, they may be nonetheless rooted in core knowledge. And if the UG hypothesis violates core knowledge, then it is little wonder why people consider this hypothesis unlikely.

To be clear, the question of whether Dualism and Essentialism are each rooted in core knowledge remains wide open; it is also unknown whether these two biases are universal, and whether they universally beget Empiricism; each of these steps is an open scientific question that requires much more research. As such, the theory advanced here remains partly speculative. Nonetheless, there are reasons to expect that (a) Dualism and Essentialism emerge in humans quite generally; (b) they are rooted in core knowledge; and (c) they are responsible for our Empiricist intuitions. If so, our troubles reasoning about innate knowledge could be principled.

5 Scholars aren't immune from the claws of Empiricism

While the question of why people are Empiricist is still open, it seems safe to conclude that laypeople are Empiricist – this is certainly so for Western participants, and the empirical support for this conclusion is sound. So, inasmuch as laypeople are biased, and scholars are people, scholars may not be immune from this bias either.

There is some evidence that indeed, they are not. I will first consider experimental results documenting an Empiricist bias among scholars; I will then consider some intuitions about phonology and how they fare against scientific evidence. To be clear, these results are insufficient to establish that phonologists are biased. But they certainly suffice to urge scholars to exercise greater caution.

5.1 “Mind scientists” underestimate core knowledge

To evaluate scholars' reasoning about innateness, Wang & Feigenson (2019) asked a large group of academics ($N = 400$) to evaluate the origins and onset of a number of psychological traits. Some of the questions captured sensory traits (e.g., How come Alex can see/hear?); others captured aspects of core knowledge (e.g., How come Alex thinks that hidden objects are still there?) and some consisted of knowledge that is clearly learned (e.g., reading).

Results showed that, when it comes to core knowledge, scholars grossly overestimated the role of learning, and thought these traits emerge far later in life than they demonstrably do. Shockingly, this was also the case for “mind scientists” – those that work in linguistics, psychology and neuroscience ($N = 200$).

While Wang & Feigenson (2019) did not assess the source of those intuitions, their results make it clear that “mind scholars” do lean towards Empiricism.

To reiterate, these results do not establish that scholars reject UG, and they certainly don’t show that if one rejects UG, then this position reflects an intuitive bias. Still, in light of the linguistic biases detected in laypeople (see Figure 1), certain assumptions about language ought to be particularly alluring to scholars. We now review laypeople’s intuitive understanding of phonology and compare it with some of the “received wisdom” amongst linguists.

5.2 Phonological intuitions: “It’s all in my body”

Laypeople, recall, believe that innate traits must be patently embodied. So, to the extent that language seems to exhibit some common structural regularities, those putative innate tendencies ought to arise from physical, rather than cognitive, constraints. The leap from “language universals” to “physical causes” (e.g., articulatory, auditory) is especially alluring for phonology, where cross-linguistic regularities are well attested (e.g., Greenberg 1966), and physical (articulatory, auditory) limitations are patent to introspection.

For example, it is well established that (a) syllables like *blog* (with obstruent-sonorant onsets) are far more frequent across languages than *lbog* (with the reverse sequence), and that languages that tolerate the latter (*lbog*-type) syllables tend to also manifest the former (e.g., *blog*-type syllables; Greenberg 1978). Moreover, similar preferences arise in the behavior of individual speakers (e.g., Berent 2013b). It is also patently evident that (b) language production is subject to articulatory limitation, and that *blog*-type syllables are preferred on articulatory and auditory grounds (Mattingly 1981, Wright 2004). A critical scientific question is whether the physical limitations (b) are the direct cause of the typological and behavioral observations (a).

The causation is uncertain because, logically speaking, the correlation between these two facts – (a) and (b) – can also arise from other sources. In particular, it is conceivable that the human preference for *blog* arises not from the physical causes directly (from (b)), but rather from a third cause – from some universal linguistic principles, UG (Figure 4). One can further speculate that those UG principles acquire this particular shape because they have been constrained by physical limitations in ontogeny and phylogeny – they are “grounded” in the sensorimotor system. And yet, the linguistic preferences evident in behavior could still be caused, in part, by UG (Berent 2013a). Put simply, the correlation between physical limitations and linguistic preference (b) does not necessarily imply causation.

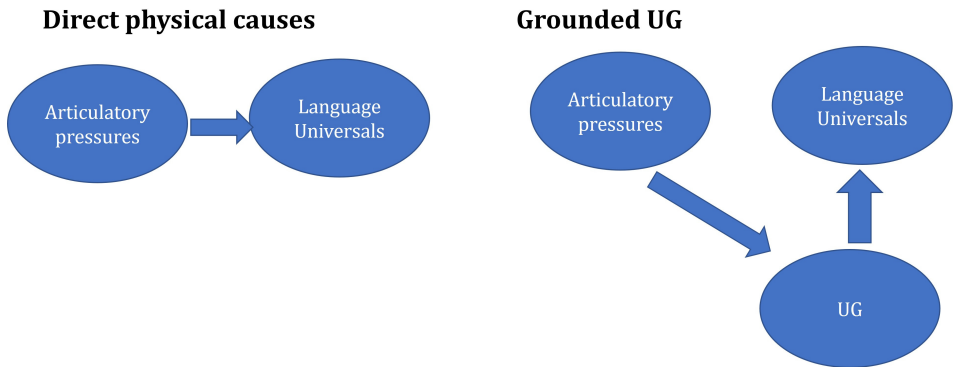


Figure 4: Two competing accounts of phonology.

A priori, one could, of course, challenge the “UG grounding” hypothesis using arguments from parsimony: if physical constraints can explain the phonological facts, then the assumption of other sources is unnecessary (i.e., unparsimonious). But arguments from parsimony are hardly decisive. In fact, since evolution is a tinkerer, not an inventor (Jacob 1977), unparsimonious biological systems are only expected. Accordingly, if discussions of UG are to rely on arguments from parsimony, then those arguments ought to be a weapon of last resort – it is the empirical evidence that ought to win the day.

And yet, few phonologists bother to differentiate, let alone adjudicate, between these competing hypotheses. For example, Fitch, Hauser and Chomsky have famously asserted that “Much of phonology is likely part of FLB, not FLN” (Fitch et al. 2005), but they bring only scant arguments in support of this conclusion. The lack of interest in the causal role of the motor system is particularly surprising given that the notion of “grounded phonology” is quite influential in modern phonology (Archangeli & Pulleyblank 1994, Hayes et al. 2004).

Why phonologists often assume that the correlation (between language universals and the sensorimotor system) implies causation is a question I cannot decide here. Instead, I describe in detail one recent piece of experimental evidence that calls this common practice into question (Berent et al. 2023).

5.3 Science counters our phonological intuitions: Evidence from TMS

To dissociate the causal role of the motor system from phonological preferences, my colleagues and I used Transcranial Magnetic Stimulation (TMS) – a method that perturbs (either increases or decreases) activity in specific brain areas by applying an electromagnetic current (Rossi et al. 2021).

A large literature, including many TMS studies, has shown that phonetic categorization relies on the articulatory motor system. Thus, the perception of labial sounds is selectively disrupted when the lip articulatory motor system is stimulated, whereas the identification of coronal sounds is selectively disrupted by stimulating the tongue. In both cases, these effects obtain regardless of whether the relevant articulator is stimulated using TMS (D’Ausilio et al. 2012, 2009, Mötönen & Watkins 2009, Smalle et al. 2015), or mechanically (e.g., by having participants bite on the lips vs. tongue, e.g., Bruderer et al. 2015; see also Berent et al. 2020c).

These results make it clear that phonetic categorization relies on motor simulation: to perceive a labial, people must tacitly simulate the articulatory process of producing a labial sound. Accordingly, when the process is disrupted (mechanically, or by TMS), identification is altered selectively.

All this shows that speech perception engages the articulatory motor system, just as our intuitions suggest. But there is a big caveat: the results presented so far concern phonetic categorization. And what is true for phonetics may not necessarily “scale up” for phonology. Our intuitions, of course, suggest it must. But as we should now know, what our intuitions say ought to be taken with a very large grain of salt. Better yet is to confront them directly using science. And so, we did.

In a series of experiments, we compared the effect of TMS on two tasks: phonetic and phonological (Berent et al. 2023). As participants performed the task, we applied TMS to either the brain motor area that controls the lip (the *Orbicularis Oris*, OO), or to a part of Broca’s area (the *Pars Triangularis*, PT).

We reason that, if the computation in each task relies on motor simulation, performance ought to be more strongly perturbed by stimulating the OO than the PT. But if the computation recruits abstract linguistic principles, then stimulating the PT ought to play a greater role; this is in line with past research suggesting that phonological computation of syllable structure engages the PT (Berent et al. 2014).

The phonetic task asked participants to identify a speech sound that was ambiguous with respect to its voicing – either labial (in between *ba* and *pa*) or coronal (in between *da* and *ta*). The logic is that, if motor simulation plays a role, then it ought to affect the perception of all features associated with a given phoneme, including voicing. Of interest is whether the perception of voicing will differ, depending on the congruence between the sound’s place of articulation (labial or not) and the stimulated area (controlling the lips or not, i.e., OO vs. PT).

Results suggested that it did, as the stimulation of the OO had opposite effects on labials and coronals. For the coronal sound, OO stimulation increased

“voiced” (i.e., *da*) response (relative to the PT). For the labial sound, by contrast, OO stimulation tended to attenuate “voiced” (i.e., *ba*) responses (relative to the PT). So far, these conclusions replicate and extend past research showing that the speech-motor system has a causal role in phonetic categorization.

The critical question concerns its role in phonological processing. To find out, we applied the same TMS manipulation to a phonological task. Here, we presented participants (English speakers) with two types of unattested monosyllables (e.g., *bnif* vs. *lbif*) along with their disyllabic counterparts (e.g., *benif* vs. *lebif*) – the task was to count the number of syllables (one/two).

Results showed that, in the syllable count task, the stimulation of the PT perturbed performance more than the OO: it promoted a bias to perceive all stimuli (monosyllables or disyllables) as disyllabic. The bias is only expected if the PT plays a causal role in the computation of syllable structure. If it does, then disrupting the PT ought to disrupt sensitivity to syllable structure; consequently, sensitivity to syllable structure ought to decline, and bias ensues. This is precisely what we found.

To summarize (see Figure 5), these results suggest that phonetic categorization relies on the speech-motor system, just as our intuitive psychology suggests. But the phonological computation of syllable structure does not: it relies on Broca’s area (PT) more heavily than the motor system. Moreover, this effect of Broca’s area is causal: when the PT is disrupted, the computation of syllable structure declines accordingly.

This conclusion flies in the face of our intuition that the dispreference of *lbog* is caused by motor difficulties alone. To be clear, these results do not specifically speak to whether the ban on *lbog* arises from rules, nor do they tell us about the origins of those rules – innate or learned. Thus, these results are moot with respect to the question of UG.

Still, a large literature in phonology and psychology interprets the undeniable correlations between phonological universals and articulatory constraints as causation (e.g., Hayes et al. 2004). This study suggests that this assumption ought to be revisited. In line with this conclusion, other results suggest that the restriction on onset structure is present at birth (well before infants can articulate such syllables; Gómez et al. 2014) and it also survives a mechanical form of articulatory suppression (Berent & Platt 2022).

At yet a broader level, research from my lab has shown that some phonological restrictions (a) rely on abstract algebraic rules (rather than statistical regularities alone, e.g., Berent et al. 2002, Gervain et al. 2012); and (b) they apply amodally – speakers spontaneously project their knowledge of spoken language to signs (Berent et al. 2021a, 2016, 2020a, 2021b, 2023).

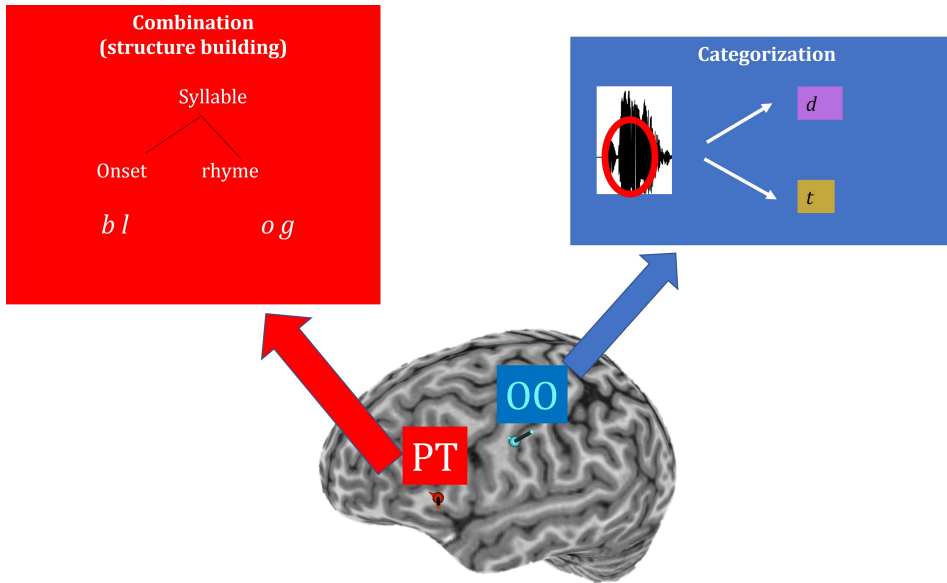


Figure 5: Graphic summary: phonetic categorization relies on motor simulation (by the OO), whereas the phonological combinatorial computation of syllable structure is abstract, and engages Broca's area (the PT) (from Berent et al. 2023).

Thus, the presumption that “phonology is all in my body” is false: the correlation between phonology and articulation doesn't imply causation. And yet, the presumption of causation is prevalent. To the extent scholars maintain this bias despite evidence to the contrary, the possibility of a bias ought to be considered.

6 Why is UG so hard?

Why, then, is UG so difficult for us to grasp – arguably, even for scholars? I think the answer to this question becomes clearer when we place our troubles within a broader context. Indeed, UG is hardly the only “hard” question. Many scientific theories can be perfectly coherent, and yet, they are difficult for people to grasp.

Quantum physics and evolutionary biology are notorious examples. Concepts in these fields are amenable to formal description, and yet, they are difficult for us to comprehend. Even notions such as “gravity” and “electromagnetism” are hard for laypeople – it is difficult to appreciate that forces can apply at a distance (Chomsky 2015, Shtulman 2017). These proposals are perfectly coherent, but they are not fully intelligible: these are notions that people struggle to grasp

intuitively. This difficulty likely arises because these scientific concepts violate principles of core knowledge (Shtulman 2017).

Consider, for example, Intuitive Physics. Young infants – indeed, newborns and nonhuman animals – possess an early understanding of what objects are and how they behave (Mascalzoni et al. 2013, Regolin & Vallortigara 1995, Vallortigara 2021). They know that objects are cohesive entities that interact only by contact (Spelke et al. 1992). So, when they see an impossible event, such as contactless interaction between two moving balls, infants are demonstrably surprised (Mascalzoni et al. 2013). And, if we come to the world expecting physical causation to require contact, it is no wonder that, when physical science shows us that forces can operate at a distance (e.g., gravity), we are baffled. The concept of contactless physical causation is perfectly coherent, but it violates Intuitive Physics. For this reason, the notion of “contactless causation” is hard.

I believe the same applies for UG. As we have seen (in Section 4), the notion of “innate knowledge”, generally, likely violates Essentialism and Dualism – biases that are rooted in core knowledge. And scientific theories that violate core knowledge are hard for us to grasp. Accordingly, core knowledge could well explain our troubles with UG.

7 Conclusions

In this piece, I sought to determine why the UG question is such a difficult problem for scholars. I’ve argued that the problem isn’t in the question itself – there is nothing incoherent about asking whether knowledge – of language or otherwise – is innate. And yet, the possibility of innate knowledge seems difficult for us to grasp.

New evidence suggests that laypeople are systematically biased against this possibility, and there is evidence that their biases are rooted in core cognition. These results open up the possibility that the difficulty with UG arises for the same reasons that people struggle to reason about gravity, electromagnetism and natural selection: these scientific proposals violate principles of core knowledge. There is indeed preliminary evidence to suggest that “mind scientists” systematically underestimate the role of core knowledge, and some anecdotal evidence to suggest that articulatory explanations of phonology are alluring. How this bias arises, and whether it is linked to core knowledge cannot be determined here. This possibility, however, cannot be ruled out.

Still, the possibility that scholarly discussions of UG are biased hardly means destiny – that the UG question cannot be scientifically evaluated. This conclusion

doesn't follow because people are equipped with systems of rational reasoning. So even if intuitive psychology engenders bias, we can still put checks and balances on our intuitive biases, just as we do in many aspects of math, physics and biology. But if such biases exist, then recognizing them might be a necessary first step to reining them in. And this is precisely why I'm writing this piece.

Evidently, the view of human nature I advance does not accord with Dan's current position. But the pursuit of human nature is a passion we share in common, and I have embarked on this path guided, in part, by his teachings.

A certain personal anecdote might serve as an illustration. As noted, Dan was my only linguistics teacher – I took with him one and a half classes; the “half” was a seminar in phonology, cut short by the birth of my son. But notwithstanding the challenges of fitting the nine-month pregnancy within the confinements of the seminar chairs, I was there pretty much until the last day. On my way to the hospital, one November night at 2:30 am, I saw a light up at the Cathedral of Learning, where Dan's office was. Recognizing his work ethic, legendary among the impressionable students, I couldn't help wondering in between contractions: is it really Dan up there? What is he writing? This is to show that, when I think of nativism, Dan has been in my mind often, and in more ways than one.

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Chapter 6

Just where are the universals? Complexities of place

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A major thrust of much theoretical linguistics has been the search for “universals”: innate, abstract, formal design principles governing all languages. Work by Dan Everett, based on long-term observation of speech in its cultural context, has raised questions about their universality and precise nature, and explored the power of culture in shaping language (Everett 2012, 2015, 2018). One proposed universal is that a fundamental design feature is recursion via complex sentences. If we look only at translations of isolated sentences from English or another contact language, we stand to miss some intriguing complexities and perhaps some deeper understanding of forces that can shape language. Here such issues are explored in the examination of the expression of place in Mohawk. Though the language does contain complex sentences of the type ascribed to universals, an examination of unscripted speech in context shows that syntactic constructions specifying place are more complex and powerful than might be predicted purely from a principle of recursion.

1 Place

Specification of place is accomplished with various formal devices in languages, most commonly demonstratives, adverbs, case-marked nouns, adpositional phrases, and adverbial clauses. An examination of spontaneous speech in Mohawk, however, a language of the Iroquoian family spoken in northeastern North America in Quebec, New York State, and Ontario, shows that there may be more, and that things might be more interesting.



Mohawk is polysynthetic and head-marking. There are three lexical categories defined in terms of their morphological structure: particles, nouns, and verbs. Location and direction can be specified with all three. Since the language is head-marking, there is no nominal case and no adpositional phrases. Locative and directional relations are understood from verbal semantics and verbal morphology. In clauses with verbs meaning such things as ‘live’ or ‘sit’, a term designating a place is likely to be in a locative relation. In clauses with verbs meaning ‘go’ or ‘insert’, a term designating a place is likely to indicate a source or goal. A cislocative prefix on verbs indicates location at an unmarked location or direction toward a reference point: ‘hither.’ A translocative prefix marks a situation at a distant location of direction away from a reference point: ‘thither.’ Place can also be specified by adverbial clauses. If, however, we observe what speakers actually do as they are choosing what to say and how to say it, we see that there are more intricate constructions used for designating place.

2 Basic grammatical structure: Managing the flow of information

Both morphological and syntactic constructions in Mohawk provide speakers with choices for how they shape the flow of information. Significant new referents are often introduced in a word or phrase, then backgrounded in subsequent speech as morphological elements of verbs. The pattern can be seen with noun incorporation, a kind of noun-verb compounding. An example is in (1). Some friends were traveling to another community, a trip which involves crossing the Canada–U.S. border. The border was introduced here in the phrase *tsi karisti:seron*, literally ‘place metal is dragged’, coined from the time the border was established by surveyors.

- (1) Separate word (Watshenní:ne’ Sawyer, speaker)
- | | | | | |
|----|--|------|----------|-----|
| Oh | neniá:wen’ne’ | ki: | nó:nen | |
| oh | n-en-iaw-en’n-e’ | ki: | n=onen | |
| | how PRT-FUT-N-happen-PFV | this | ART=when | |
| | ieniákwawe’ | | | tsi |
| | i-en-iakwa-w-e’ | | | tsi |
| | TRANSLOCATIVE-FUT-1EXCL.PL.AGT-arrive-PFV | | place | |
| | karisti:seron? | | | |
| | ka-rist-i’ser-on? | | | |
| | N-metal-drag-STATIVE | | | |
| | ‘What’s going to happen when we arrive at the border?’ | | | |

The party crossed without incident, however, and felt relieved. At this point the term for border was simply incorporated into the verb ‘cross’, since it was already part of the scene.

- (2) Incorporated noun (Watshenní:ne’ Sawyer, speaker)

Nek tsi ó:nen ia’tiakwaristí:ia’ke’ [...]

nek tsi onen i-a’-t-iakwa-rist-í:ia’k-e’

TRANSLOCATIVE-FACT-1EXCL.PL.AGT-metal-cross-PFV

but now there.we.border.crossed

‘But after we crossed the border [we felt a little better again].’

There is no syntactically based constituent order. After various orienting and connective markers, constituents occur essentially in descending order of newsworthiness at the moment. When the friends were returning home, they had to cross back over the border.

- (3) Constituent order (Watshenní:ne’ Sawyer, speaker)

Karíhton.

‘Police.’

Ó:nen sénha’ é:so’ rá:ti karíhton tho thó:nete’,

now more much of.them police there there.they.stand

‘There were more police there,’

ahskwákta.’

near.bridge.place

‘near the bridge.’

Akwé:kon káhonre’ ratíhawe.’

all gun they.carry

‘They had guns.’

Akwé:kon ne: enhóntken’sé’ ne sà:sere.

all it.is they.will.inspect the your.car

‘They were going to inspect the car.’

By the second line, the police had already been introduced. The most important information was that there were more than before, so ‘many more’ preceded the term for the police. The location by the bridge was incidental. The point of the fourth line was the guns; the verb ‘carry’ simply served to bring them onto the scene. The point of the fifth line was the inspection; the car was already part of

the scene. Similar patterns can be seen in focus constructions, especially clearly in questions and answers. A mother and daughter were preparing to go out to a meeting. The verb 'it is not cold' was the main point of the question. The particle 'outdoors' accordingly occurred after the verb.

- (4) Question and answer (Grace Curotte, Audrey Curotte, speakers)

GC Iáh ken teiowísto nátsste?
not Q is.it.cold outdoors
'Isn't it cold outside?'

AC En:, iowísto.
'Yes, it is cold.'

On another occasion, a speaker noted that the church bells were loud. Here the particle 'outside' occurred at the beginning of her statement; it was more important to her message than the sitting.

- (5) Word order (Doris White, speaker)

Átsste ki' tkítskó:tahkwe' ...
outside in.fact there.I.was.sitting
'In fact I was sitting outside.'

3 Prosodic structure

Mohawk speakers, like those of other languages, tend to produce speech in spurts, one intonation unit or prosodic phrase at a time, each conveying one significant new idea or focus of consciousness. The speaker in (6) remarked, 'I stop by the old folks' home sometimes.' Each line of transcription below and henceforth represents a single intonation unit, characterized by a coherent pitch contour. The punctuation reflects the prosody, with commas for a non-terminal contour, and periods for a terminal contour.

- (6) Intonation units (Leo Diabo, speaker)

Né: ki: ni' kí:kén:,
it.is this myself this
'I myself,'

kenh nekwá:ti,
there the.side
'over that way,'

shionsakkwátho' ki:kén:,
 when.I.stop.back.by.over.there this
 'I stop by there,'

thati'teròn:ton' ki:kén: um,
 there.they.reside.variously this
 'they live there,'

ratikstenhokòn:'a tho nekwá: ia'kkwá:tho' ostòn:ha,
 they.are.old.variously there side there.I.stop.by a.bit
 'I stop by the old folks place a bit,'

sewatié:rens.
 'sometimes.'

The next to the last intonation unit contains more words than some of the others: the action 'I stop by there' was not new information.

Constructions involving demonstratives are exploited pervasively by speakers to manage the flow of information over intonation units and sentences.

4 Structuring the flow within sentences

Cataphoric and anaphoric demonstrative constructions permit speakers to package one idea at a time prosodically, ordering them according to their significance at the moment, while retaining their coherence. Cataphoric demonstratives in one intonation unit can serve as placeholders, signaling that further elaboration is to follow. The speaker in (7) was explaining that her mother was going to go out and leave her in charge of watching her younger brother. In the first intonation unit here she noted that she (the mother) made him a pallet (a single idea conveyed in a single word with incorporated noun), and in the second, she supplied the location.

- (7) Cataphoric demonstrative (Sadie Smoke Peters, speaker)
 Thó ki' wahonwéntskaron'se' ki:kén:,
 there just she.made.him.a.pallet this
- tsi iotékha'.
 place it.burns
 'She made him a pallet by the fire.'

A similar construction is in (8). Friends were discussing a funeral, noting that the deceased was not left at the mortuary.

- (8) Cataphoric demonstrative (Josephine Kaieríthon Horne, speaker)

Thó ki' iá:ken' iahonwaia'ténhawe',
there just they.say away.they.bodily.took.him
'They just took him'

tsi-- tsi thonónhsote', ...
place-- place there.his.house.stands
'to his [father's] house.'

The speaker in (9) was relating that a man left his house in the forest and was walking to town. The stopping was the significant event in the sequence at this point, and it occurred first. The demonstrative *tho*, however, signaled that specification of the location was to follow.

- (9) Cataphoric demonstrative (Annette Kaia'titáhkhe' Jacobs, speaker)

Tánon' thó ia'thá:ta'ne' thi:kén:,
and there over.there.he.stopped that.one

ononhsatokenhthì:ke,
holy house place

ísi' na'oháhati.
yonder it.is.on.the.other.side.of.the.road
'And he stopped across the road from the church.'

Demonstratives are also used anaphorically to link an intonation unit to a preceding unit specifying place. The speaker in (10) opened his statement by setting the scene along the river.

- (10) Anaphoric demonstrative (Joe Awenhráthon Deer, speaker)

Kí:ken atsa'któntie' wáhi',
this along.the.river you.know,

thó thonathéhtaien'
there there.they.garden.have,

ónhka'k, takwáh.
just.somebody or.other
'Somebody has a garden along the river.'

A mother cleaning her son's bedroom found something dubious under her son's bed. She later recounted that when asked about it, the son responded that his teacher had told him to put the object someplace dark and warm. Here the location was the main point of his comment, so it occurred at the beginning of his statement. The demonstrative in the following intonation unit linked it to the preceding.

- (11) Anaphoric demonstrative (Marie Kahentorehtha' Cross, speaker)

Rakhró:ri se' tsi nón: tetiò:karas
 he.told.me just place it.is.dark

tánon' wahèn:ron' tsi nón: ne io'tarihen,
 and he.said place the it.is.warm

thó ki' nón: nénh sien.'
 there in.fact place there.you.will.lay.it

'He told me to put it where it's dark and warm.'

5 Distributing information across sentences

Demonstrative constructions are also used pervasively to give prominence to important ideas in separate sentences rather than subordinate clauses, while retaining coherence. The speaker cited earlier in (7) continued her story about her babysitting adventures noting that her mother laid her younger brother near the fireplace in front of the fire screen. She introduced the fire screen, a fairly complex term, in one sentence, then her mother's action in another, tying it to the preceding with the demonstrative *tho* 'there.'

- (12) Discourse coherence (Sadie Smoke Peters, speaker)

Nek tsi ohén:ton ki: ne' kà:niote' ne,
 but area.in.front this it.is it.stands it.is

tóhsa' ki: sótsi taon- taonré:ni' kí:ken,
 not this too not.would- not.would.it.spread this

katsiénha.'
 ember.

Thó wahonwaia'kión:nite' kí:ken, ...
 there she.bodily.laid.him this

'She laid him in front of the firescreen.'

The speaker in (13) was describing a pow wow ground, where people set up tables to sell jewelry. She introduced the tables in a separate sentence, as a separate element of her description of the area, not simply a location for the jewelry. Coherence was established by the demonstrative *tho* 'there.'

- (13) Discourse coherence (Annette Kaia'titáhkhe' Jacobs, speaker)

Tánon' shes' ò:ni' watekhwahra'tsheró:ton' um,
and then also it.was.tables.standing.here.and.there

ata'èn:rakon.
fence.interior.

Tánon' thó shes non: kahrónnion' ki:kén:,
And there then place it.is.set.up.here.and.there this

tsi nahò:ten' rotihsa'ánion.'
what they.have.made.variously

'And people set out things they had made on tables in the yard.'

The speaker in (14) was describing the house she grew up in, which had attached quarters for her grandmother. She introduced the addition in one sentence. She then mentioned that that arrangement was customary in those days. Then, in a separate sentence, she said that her grandmother lived there, with a demonstrative link to the preceding.

- (14) Discourse coherence (Watshenní:ne' Sawyer, speaker)

Kenh neká: iononhsanontá:kon.
there side the.house.had.an.addition

Akwé: shens tho niohtòn:ne' tsi náhe'.
all customarily there it.was.so back.then.

Thó iè:teron' ne,
there she.lived the

ne: 'ne Tóta.
the one Grandma.

The same speaker described earlier times when people would go out to an island on the other side of a bridge. Here the introduction of the island was complex, presented in one sentence, with an anaphoric demonstrative. The going was presented in a second, linked to the first with another anaphoric demonstrative.

- (15) Discourse coherence (Watshenní:ne' Sawyer, speaker)

Né: kí: tsi tiowè:note' tho nón:,
it.is this place there.it.island.stands there place

tho nón: ieiorhárhon ki:kén:,
there place over.there.it.is.moored this

áhska.
bridge

Thó ki' ní' tsi ieiakwéhtha.'
there in.fact we place over.there.we.go

'We would go to the island connected by a bridge.'

The speaker in (16) first established a location, then in a second sentence introduced chickens with an anaphoric demonstrative linking this to the previous sentence.

- (16) Discourse coherence (Joe Awenhráthon Deer, speaker)

Kawinehthà:ke ohnà:ken énska shé: kanónhsote.'
Kawinehtha's.place behind one still it.house.stands

Tho se' non: konti'terontónhkwe' ki:kén:,
there then place they.used.to.live these

kítkit.
chicken.

'The chickens used to live in the house behind Kawinéhtha's.'

6 Conclusion

Over the past several decades Dan Everett has provided thought-provoking ideas on the nature of language universals, questions of innateness, and the role of culture in shaping language. The role of culture appears to have shaped the Mohawk language in striking ways. There are long, well-documented traditions among the Iroquois of appreciation and cultivation of linguistic virtuosity in all genres, from formal oratory, through traditional tales, anecdotes, jokes, and snappy comebacks. Mohawk people often comment on the skill of particular speakers and visibly delight in it. This cultural value is reflected pervasively in masterful attention to shaping the flow of information.

Looking at what speakers actually do on a daily basis, in terms of what they choose to say and how they choose to frame it, rather than stopping at translations of isolated sentences from a contact language, promises to tell us more and more about the deeper cognitive and cultural forces shaping languages.

Abbreviations

AGT	grammatical agent
FACT	factive
PAT	grammatical patient
PRT	partitive

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Chapter 7

A Peircean turn in linguistics: Syntactic-semantic composition as logical inference

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In the late 19th century Charles Sanders Peirce proposed what can be seen as a model of natural language in which the combinatoric affinity of lexical items – which he characterizes as their respective valence – drove the composition of sentences. In this paper I argue that Peirce’s conception of valence as the basis of linguistic composition, incorporated into a logic of types in which valence is interpreted as implication, finds its formal realization in a species of categorial grammar. I further show the power of this conception in capturing a complex interaction of filler-gap connectivity with ellipsis, which has been claimed to be one of the strongest pieces of evidence for covert structure analyses of ellipsis patterns. The type-logical treatment of this supposed pattern of extraction from ellipsis sites undercuts such claims, and reinforces Joachim Lambek’s invocation of Peirce as perhaps the earliest intellectual ancestor of modern type-logical approaches to natural language architecture.

1 Peirce and valence

The work of Charles Sanders Peirce – a long-time research focus of Dan Everett, whom this festschrift honors – spans a range of interests in, and major contributions to, a variety of mathematical and scientific domains that may well be unique in the history of human accomplishment. Peirce’s work is widely recognized as seminal in mathematics, logic, the philosophy of knowledge, chemistry, astronomy and many other fields, but it is not generally recognized that he was the



source of an analytic concept, *valence*, which has become a foundational tool in linguistic theory. It is only recently that Peirce has been credited with his identification of the combinatorial potential of lexical items as one of the key drivers of linguistic form, or for being the first scholar to use the term “valence”, which he borrowed from chemistry as a close analogue of this linguistic concept, and it is still common to see linguistic applications of valence as having been “founded in 1959 by Lucien Tesnière” (Höllein 2022). There were, as noted in Przepiórkowski (2018), a number of earlier invocations of the metaphor referencing the electrons needed by atoms of an element in order to attain a stable state. But the earliest such appeal to this metaphor, as Askedal (1991) and Przepiórkowski document, was Peirce (1897), where the word *gives* is explicitly identified as having the same number of “unsaturated bonds” as the nitrogen atom, which combines with three hydrogen atoms to form the ammonia molecule NH_3 , and Przepiórkowski considers it likely that Tesnière and the others who introduced valence into the parlance of grammarians were all influenced by Peirce’s original invocation of the concept. Przepiórkowski (2018: 155) notes that “four linguists working in four different countries independently came up with the valence metaphor”, within the space of a single decade, and suggests that the common source for their exposure to Peirce’s metaphor was not Peirce himself, but Roman Jakobson, who was probably the earliest grammarian of the modern era to recognize the depth of Peirce’s insights on natural language, particularly Peirce (1897), and actively promoted Peirce’s work in conversations and international gatherings, such as the 1948 International Congress of Linguists in Paris, among other venues.

Those who have studied Peirce’s work as it bears on natural languages generally concede that his perspective was primarily rooted in their semiotic capabilities, as systems of signs. But as Nöth observes, for Peirce, “the key to syntactic structure is the predicate and its valence” (2000: 7). Peirce seems to have regarded the valence of sentences in both a syntactic and semantic way: on the one hand, the places in which names can appear (whose occupants he called “subjects”) and, on the other, as the parts of propositions which the predicate sets into the relationship that the predicate denotes, and which point to particular individuals – the referents of the names themselves.

There are a few aspects to this conception of syntax which deserve some amplification, because they bear directly on what I believe amounts to a specific development of Peirce’s ideas. Peirce clearly did not adopt the widespread contemporary view that syntactic categories are to be regarded as projections of lexical categories; that e.g. NPs are in effect just nouns with various other encrusted bits – adjectives, determiners and so on that are attached to the Ns that are the “head” of the NP. Rather, his perspective appears to have been based

much more on the conceptual burden of the items corresponding to the parts of the proposition conveyed by the sentence. But in principle, if there were a one-to-one relationship between the way in which syntactic valence is satisfied and the way in which semantic meaning is assembled, then the conceptual construal of valence and the syntactic combinatorics of language would essentially mirror each other.

Contemporary phrase structure approaches, of course, do not adhere to the analytic program such a unified view of syntax and semantics imposes. Typically, we find a set of lexical (and, in certain approaches, morphological) elements that represent the lowest tier of syntactic objects, corresponding to the terminal nodes in phrase structure trees, and more complex objects that these elements compose into, which satisfy some set of criteria – typically based heavily on distributional possibilities, displaceability chief among them. These elements combine by rules which license hierarchical structures that represent the syntactic form of a sentence as the record of all the combinatorial steps that had to apply to derive that sentence. But there is an alternative approach available, one in which lexical items are regarded as inhabitants of different *TYPES*, representing what is in effect the combinatorial “destiny” of the words inhabiting that type, and in which the mode of syntactic composition and the mode of semantic composition are at a more abstract level the same operation. Such a theoretical architecture represents, in my view, one possible way in which contemporary formal linguistics reflects a Peircean turn, although one perhaps rather different from what Peirce himself had in mind.

In a sense, it seems a bit of truism to describe any particular framework as “valence-based”; virtually all major theories or “programs” utilize some notion of valence as a central feature in licensing sentences. But it is not often appreciated how much mileage is possible by driving an approach in which the combinatoric possibilities of individual words can determine quite complex patterns and effects, including arbitrarily non-local dependencies and interactions amongst such dependencies. In the following sections I outline a framework based on this architecture – as first envisaged and articulated by Peirce – and show how it allows us to formulate alternatives to standard phrase structure analyses that do not require us to posit elaborate machinery altering the hierarchical arrangement of structures that have already been formed, but nonetheless capture a particularly intricate relationship between long-distance dependencies and ellipsis strictly on the basis of lexical argument structure.

2 Argument structure and labeled deductive systems

Theories of syntactic structure of the sort alluded to in the final paragraph of the preceding section belong to a family of frameworks that represent different versions of type-logical categorial grammar. The essential premise shared by the frameworks is that the rules of syntactic composition are stated as a deductive calculus formally equivalent to at least the implicational fragment of one or another standard truth conditional logic, with inference from valid type(s) to valid type in place of inference from true premise(s) to true premise. In the framework described below, each linguistic sign comprises a phonological and semantic annotation which is said to *label* the sign’s syntactic type. The compositional rules of the grammar are homologous to the implicational subsystem of substructural intuitionistic propositional logic (SIPL), i.e., IPL lacking rules of permutation, contraction or weakening, with implication corresponding to types of the form Y/X , $X\backslash Y$ and $Y\uparrow X$. The first of these can be thought of as something like, “give me a sign of type X on my right and you’ll get back a sign of type Y ”, and the second is the same with “left” in place of “right”. The third is a bit more complex: it tells you that, if there is a sign of type X it can be realized in a certain designated position “within” the sign typed $Y\uparrow X$. I refer to inhabitants of slashed types as *functional* terms, in view of their semantics, as discussed below.

What are the syntactic types that can instantiate Y and X ? For our purposes, we can posit three atomic types, which are in a one-to-one relationship with basic semantic types:¹

(1)	Type	Semantic object	Semantic type
	S	proposition	t
	NP	referring expression	e
	N	property	$\langle e, t \rangle$

Clauses then correspond to propositions, and NPs to Peirce’s “subjects”, so that in (2a), for example, we would assign *give* the type $(NP\backslash S)/NP/NP$.²

- (2) a. John gave Mary the manuscript.
 b. gave; **give**; $(NP\backslash S)/NP/NP$

¹Here and in what follows, I used the standard angled bracket notation $\langle \tau_1, \tau_2 \rangle$ to indicate a function from some object of semantic type τ_1 to an object of semantic type τ_2 .

²We will also take PP to be a basic type, although here matters are a bit more complex: typically, inhabitants of the type PP have the same semantic type as those typed NP.

The rules under which (2b) composes with its argument terms to yield the sentence in (2a) are, as noted, the elimination and introduction rules for implication of intuitionistic propositional logic, where implication takes the three forms noted earlier. There are a number of different formats for logical rules; the system I introduce here belongs to a subfamily of type-logical frameworks which uses the Natural Deduction conventions. In the Prawitz notation followed below, the ordinary IPL rule would take the form in (3b):

$$(3) \quad \text{a. } \frac{\phi \supset \psi \quad \phi}{\psi} \supset \text{Elim} \qquad \text{b. } \frac{\begin{array}{c} \phi \\ \vdots \\ \psi \end{array}}{\phi \supset \psi} \supset \text{Intro}$$

(3a) is nothing other than the ancient principle of *modus ponens*, where there is an antecedent (ϕ) and a consequent (ψ), such that the truth of ϕ is a guarantor of the truth of ψ (or, under the more appropriate intuitionistic interpretation, a proof of ψ follows from a proof of ϕ). (3b) is the slightly less transparent rule of hypothetical reasoning: if, in some context of established results, introducing an hypothesis ϕ allows us to deduce ψ , then in that same context, we know that the implication $\phi \supset \psi$ follows.³ In a nutshell, if we assume a certain premise that allows us to deduce a certain result, we know that, *mutatis mutandis*, if that premise were true, the result would then follow.

But translating these rules into the type-logical domain requires a good deal more than just inference rules for types. Linguistic signs do not just inhabit types; they also carry phonological and semantic information. Unlike the propositions that combine under intuitionistic rules of inference, the word(sequence)s that are the corresponding type-logical objects are ordered linearly in sentences – a property we take to be a prosodic, not syntactic fact, reflecting our partial adoption of the tectogrammatical/phenogrammatical distinction advanced in Curry (1961). Similarly, syntactic composition and inference are exactly mirrored in the semantic combinatorics, as will become evident from the full statement of the type-logical rules of inference given in (4), corresponding to (3), assumed throughout this paper. In (4) and hereafter, I take a sign to be a tripartite object with a prosodic sector, a semantic sector and a type value, presented in that order.

³Intuitionistic implication differs from classical implication in that Peirce's Law – $((\phi \supset \psi) \supset \phi) \supset \phi$ – holds for the latter but not the former, since on intuitionistic assumptions there is no way to deduce the consequent ϕ from the antecedent $(\phi \supset \psi) \supset \phi$. This is as it should be so far as our type logic is concerned, since translation of Peirce's Law into type logic results in a generally false prediction about argument structure.

(4) <i>Connective</i>	<i>Introduction</i>	<i>Elimination</i>
/	$\frac{\begin{array}{c} \vdots \vdots \\ \vdots \vdots \end{array} \frac{[\varphi; x; A]^1}{\vdots \vdots} \begin{array}{c} \vdots \vdots \\ \vdots \vdots \end{array}}{b \circ \varphi; \mathcal{F}; B} \uparrow^n$ $\frac{\vdots \vdots \quad \vdots \vdots}{b; \lambda x. \mathcal{F}; B/A} \uparrow^n$	$\frac{b; \mathcal{P}; B/A \quad a; \alpha; A}{b \circ a; \mathcal{P}(\alpha); B} /E$
\	$\frac{\begin{array}{c} \vdots \vdots \\ \vdots \vdots \end{array} \frac{[\varphi; x; A]^1}{\vdots \vdots} \begin{array}{c} \vdots \vdots \\ \vdots \vdots \end{array}}{\varphi \circ b; \mathcal{F}; B} \downarrow^n$ $\frac{\vdots \vdots \quad \vdots \vdots}{b; \lambda x. \mathcal{F}; A \setminus B} \downarrow^n$	$\frac{a; \alpha; A \quad b; \mathcal{P}; A \setminus B}{a \circ b; \mathcal{P}(\alpha); B} \setminus E$
\uparrow	$\frac{\begin{array}{c} \vdots \vdots \\ \vdots \vdots \end{array} \frac{[\varphi; x; A]^1}{\vdots \vdots} \begin{array}{c} \vdots \vdots \\ \vdots \vdots \end{array}}{b; \mathcal{F}; B} \uparrow^n$ $\frac{\vdots \vdots \quad \vdots \vdots}{\lambda \varphi. b; \lambda x. \mathcal{F}; B \uparrow A} \uparrow^n$	$\frac{b; \mathcal{P}; B \uparrow A \quad a; \alpha; A}{b(a); \mathcal{P}(\alpha); B} \uparrow E$

In (4), the vertical ellipses surrounding the variable and its composition into the proof denote the proof history subsequent to the introduction of the variable. a, b are metavariables over strings – lexical items or sequences of lexical items – while φ is a variable, supplied not by the lexicon, or as a stand-in for some actual string whose value is irrelevant in the context of the rule. Rather, variables are part of the logic itself, representing, in effect, a space in a prosodic or semantic expression that could be occupied by any term of the same type as the variable. Each variable sign is introduced with a specific index, and each application of an introduction rule is keyed to the index of the variable which is removed in the introduction of the directional slashes or λ -bound in \uparrow introduction. The elimination rules shown are, again, different avatars of (3a): a slashed term seeks a term of the antecedent type to give us a consequent type, and the result of composing the slashed term with the antecedent term is necessarily a term of the consequent type. One can see these inference rules as inversions of ordinary context-free PS rules; for example, taking VP to be an abbreviation for NP\S – a clause modulo an NP term on its left edge – we have $S \rightarrow NP \ NP \setminus S$ on the one hand and a deduction

$$\frac{NP \quad NP \setminus S}{S} \setminus E$$

on the other.⁴ The prosodic and semantic sectors combine in lockstep with the type composition: the prosody of directionally slashed types – Y/X and $X \setminus Y$ – reflects the direction of the slash: the former precedes the prosody of its type X argument, the latter follows it.⁵ The semantics, however, does not reflect the

⁴However, as noted below, this view leaves the nature of a type-logical proof open to a foundational misinterpretation.
⁵I defer discussion of vertically slashed terms till we get to the introduction rules.

direction of the slash: for all functional types, the semantic term is a function which takes the denotation of the syntactic argument as its own argument.

With this much in hand, we can now provide a complete proof of an English sentence that illustrates the ways in which type, prosody and semantics collaborate to derive the sentence in (5) as, in effect, a theorem. We start with a lexicon, as in (6):

(5) John sent those documents to that committee over the weekend.

(6) john; j ; NP	sent; send ; VP/PP _{to} /NP
those; <i>t</i> ; NP/N _{pl}	documents; docs ; N _{pl}
to; $\lambda x.x$; PP _{to} /NP	that; <i>t</i> ; NP/N
committee; comm ; N	over; over ; (VP\VP)/NP
the; <i>t</i> ; NP/N	weekend; wknd ; N

Lexical entries are axioms of the type logic (though other axioms are possible, including axioms which incur some kind of penalty, and license proofs whose output is not fully acceptable, allowing us to incorporate a range of gradience effects into the framework). A few comments on (6) are in order: PP_{to} is a subtype of PP, derived via the the unique prepositional type PP_{to}/NP, whose semantic interpretation is an identity function, yielding a denotation identical to that of its argument. Ns have subtypes N_{sg} and N_{pl}, with some determiners targeting one or the other. Finally, *over*, despite its standard identification as a preposition, is in type-logical terms a function composing with an NP to yield a function which applies a temporal semantics to a property, corresponding to a restriction of the event instantiating that property.

The rules of the logic apply to the lexical axioms to yield the proof in Figure 1. This proof can be seen as a realization of Peirce's emphasis on argument structure, and its satisfaction, as the "engine" of syntactic combinatorics. As noted at the beginning of this chapter, the types associated with strings – either in the lexicon or via composition in the course of the proof – do not reflect the standard parts of speech inherited from the classical grammarians, but rather their combinatorial affinities, determined in part by the nature of their contribution to the formation of the proposition conveyed by a declarative sentence, or of the more complex semantical object denoted by questions, and so on. Proofs proceed purely on the basis of logical inference driven by type specifications, with semantic composition mirroring the composition steps determined by the inference rules given in (4), and the rules themselves reflecting standard truth-conditional deductive systems.

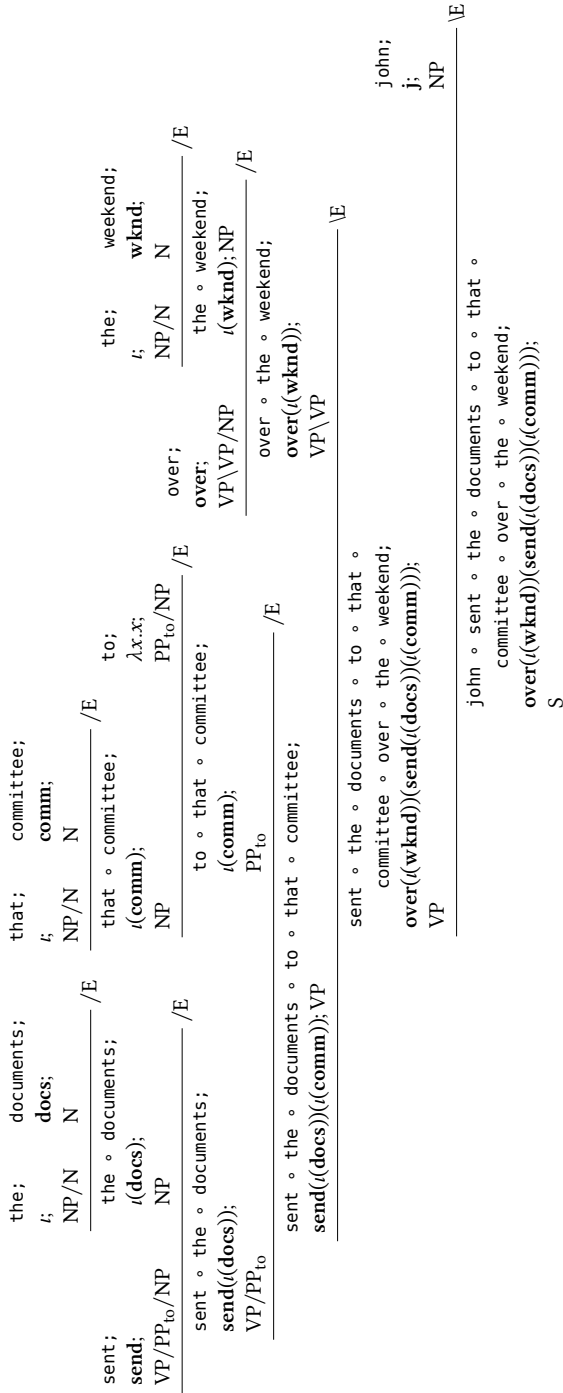


Figure 1: Proof for *John sent the documents to the committee over the weekend.*

On the other hand, one might take the view that such a proof is nothing more than a recasting of a standard hierarchical representation licensed by context-free phrase structure rules. This would be however a fundamental error: note that, in contrast to the hierarchical realization of phrase-structure rules in branching tree representations of constituent syntax, the proof steps in Figure 1 have no representational status so far as the structure of the sentence is concerned. Indeed, strictly speaking there *is* no such structure: what we have in Figure 1 is a demonstration that the closure of the axioms of the system – the English lexicon – under the inference rules of the logic allows a valid inference of a prosodic string *john ◦ sent ◦ the ◦ documents ◦ to ◦ that ◦ committee ◦ over ◦ the ◦ weekend* which signifies a proposition $\text{over}(i(\text{wknd}))(\text{send}(i(\text{docs}))) (i(\text{comm}))(j)$ and that the linguistic expression which has those prosodic and semantic values is a sentence. The steps involved in the proof have no representational status, any more than, given a set of premises Γ , the steps in the proof of $\Gamma \vdash \phi$ in some standard logic have any bearing on the content of ϕ .⁶

The difference between the logical composition of terms in Figure 1 and a tree representation of (5) under a set of phrase structure rules becomes far more stark when we turn from the elimination rules, which are the only ones in play in Figure 1, to the introduction rules shown in (4). There is nothing in phrase structure grammar which corresponds to the introduction rules, and here the advantages of the proof-theoretic framework become apparent. So-called non-constituent coordination patterns such as Right Node Raising in (7a) and Dependent Cluster Coordination in (7b) are pointed examples:

- (7) a. John bought, and Bill baked, the pizza margherita.
 b. John sent that message to Bill on Thursday and Mary on Saturday.

Both of the patterns in (7) are essentially embarrassments to frameworks based on phrase structure configurations, requiring either transformational grammar's complex arrangements of structure-altering operations, including movement and/or deletion (along with the purely stipulative constraints on the linear output of these operations required to get the facts right), or essentially stipulative constructional templates, as in later developments of HPSG (for detailed critiques of these approaches, see Levine 2011 and Kubota & Levine 2015, 2020). For proof-theoretic approaches, on the other hands, where valence satisfaction is driven by the inference rules of standard logics, the data in (7) are almost trivial to obtain with the correct semantics, once we've generalized the system based only on the

⁶For example, there are any number of ways to prove that $\vdash \phi \supset (\neg \phi \supset \psi)$ in classical logic, but the content of the implication is altogether independent of proof narrative.

elimination rules to the introduction rules that are their logical duals, as in all Natural Deduction systems. For example, we have the following straightforward application of the / Elimination rule:

$$(8) \frac{\frac{\text{bought}; \mathbf{buy}; \text{VP/NP} \quad \varphi; x; \text{NP}}{\text{bought} \circ \varphi; \mathbf{buy}(x); \text{VP}} \quad \text{john}; \mathbf{j}; \text{NP}}{\text{john} \circ \text{bought} \circ \mathbf{p}; \mathbf{buy}(x)(\mathbf{j}); \text{S}} \\ \text{john} \circ \text{bought}; \lambda x. \mathbf{buy}(x)(\mathbf{j}); \text{S/NP}$$

The / Elimination rule allows us to obtain what in standard phrase structure approaches would be characterized as a partial constituent (although, in the framework adopted here, it is no more “partial” than VPs, i.e., signs inhabiting the type $\text{NP} \setminus \text{S}$). A completely parallel proof will derive the sign in (9):

$$(9) \text{bill} \circ \text{baked}; \lambda u. \mathbf{bake}(u)(\mathbf{b}); \text{S/NP}$$

Application of the standard generalized conjunction operator \sqcap introduced in Partee & Rooth (1983), which we take to be the denotation of *and*, with the type $(X \setminus X) / X$, will then lead to the inference in (10):

$$(10) \text{john} \circ \text{bought} \circ \text{and} \circ \text{bill} \circ \text{baked}; \lambda w. \mathbf{buy}(w)(\mathbf{j}) \wedge \mathbf{bake}(w)(\mathbf{b}); \text{S/NP}$$

The final step in the proof will then be (11):

$$(11) \frac{\begin{array}{ll} \text{john} \circ \text{bought} \circ \text{and} \circ & \\ \text{bill} \circ \text{baked}; & \quad \quad \quad \vdots \quad \vdots \\ \lambda w. \mathbf{buy}(w)(\mathbf{j}) \wedge \mathbf{bake}(w)(\mathbf{b}); & \text{the} \circ \text{pizza} \circ \text{margherita}; \\ \text{S/NP} & \iota(\mathbf{pzzmarg}); \text{NP} \end{array}}{\text{john} \circ \text{bought} \circ \text{and} \circ \text{bill} \circ \text{baked} \circ \text{the} \circ \text{pizza} \circ \text{margherita}; \\ \mathbf{buy}(\iota(\mathbf{pzzmarg}))(\mathbf{j}) \wedge \mathbf{bake}(\iota(\mathbf{pzzmarg}))(\mathbf{b}); \\ \text{S}}$$

(7b) can be similarly derived via a somewhat tedious but straightforward sub-proof that yields *Bill on Thursday* and *Mary on Friday* as inhabitants of the type $(\text{PP}_{\text{to}}/\text{NP}) \setminus (\text{NP} \setminus (\text{PTV} \setminus \text{VP}))$, where PTV is an abbreviation for the type $\text{VP}/\text{PP}_{\text{to}}/\text{NP}$. The conjunction of the two is therefore also of this same type, so that *Bill on Thursday and Mary on Saturday* combines to its left first with a $\text{PP}_{\text{to}}/\text{NP}$ sign (i.e., *to*), then an NP (*the message*), then a PTV sign (*sent*), and finally VP, i.e., $\text{NP} \setminus \text{S}$, which picks up *John* to give us (7b).

In a nutshell, in both of the patterns exhibited in (7), the interplay of the elimination and introduction rules allows us to compose each of the conjoined “non-constituents” into an S as arguments of a variable, with all other components of

the S realized as variables, and then eliminate the variable terms by successive applications of the relevant introduction rules. The result is that the apparent nonconstituent prosodic elements are assigned a type, with a corresponding semantics corresponding to the application of abstraction operators at each elimination step. They are thus, in our terms, full constituents, now with the status of functional terms, and can then be conjoined. The resulting conjunction, possibly with a rather elaborate valence as in the case of (7b), then composes with its arguments to form the coordination. No structural operations, or indeed any structures at all are involved; the proofs given do nothing more than verify the association of the prosody of the specific conjunctions with a certain valence, or argument structure, and a corresponding semantics. This kind of operation is often characterized as type-raising, but in the deductive system embodied in (4), it is simply a by-product of the logic of implication elimination and introduction.

This leaves the rule for \uparrow introduction to be considered. \uparrow introduction differs from directional slash introduction in one foundational respect: rather than simply removing φ from the prosodic string, the variable becomes bound by an abstraction operator. This makes the resulting prosodic object a function, not a string, and when the prosody of a sign typed $Y \uparrow X$ composes with the prosody of a type X sign, the former takes the latter as an argument (as aptly illustrated by Figure 2). It is worth noting that the introduction rules represent a formal expression of Peirce’s observation, quoted in Nöth (2000: 8): “in the proposition ‘Anthony gave a ring to Cleopatra’, Cleopatra is as much a subject of what is meant and expressed as is the ring or Anthony. A proposition, then, has one predicate and any number of subjects.”. The significant insight here – that a sentence expressing a proposition can be composed as the ascription of some property to *any* of the argument terms – corresponds exactly to the possibility of deriving a predicate by composing a predicate with one variable term, with constants for all the other arguments, and then abstracting on that variable.

$$\begin{array}{c}
 \vdots \quad \vdots \\
 \frac{\text{gave; give; VP/PP}_{\text{to}}/\text{NP} \quad \varphi; x; \text{NP} \quad \text{to} \circ \text{cleopatra;}}{\text{gave} \circ \varphi; \text{give}(x); \text{VP/PP}_{\text{to}} \quad \text{cleop; PP}_{\text{to}} \quad \text{antony;}} \\
 \frac{\text{gave} \circ \varphi \circ \text{to} \circ \text{cleopatra; give}(x)(\text{cleop}); \text{VP}}{\text{antony} \circ \text{gave} \circ \varphi \circ \text{to} \circ \text{cleopatra; give}(x)(\text{cleop})(\text{ant}); \text{S}} \\
 \frac{\text{antony} \circ \text{gave} \circ \varphi \circ \text{to} \circ \text{cleopatra; give}(x)(\text{cleop})(\text{ant}); \text{S}}{\lambda\varphi.\text{antony} \circ \text{gave} \circ \varphi \circ \text{to} \circ \text{cleopatra; } \lambda x.\text{give}(x)(\text{cleop})(\text{ant}); \text{S} \uparrow \text{NP}}
 \end{array}$$

Figure 2: Variable introduction

The predicate in the final line of Figure 2 ascribes a property to some object; that object is in the set of things given to Cleopatra to Antony, and the proposition in

the passage Noth quotes from Peirce is decomposable into λ -terms along these lines just as much as it is the composition of *Anthony* with the denotation of the VP *gave a ring to Cleopatra*. In this sense, \uparrow introduction is the logical warrant for Peirce's view that *Anthony gave a ring to Cleopatra* is "about" any (or all) of its "subjects", not just the NP which carries the grammatical function "subject".

But the empirical problem which led to \uparrow introduction (under a different notation) in Oehrle (1994) was rather different, and took the form of the question, how can we capture the fact that quantified expressions such as *every student*, *some book*, *most journals* and so on have the same syntactic distribution as NPs, i.e., names and definite descriptions, while corresponding to radically different semantic objects? And, related to this question, is a second: how do quantifiers interact syntactically with the sentences they appear in such that they take scope over subportions of the semantic interpretation of those sentences? Various solutions have been proposed, e.g., the machinery introduced by Montague (1973), whereby all quantified expressions and names denote property sets, i.e., are functors on the properties denoted by the VPs that take them as syntactic subject, which require the use of meaning postulates; post-SpellOut movement operations ("Quantifier Raising", originating in May 1985) in the most recent incarnations of transformational grammar; Cooper's (1975, 1983) storage mechanism, adopted in Pollard & Sag (1994), and many others. In some cases the solutions involve formal devices that seem to be purpose-built for the description of quantifier's syntactic and semantic behavior, with little use outside the specific problem they were designed for, e.g., quantifier storage and retrieval; in others, there is no connection to an actual model-theoretically accessible semantic denotation, as is the case with "Quantifier Raising" in transformational grammar and the Pollard & Sag (1994) proposal; and still others are problematic in both respects.

Oehrle's (1994) breakthrough, in contrast, is conceptually simple, of extremely broad application to problems of the syntax-semantics interface, and yields a directly interpretable expression in higher-order logic that is model-theoretically defined in a straightforward way. But this last point needs to be amplified: the basic approach is itself compatible with a wide range of explicit semantic frameworks, including proof-theoretic approaches that do not appeal to any model. Oehrle's key innovation was the application of a higher-order logic in the prosodic sector, with a corresponding type hierarchy, allowing the semantics and the prosody to operate independently of each other so that quantified expressions, and scopal operators generally, can in effect take the syntactic contexts in which they appear as their own arguments. The following simple example is representative of the setup generally. We have

(12) John gave someone that book.

We take the quantified expression *someone* to be a functor that intersects a property argument with the set of people, and returns a truth value of 0 if the intersection is \emptyset , and 1 otherwise. A proof along the lines of Figure 2 will directly yield the sign in (13):

(13) $\lambda\varphi.\text{john} \circ \text{gave} \circ \varphi \circ \text{that} \circ \text{book}; \lambda x.\text{gave}(x)(i(\mathbf{book})); S \uparrow \text{NP}$

The semantics here is just what we need: the characteristic function of the set of entities who received some discourse-prominent (and in some sense pragmatically distal) member of the set of books from John. *Someone* intersects this set with that of people and, based on the model, returns a value of 0 or 1. But in that case, the pronunciation *someone* cannot itself be the prosody of *someone*, since in that case it would be an argument of the prosody in (13) despite *someone*'s semantics taking the latter's interpretation as its argument. Prosody and semantics would thus be at irreconcilable cross-purposes.

Oehrle's ingenious solution to this seeming contradiction takes the prosody of *someone* to be, not someone, but a function that applies the prosody of its $S \uparrow \text{NP}$ argument to someone. Since $\lambda\varphi.\text{john} \circ \text{gave} \circ \varphi \circ \text{that} \circ \text{book}$ is a string-to-string function, *someone* is given a prosody which applies to such functions and positions them to take a string argument someone to the pronunciation of (12). The lexical entry for *someone* is then

(14) $\lambda\sigma.\sigma(\text{someone}); \lambda P.\exists(\mathbf{person})(P); S \uparrow (S \uparrow \text{NP})$

and we have the simple proof in Figure 3.

The quantified expression takes scope over the context in which it appears – its continuation, in Barker's terms (2002, 2004) (see also Barker & Shan 2015). If two quantified expressions are introduced into a single proof, the first one introduced into the proof will scope over the material included into the proof up to that point, and will then be part of the context which the second one scopes over when the latter is in turn added in the proof. A different proof, in which the two are introduced in the opposite order, will yield the opposite scoping. No special mechanism or operation is therefore required to obtain multiple scopings under the inference rules in (4) (see Kubota & Levine 2020: Section 2.3 for details).

Oehrle's solution to the parallelism of NP and quantified expression distributions plays on the independent but linked relationship of prosody and semantics in type-logical grammar – a relationship made possible by the \uparrow connective.

$$\begin{array}{c}
 \begin{array}{cc}
 \vdots & \vdots \\
 \lambda\phi. \text{john} \circ \text{gave} \circ \phi \circ \text{that} \circ \text{book}; & \lambda\sigma.\sigma(\text{someone}); \\
 \lambda x.\text{gave}(x)(\iota(\text{book})); S \uparrow \text{NP} & \lambda P.\exists(\text{person})(P); \\
 & S \uparrow (S \uparrow \text{NP})
 \end{array} \\
 \hline
 \begin{array}{c}
 \lambda\sigma [\sigma(\text{someone})](\lambda\phi. \text{john} \circ \text{gave} \circ \phi \circ \text{that} \circ \text{book}); \\
 \lambda P[\exists(\text{person})(P)](\lambda x.\text{gave}(x)(\iota(\text{book}))); S \\
 \dots\dots\dots \beta\text{-conversion} \\
 \lambda\phi[\text{john} \circ \text{gave} \circ \phi \circ \text{that} \circ \text{book}](\text{someone}); \\
 \exists(\text{person})(\lambda x.\text{gave}(x)(\iota(\text{book}))); \\
 \dots\dots\dots S \\
 \dots\dots\dots \beta\text{-conversion} \\
 \text{john} \circ \text{gave} \circ \text{someone} \circ \text{that} \circ \text{book}; \\
 \exists(\text{person})(\lambda x.\text{gave}(x)(\iota(\text{book}))); \\
 S
 \end{array}
 \end{array}$$

Figure 3: Proof for *John gave someone that book*.

Quantified expressions parallel NPs precisely because they are in a sense parasitic on NP variables: they only appear in parts of the string where such variables can appear, undergo abstraction and ultimately replacement by the string element in the prosody of quantifiers. At the same time, their syntax targets sentences which are “missing” NPs, in the sense that some argument position in the semantics is occupied by a λ -bound variable. These characteristics of \uparrow play an essential role across a wide range of phenomena, one of which is considered in detail in Section 3 as a dramatic illustration of the way effects which require recourse to operations on phrase structure in other approaches can be reduced to mappings between valence values in type-logical grammar, with no need to posit syntactic configuration.

At this point, it’s important that we take a step back from the technical details covered in this section in order to get a more global picture of the strategy embodied in an approach based on (4). The explicit correspondence between the implicational syntax and the operations of abstraction and function application in the semantics and prosody via independent type hierarchies with their own respective λ -calculi, guarantee a fully compositional derivation of signs, with the syntactic types guiding the composition on the basis of the familiar logic of modus ponens and hypothetical reasoning. The critical point here is that not only obviously local dependencies involving argument structure, but arbitrarily long-distance effects – in particular, the interpretation of quantifier scope – are reducible to the satisfaction of argument requirements; in effect, in the proof-theoretic architecture of type-logical frameworks, valence satisfaction is the source of all observed grammatical regularities, as well as constructional idiosyncrasies.⁷ In the case of

⁷For a demonstration of how these eccentricities can be elegantly accounted for, see Kubota & Levine (2022).

scopal operators, such as generalized quantifiers – as well as symmetrical predicates such as *same*, *similar*, *different*, and various other varieties, the relationship between semantics and syntax is immediate and transparent: quantified expressions scope over the denotations of their syntactic arguments, in exactly the same way that modal auxiliaries and raising verbs scope over their VP arguments. In all cases, truth-conditional meaning is composed in accord with the valence of predicates and operators.

What about genuinely long-distance dependencies, of the sort exemplified by topicalization, wh-displacement, *tough* constructions and many others? These are standardly treated by machinery which “localizes” the dependency, but in neither derivational nor monostratal phrase-structure frameworks is the same mechanism employed for this localization as for garden-variety valence satisfaction. The point of the following analysis is to demonstrate the degree to which a proof-theoretic approach in which valence satisfaction, rather than syntactic configuration, yields the extraction dependency can capture the relevant phenomena in a simple and transparent fashion.

3 “Extraction” from ellipsis sites: What you don’t see is what you don’t get

3.1 The empirical problem

There is a sizable contemporary cross-linguistic literature on ellipsis, generally understood to refer to a varied range of phenomena in which semantic content from one part of a discourse context is part of the interpretation supplied by other (typically, but not necessarily, following) material, despite the absence of any overt phonology and syntax corresponding to that interpretation. We find, for example, patterns such as the following:

- (15) a. John likes pizza, but Bill doesn’t \emptyset \
- ‘John likes pizza, but Bill doesn’t like pizza.’
(VP/Post-auxiliary ellipsis)
- b. John eats way more junk food than he does \emptyset real food.
- ‘John eats way more junk food than he eats real food.’
(Pseudogapping)
- c. John was arguing with someone, but I don’t know who \emptyset \
- ‘John was arguing with someone, but I don’t know who John was arguing with.’
(Sluicing)

- d. Q: Who was John talking to?
A: \emptyset Someone from his department.
'John was talking to someone from his department'
(Fragment answers)

There are a number of other subspecies of ellipsis, but those in (15) have had the lion's share of attention from theorists, most of whom appear to favor some version of the basic analytic line that originates in Kuno (1981) and has been most influentially developed in Merchant (2001) and subsequent work, whereby the interpretive glosses in (15) are, in essence, the syntactic sources of the examples themselves. (15a) on this approach arises from a series of processes that can be graphically summarized as something very much like (16):

- (16) John likes pizza, but Bill doesn't [_{VP} like pizza]

Pseudo-gapping, as in (15b), is the result of a movement to the left or right of the post-auxiliary "remnant" followed by the VP deletion process suggested in (17):

- (17) John eats way more junk food than he does [_{VP} [_{VP} eat —] real food]

and so on. Most of the arguments in favor of this approach are necessarily indirect, based on patterns of acceptability judgments which seem to mirror judgments of corresponding non-ellipsed data; in Kubota & Levine (2020), a detailed examination of what appear to be the most persuasive of these arguments strongly suggests that they are in fact quite fragile on both empirical and methodological grounds. The central difficulty with such arguments is their pivotal assumption that the phenomena in ellipsis and corresponding non-ellipsed example which evoke parallel judgments of acceptability – e.g., island effects, restriction on anaphora, etc. – are themselves syntactic in nature. Building this assumption into any argument that parallel judgments of ellipsis and corresponding non-ellipsed data reflects the need to posit covert phrase structure which is deleted in the course of derivations thus appears to be a textbook instance of begging the question.

Defenders of the view that what you don't see in ellipsis was never there in the first place still have their work cut out for them, of course; it is necessary to construct plausibility arguments for the premises that (i) the putatively syntactic effects alluded to have non-syntactic origins and (ii) that the parallels between ellipsis and non-ellipsed examples can originate in the extragrammatical sources adduced in establishing (i). Examples such as the following are particularly challenging insofar as (i) is concerned;

- (18) a. I know what John ate for lunch, but I don't know what Bill did.
 b. I'm acutely aware of what I can do and what I can't. (Mahoney 2004: 735).
 c. John is certain *he* would buy *this* kind of sports car, but I have no idea what kind *I* would.

Although examples of the sort displayed in (18) are not easy to discover in corpora, they can be found with a bit of persistence, though the third example is unattested (but has been checked with multiple informants, the great majority of whom found it altogether unproblematic with the right prosody (though the latter varied somewhat from speaker to speaker)). But there is one species of this class of *wh*-extractions, so called antecedent-contained deletion, which is quite common. Data parallel to (19) can readily be found in Google search results, for example.

- (19) a. I hate feeling like everyone knows something I don't ____.⁸
 b. And perhaps they would nod with understanding at what a senior once told me: "Everyone knows something that I don't ____ I keep asking until I find out what that is."⁹
 c. However, 4 months ago i said something which i shouldn't have.¹⁰

Dozens of such instances of the construction can be found in Google searches, and there is a very substantial literature on them. Versions of the sort shown in (18) are less well-studied, but there *has* been a certain amount of research devoted to them (see, for example, Schuyler (2002) and references there).

The problem for (i) is that whereas there is now a deep body of results constituting compelling evidence against the structural origins of island effects (for recent overviews of the relevant literature, see, e.g., Chaves & Putnam (2020), Kubota & Levine (2020), Liu et al. (2022)), most frameworks take filler-gap connectivity itself as irreducibly syntactic in nature. And while there are deep consequences that follow from rejecting movement operations as the source of extraction, this theoretical position does not, on its own, give us any particular help in explaining what the *wh*-word is doing in (18). In GPSG and its descendent HPSG, for example, a feature carrying information about the syntactic and semantic content of a *wh* constituent must be carried through the structure to the point where a category matching that content satisfies the valence requirements of a selecting

⁸<https://twitter.com/therealkimj/status/1640857002896396288>, 2024-03-14.

⁹<https://www.ciomastermind.com/blog/the-arrogance-of-the-arrived>, 2024-03-16.

¹⁰<http://disq.us/p/1dhjjmu>, 2024-03-16.

head. In the case of (18), the default analysis in these frameworks would license a connectivity linkage of this sort which would be “cashed out”, as it were, by either an empty category corresponding to a valent of some transitive verb or, as in later work in HPSG assumed, in a reduction in the valence of such a verb (e.g., per the analyses of extraction patterns in van Noord & Bouma (1994) and Bouma et al. (2001)). And the entire “point” of VP ellipsis is that no such verb is present. Unsurprisingly, advocates of analyses based on covert-structure solutions to the problems posed by ellipsis seem to have been taken such examples as *prima facie* evidence for the presence of covert structure. Thus Johnson (2001) takes examples such as (18) to show that “the ellipsis site seems to have internal parts”, while Elbourne agrees that “things seem especially difficult for [approaches to ellipsis] according to which there is nothing whatsoever in ellipsis sites” (Elbourne 2008: 216). So far as I am aware, there has to date been no account of the pattern exhibited in (18) in any work in the monostratal phrase-structure tradition that offers an explicit counteranalysis to the movement-and-deletion analysis assumed by transformationalists.

But such an alternative is readily available. It rests however on a particular approach to extraction connectivity and assumes a specific analysis of VP ellipsis, both of which differ considerably from standard positions shared by both transformational and monostratal frameworks. In the following section, I first outline a commonly assumed type-logical treatment of filler/gap linkage, and in the next section, recapitulate the treatment of VP ellipsis, and its generalization to pseudogapping, proposed in Kubota & Levine (2017). This background sets the stage for my account of (18).¹¹

Muskens (2003) outlines a treatment of unbounded *wh*-dependencies, readily extendable to topicalization, which differs radically from previous analyses of extraction within both phrase-structure-based approaches and categorial grammar. In terms of *wh*-relatives, Muskens’ proposal takes the form of the lexical sign in (20):

$$(20) \quad \lambda\sigma.\text{which} \circ \sigma(\varepsilon); \lambda P\lambda Q\lambda w.P(w) \wedge Q(w); (N \setminus N) \uparrow (S \uparrow NP)$$

Unpacking this operator a bit, we can see that its argument structure seeks a clause missing an NP, and its denotation is predicated of some entity, while the prosodic functor corresponding to the $S \uparrow NP$ argument applies to a string of length zero. To derive (21), then, we start with the subproof in Figure 4.

(21) the book which John lost yesterday

$$\begin{array}{c}
\text{lost;} \\
\lambda x \lambda y \mathbf{lose}(x)(y); \\
\frac{(\text{NP} \setminus \text{S}) / \text{NP} \quad [\varphi_0; u; \text{NP}]^1}{\text{lost} \circ \varphi_0; \lambda y \mathbf{lose}(u)(y); \text{NP} \setminus \text{S}} \quad \frac{\text{yesterday;} \\ \lambda P \lambda v. \mathbf{y}st(P)(v); \\ (\text{NP} \setminus \text{S}) \setminus (\text{NP} \setminus \text{S})}{\text{john};} \\
\frac{\text{lost} \circ \varphi_0 \circ \text{yesterday;} \\ \lambda v. \mathbf{y}st(\mathbf{lose}(u))(v); \text{NP} \setminus \text{S}}{\text{j}; \\ \text{NP}} \\
\textcircled{1} \rightarrow \frac{\text{john} \circ \text{lost} \circ \varphi_0; \mathbf{y}st(\mathbf{lose}(u))(j); \text{S}}{\lambda \varphi_0. \text{john} \circ \text{lost} \circ \varphi_0 \circ \text{yesterday}; \lambda u. \mathbf{y}st(\mathbf{lose}(u))(j); \text{S} \upharpoonright \text{NP}}
\end{array}$$

Figure 4: Relative clause subproof 1

The operator in (20) takes arguments of this type and returns a function which picks up an N on the left, while β -converting a zero-length string into the position occupied by φ_0 in the last proof line in Figure 4, giving us *which John lost yesterday*.

$$\begin{array}{c}
\lambda \sigma. \text{which} \circ \sigma(\varepsilon); \quad \lambda \varphi_0. \text{john} \circ \text{lost} \circ \varphi_0 \circ \text{yesterday}; \\
\lambda P \lambda Q \lambda w. P(w) \wedge Q(w); \quad \lambda u. \mathbf{y}st(\mathbf{lose}(u))(j); \\
\frac{(\text{N} \setminus \text{N}) \upharpoonright (\text{S} \upharpoonright \text{NP}) \quad \text{S} \upharpoonright \text{NP}}{\lambda \sigma [\text{which} \circ \sigma(\varepsilon)] (\lambda \varphi_0. \text{john} \circ \text{lost} \circ \varphi_0 \circ \text{yesterday}); \\ \lambda P [\lambda Q \lambda x. P(x) \wedge Q(x)] (\lambda u. \mathbf{y}st(\mathbf{lose}(u))(j)); \\ \text{N} \setminus \text{N}} \\
\cdots \cdots \cdots \\
\text{which} \circ \lambda \varphi_0 [\text{john} \circ \text{lost} \circ \varphi_0 \circ \text{yesterday}] (\varepsilon); \lambda Q. [\lambda w. \mathbf{y}st(\mathbf{lose}(w))(j) \wedge Q(w)]; \text{N} \setminus \text{N} \\
\cdots \cdots \cdots \\
\text{which} \circ \text{john} \circ \text{lost} \circ \varepsilon \circ \text{yesterday}; \lambda Q [\lambda w. \mathbf{y}st(\mathbf{lose}(w))(j) \wedge Q(w)]; \text{N} \setminus \text{N}
\end{array}$$

Figure 5: Relative clause subproof 2

The final part of the proof supplies an N argument to the functional term in the last proof line in Figure 5.

$$\begin{array}{c}
\text{which} \circ \text{john} \circ \text{lost} \circ \varepsilon \circ \text{yesterday}; \\
\lambda Q [\lambda w. \mathbf{y}st(\mathbf{lose}(w))(j) \wedge Q(w)]; \\
\frac{\text{book}; \mathbf{book}; \text{N} \quad \text{N} \setminus \text{N}}{\text{book} \circ \text{which} \circ \text{john} \circ \text{lost} \circ \varepsilon \circ \text{yesterday}; \lambda w. \mathbf{y}st(\mathbf{lose}(w))(j) \wedge \mathbf{book}(w)}; \text{N}
\end{array}$$

Figure 6: Relative clause subproof 3

¹¹For a rather different, though ultimately related approach to a solution in a framework belonging to a distinct class of categorial grammar frameworks, see Jacobson (1992).

We thus obtain *book which John lost*, denoting the set of things which have the properties of being books and being objects that John lost.

The critical point for us is what happens at ① in Figure 4. Application of \uparrow Introduction abstracts on the variable terms superscripted as 1 – an operation completely indifferent to the length of the string in which the variable φ_0 appears. Exactly the same step would take us from the expression above the proof line in Figure 7 to the sign below the line.

$$\begin{array}{l}
 \text{mary} \circ \text{thinks} \circ \text{bill} \circ \text{remembers} \circ \text{ann} \circ \text{saying} \circ \text{john} \circ \text{lost} \circ \varphi; \\
 \text{think}(\text{remember}(\text{saying}(\text{yest}(\text{lost}(u))(j))(a))(b))(m); \\
 \text{S} \\
 \hline
 \lambda\varphi. \text{mary} \circ \text{thinks} \circ \text{bill} \circ \text{remembers} \circ \text{ann} \circ \text{saying} \circ \text{john} \circ \text{lost} \circ \varphi; \\
 \text{think}(\text{remember}(\text{saying}(\text{yest}(\text{lost}(u))(j))(a))(b))(m); \\
 \text{S}
 \end{array}$$

Figure 7: Long-distance relative clause subproof

Essentially the same proof storyline in Figures 4–6 will give us *book which Mary thinks Bill remembers Ann saying John lost*. There is no local registration of the information linking the filler to the gap – nothing analagous to cyclic wh-movement, no SLASH feature shared between vertically adjacent nodes in a phrase-structure tree that gets realized at the tail end of the chain. Properly speaking, there isn’t even anything that can be properly identified as a gap “site”. We have a prosodic component of the sign with no marker corresponding to some missing substring, since the model theory for the prosodic calculus interprets $a \circ b \circ \varepsilon$ as $a \circ b$; nor is there any representation in the semantics or the syntactic type of something we would want to call a “gap”. In a way, this treatment of extraction is an echo of the view in extraction in the earliest phase of transformational grammar, when wh-movement shifted a constituent to the left over an unconstrained variable. The appearance of Ross (1967) resulted in the almost universal rejection of this view, but the most recent research on the island effects that Ross first documented, as noted earlier, overwhelmingly supports a view of such effects which takes them to be epiphenomena of functional factors. Clearly, the nonlocal view of syntactic connectivity has an empirical claim on a second act.¹²

¹²This is not to say, of course, that Muskens’ operator is completely unproblematic. For one thing, it has an obvious failure in its coverage, since obviously there’s no way that (20) as given accounts for pied-piping. A second problem is that the linearity of the type logic shared by Muskens’ λ -grammar and our own HTLG, inter alia, makes it difficult to derive multiple

3.1.1 VP ellipsis and pseudogapping

The correct explanation for data such as (18) obviously depends on an empirically sound analysis of VP ellipsis in the first place. The standard transformational approaches following Kuno (1981) face severe empirical challenges and serious conceptual problems, detailed in Kubota & Levine (2017). These problems are avoided in the proof-theoretic solution proposed there, whose central premise is that VP ellipsis itself is the expression of a kind of “zero derivation” whereby signs typed VP/VP are mapped to the type VP, whose denotation is the application of the modal/aspectual operator of the input sign to some salient property retrieved from the discourse context or, under certain conditions, inferred exophorically, per Miller & Pullum (2013). This approach is implemented via the operator in (22), where \$ is a variable over sequences of arguments, following notation introduced in Steedman (2000).¹³

(22) VP ellipsis operator

$\lambda\varphi.\varphi; \lambda\mathcal{F}.\mathcal{F}(P'); ((NP \setminus S)\$)\uparrow(((NP \setminus S)\$)\uparrow((NP \setminus S)\$))$

– where P' is a free variable whose value is resolved anaphorically

(23) Anaphora resolution condition on the VP ellipsis/pseudogapping operator:

1. if there is a syntactic constituent with category VP in the antecedent clause matching the syntactic category of the missing verb in the target clause, then the value of P is identified with the denotation of that constituent;
2. if there is no such syntactic constituent, then the value of P is anaphorically identified with some salient property in the discourse that is not inconsistent with the syntactic category VP.

An example of simple VP ellipsis, illustrating how the ellipsis operator in (22) works, is given in (24).

(24) Mary should call Ann, but Bill shouldn't.

At the grayed-in proof line in Figure 8, the free variable P is instantiated as the prominent contextually available property **call(a)**.

extractions linked to a single filler. In Kubota & Levine (2020), we offer solutions for both problems, and are currently generalizing our proposal for pied-piping to take into account the interaction of the latter with a variety of coordination possibilities, an aspect of the pied-piping problem that does not appear to have been previously addressed. But for our purposes, the approach exemplified in (20) is completely serviceable.

¹³Because the prosodic term is a function, the main connective in the type description is \uparrow rather than $/$.

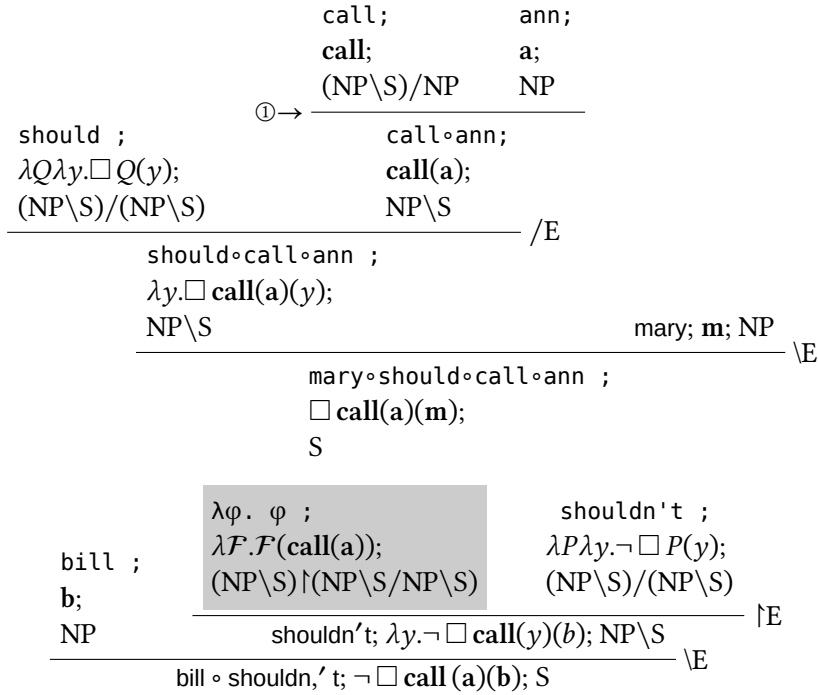


Figure 8: VP ellipsis proof

More complex cases, e.g. those involving sloppy identity (*John thinks he deserves a promotion, and Bill does too*) and scopal operators (*John read every book before Bill did*) fall out altogether straightforwardly on this approach, as shown in Kubota & Levine (2017: 236–238). But for our purposes, what is relevant is the fact that the operator in (22) applies to a functional term taking a complete NP\S to a complete NP\S – which we can abbreviate as VP/VP – and returns a complete VP. Suppose now that we generalize the operator so that it applies to a functional term taking a *partial* VP to a partial VP, and returns a partial VP. This seems perhaps like a question completely orthogonal to the phenomena we’re looking at, because auxiliaries are, in non-transformational frameworks generally, taken to apply to VPs and return VPs, period. But it is a strict theorem of our proof theory that every VP/VP type has a prosodically identical counterpart which applies to VP/NP objects and returns a VP/NP object – i.e., maps a transitive verb to a transitive verb. This is nothing more than a conversion into type logic of one consequence of the transitivity of implication in standard logics, and is simply demonstrated as in Figure 9 (where \circ is a variable over arbitrary operators).

$$\frac{\frac{[\varphi_2; R; VP/NP]^2 \quad [\varphi_1; u; NP]^1}{\varphi_2 \circ \varphi_1; R(u); VP} /E \quad \boxed{a; \lambda T. \lambda z. \circ T(z); VP/VP}}{\frac{a \circ \varphi_2 \circ \varphi_1; \lambda z. \circ R(u)(z); VP}{a \circ \varphi_2; \lambda u \lambda z. \circ R(u)(z); VP/NP} /I^1} /E$$

$$\frac{\boxed{a; \lambda R \lambda u \lambda z. \circ R(u)(z); (VP/NP)/(VP/NP)}}{/I^2}$$

Figure 9: Type-logical Geach theorem proof

The point is that $VP/VP \vdash (VP/NP)/(VP/NP)$, and a completely parallel entailment can be proven between VP/VP and terms of type $VP/NP/NP$, $VP/PP/NP$, etc. In general, then, for any auxiliary, we have an entailment $VP/VP \vdash VP\$/VP\%$. It follows that if we generalize the VP ellipsis operator to the type $VP\$\uparrow(VP\$/VP\%)$, we derive an operator that yields a form of the auxiliary as a transitive verb, a ditransitive verb and so on. And such an operator enables us to extend the coverage of the VP ellipsis rule to the pseudogapping phenomenon illustrated in (15b) above; to evade the complexities of the comparative semantics, I use the somewhat less natural (though still typically acceptable) *but*-conjunction in (25):

(25) For some reason, John will read ESSAYS but he won't NOVELS.

Generalizing the VP ellipsis operator to the form in (26) would have the effect of taking *won't*, typed $(VP/NP)\uparrow((VP/NP)/(VP/NP))$ (via application of the Geach entailment, with $\$ = NP$) to an auxiliary typed VP/NP , i.e., a transitive verb. This revised operator can be stated as in (26):

- (26) Generalized ellipsis operator
 $\lambda\varphi.\varphi; \lambda\mathcal{F}.\mathcal{F}(P); VP\$\uparrow(VP\$/VP\%)$
 – where P is a free variable whose value is resolved anaphorically
- (27) Anaphora resolution condition on the VP ellipsis/pseudogapping operator:
1. if there is a syntactic constituent with category $VP\%$ in the antecedent clause matching the syntactic category of the missing verb in the target clause, then the value of P is identified with the denotation of that constituent;
 2. if there is no such syntactic constituent, then the value of P is anaphorically identified with some salient property in the discourse that is not inconsistent with the syntactic category $VP\%$.

We can now derive Figure 10 directly.

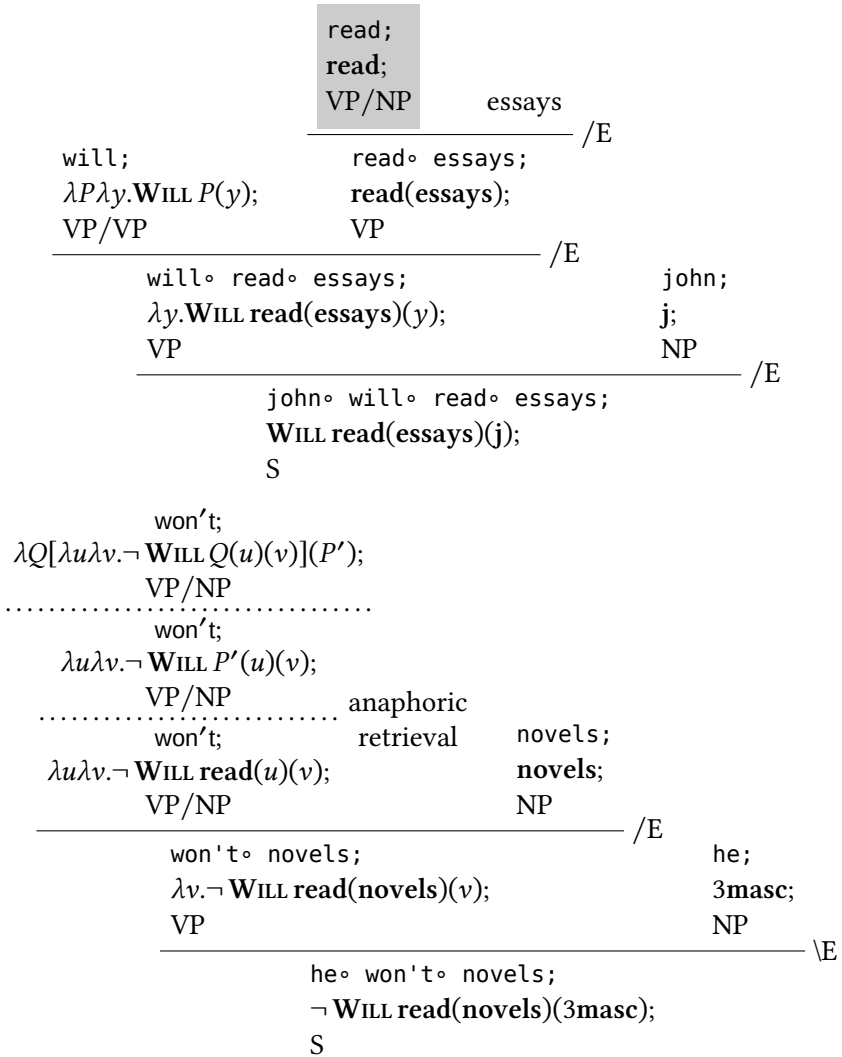


Figure 10: Pseudogapping proof

Intuitively, the application of the generalized operator in (26) is to repackage an auxiliary and a transitive verb as a somewhat longer and more complex transitive verb, so that rather than composing *read novels* as a VP and applying a standardly typed auxiliary to derive a VP, we in effect repackage *won't* and *read* as a transitive verb *won't*, whose type is the same as *read* itself, but which, after the retrieval of the corresponding predicate in the antecedent clause, applies the semantics of *won't* to the proposition derived by supplying this transitive verb with its arguments.

With our generalized account of ellipsis, we are now in a position to see how the proof-theoretic approach introduced in the preceding sections can license examples such as (18) without recourse to any actual material corresponding to the gap “site” in the antecedent clause ever being involved.

3.1.2 Pseudo-extraction via pseudogapping

In the analysis that follows, apparent extraction from an ellipsed VP arises as a result of Muskens-style extraction from one or another argument of the “transitive” auxiliary which is associated with the general ellipsis operator introduced in Section 3.1.1. That is, examples such as (18) involve not just a semantic object, as in purely semantic accounts of ellipsis (e.g., that given in Hardt 1993) analysis, but an actual syntactic extraction from an ordinary overt VP, as we show below. Treatments such as Hardt’s, or that given in Dalrymple et al. (1991), have, as noted in the citation above from Elbourne (2008), a difficult time accounting in a simple way for cases such as (18); under the analysis which follows, in contrast, these constructions are predicted to conform to whatever conditions hold on extraction in general without any concomitant assumption of covert structure corresponding to an ellipsed VP.

What is distinctive about the filler-gap relationship, as vs. the standard picture of valence, is that while in both cases we have material that is missing other material required to compose a constituent, in the case of the former, the gap can be missing from anywhere within the partial constituent. That is, while Y/X is a sign that must compose on the left with a sign of type X , and $X \setminus Y$ is the same but seeking an X argument on its left to yield an object typed Y , the material missing from the string that is required for Y in $Y \uparrow X$ can, as noted in Section 2, appear anywhere. Thus, in *I wonder what John said to Mary*, the subconstituent *said ___ to Mary* constitutes a VP with a medial NP gap, meeting the description $VP \uparrow NP$. In terms of sentences such as (18), what we want is a way to get *did* to have the type $VP \uparrow NP$, in which case we would, roughly speaking, apply a Muskens-style operator *what* to a clause composed from this $VP \uparrow NP$. As I show directly, given

a sign *did* typed $VP \uparrow NP$, we can use hypothetical reasoning to deduce $S \uparrow NP$ and then apply the *what* operator to obtain signs of expressions such as *what John did*. Furthermore, we predict on such an approach the well-formedness of e.g.,

- (28) a. Do you think the British know something (about this) that we don't (at this point)? (Penn Treebank/Wall Street Journal corpus, cited in Bos & Spenader (2011), slightly modified)
 b. Kollberg suspects Petrus, who Beck does ___ as well (Kennedy 1997: 666)

(26) will not do the trick here, since it only gives us the possibility of elements missing on the right, not medially. What's needed, clearly, is some way to extend the generalized ellipsis rule still further. Fortunately, just as we were able to show that terms typed VP/VP can, by the Geach theorem proof given in Figure 9, be extended to the type $(VP/NP)/(VP/NP)$, we can prove that for any term inhabiting VP/VP , there is a corresponding term with functional prosody having the schematic form $(VP \uparrow XP) \uparrow (VP \uparrow XP)$ for any type XP . The structure of the proof is essentially the same as that of Figure 9, but involving higher order terms.¹⁴

$$(29) \quad \frac{\varphi_1; \mathcal{O}; VP/VP \quad \frac{[\sigma_1; f; VP \uparrow NP]^1 \quad [\varphi_2; x; NP]^2}{\sigma_1(\varphi_2); f(x); VP}}{\varphi_1 \circ \sigma_1(\varphi_2); \mathcal{O}(f(x)); VP}}{\lambda\varphi_2.\varphi_1 \circ \sigma_1(\varphi_2); \lambda x.\mathcal{O}(f(x)); VP \uparrow NP} \uparrow^2}{\lambda\sigma_1\lambda\varphi_2.\varphi_1 \circ \sigma_1(\varphi_2); \lambda f\lambda x.\mathcal{O}(f(x)); (VP \uparrow NP) \uparrow (VP \uparrow NP)} \uparrow^1$$

With this result in hand, all that is needed to derive any given auxiliary as a VP seeking a gap-filling NP constituent *somewhere* is a further extension of the already-generalized ellipsis operator to such “vertically Geached” auxiliaries, mapping them to type $VP \uparrow XP$, anaphorically supplying the meaning of the gapped VP . In (30), I give a “local” form of this extension of the ellipsis operator to internal gaps.

- (30) $\lambda\rho\lambda\varphi_1.\rho(\lambda\varphi_0.\varphi_0)(\varphi_1); \lambda\mathcal{F}.\mathcal{F}(R')$; $(VP \uparrow NP) \uparrow ((VP \uparrow NP) \uparrow (VP \uparrow NP))$
 – where R' is the semantic term of a sign retrieved from the context whose type is $VP \uparrow NP$

As before, we first specify how the antecedent clause of (31) makes available the predicate which is retrieved in the ellipsed clause, per Figure 11.

- (31) I know what John ate for lunch, but I don't know what_i Bill did eat ____i for lunch.

$$\begin{array}{c}
 [\varphi_1; x; \text{NP}]^1 \\
 \vdots \\
 \text{ate} \circ \varphi_1 \circ \text{for} \circ \text{lunch}; \\
 \text{ate}(x)(\text{lunch}); \text{VP} \\
 \textcircled{1} \rightarrow \frac{\lambda\varphi_1. \text{ate} \circ \varphi_1 \circ \text{for} \circ \text{lunch};}{\lambda x. \text{ate}(x)(\text{lunch}); \text{VP} \upharpoonright \text{NP}} \upharpoonright^1 \quad \left[\begin{array}{c} \varphi_2 \\ u; \\ \text{NP} \end{array} \right]^2 \\
 \frac{\text{ate} \circ \varphi_2 \circ \text{for} \circ \text{lunch}; \text{ate}(u)(\text{lunch}); \text{VP}}{\text{john} \circ \text{ate} \circ \varphi_2 \circ \text{for} \circ \text{lunch}; \text{ate}(u)(\text{lunch})(j); \text{S}} \quad \text{john}; \\
 \quad \text{j}; \\
 \quad \text{NP} \\
 \frac{\lambda\varphi_2. \text{john} \circ \text{ate} \circ \varphi_2 \circ \text{for} \circ \text{lunch}; \text{ate}(u)(\text{lunch})(j); \text{S}}{\lambda\varphi_2. \text{john} \circ \text{ate} \circ \varphi_2 \circ \text{for} \circ \text{lunch}; \lambda u. \text{ate}(u)(\text{lunch})(j); \text{S} \upharpoonright \text{NP}} \upharpoonright^2 \quad \lambda\sigma. \text{what} \circ \sigma(\varepsilon); \\
 \quad \lambda P. \text{what}(P); \\
 \quad Q \upharpoonright (\text{S} \upharpoonright \text{NP}) \\
 \hline
 \text{what} \circ \text{john} \circ \text{ate} \circ \varepsilon \circ \text{for} \circ \text{lunch}; \text{what}(\lambda u. \text{ate}(u)(\text{lunch})(j)); Q
 \end{array}$$

Figure 11: Ellipsis pseudo-extraction antecedent proof

The grayed-in semantic term in Figure 11 is an available predicate with which the free variable R' obtained in the proof line ① can be anaphorically identified. The first part of the proof for *what Bill did* then takes the following form:

$$\begin{array}{c}
 \vdots \\
 \lambda\sigma \lambda\varphi. \text{did} \circ \sigma(\varphi); \quad \lambda\rho \lambda\varphi. \rho(\lambda\varphi_0. \varphi_0)(\varphi); \\
 \lambda f \lambda x \lambda y. f(x)(y); \quad \lambda \mathcal{F}. \mathcal{F}(\lambda x. \text{ate}(x)(\text{lunch})); \\
 (\text{VP} \upharpoonright \text{NP}) \upharpoonright \quad (\text{VP} \upharpoonright \text{NP}) \upharpoonright \\
 \frac{(\text{VP} \upharpoonright \text{NP}) \upharpoonright (\text{VP} \upharpoonright \text{NP}) \quad ((\text{VP} \upharpoonright \text{NP}) \upharpoonright (\text{VP} \upharpoonright \text{NP}))}{\lambda\varphi. \text{did} \circ \varphi; \lambda x \lambda y. \text{ate}(x)(\text{lunch})(y); \text{VP} \upharpoonright \text{NP}} \quad \left[\begin{array}{c} \varphi_3 \\ v; \\ \text{NP} \end{array} \right]^3 \\
 \frac{\text{did} \circ \varphi_3; \lambda y. \text{ate}(v)(\text{lunch})(y); \text{VP}}{\text{bill} \circ \text{did} \circ \varphi_3; \text{ate}(v)(\text{lunch})(b); \text{S}} \quad \text{bill}; \\
 \quad \text{b}; \\
 \quad \text{NP} \\
 \frac{\text{bill} \circ \text{did} \circ \varphi_3; \text{ate}(v)(\text{lunch})(b); \text{S}}{\lambda\varphi_3. \text{bill} \circ \text{did} \circ \varphi_3; \lambda v. \text{ate}(v)(\text{lunch})(b); \text{S} \upharpoonright \text{NP}} \upharpoonright^3
 \end{array}$$

Figure 12: Ellipsis pseudo-extraction ‘gap’ site

The term obtained at the last step of this proof, supplied as an argument to the extraction operator, yields an interpretation identical to the unellipsed embedded question *what Bill ate for lunch*. Note that the prosodic term derived in the last proof step, $\lambda\varphi_3. \text{bill} \circ \text{did} \circ \varphi_3$, is exactly what we would have obtained via the earlier version of the generalized ellipsis operator; the associated type would however been S/NP, and therefore ineligible to compose with *what*. Moreover, as noted above, only the vertically-slashed version of the ellipsis operator would allow us to derive a sentence with a non-peripheral “gap” as in (28). But the larger point is that long-distance dependencies into what appear to be ellipsis contexts are, on this analysis, based on what is in effect the extraction of a pseudogapping

¹⁴In (29), I gloss over certain important technical details in order to lay out most clearly the proof narrative.

remnant. For example, a proof along the lines of that began along the lines of (12) might have continued as in Figure 13.

$\vdots \quad \vdots$	$\lambda\rho \lambda\varphi. \rho(\lambda\varphi_0. \varphi_0)(\varphi);$		
$\lambda\sigma \lambda\varphi. \text{did} \circ \sigma(\varphi);$	$\lambda\mathcal{F}. \mathcal{F}(\lambda x. \text{ate}(x)(\mathbf{lunch}));$		
$\lambda f \lambda x \lambda y. f(x)(y);$	$(\text{VP} \uparrow \text{NP}) \uparrow$		
$(\text{VP} \uparrow \text{NP}) \uparrow (\text{VP} \uparrow \text{NP})$	$((\text{VP} \uparrow \text{NP}) \uparrow (\text{VP} \uparrow \text{NP}))$	breakfast;	
$\lambda\varphi. \text{did} \circ \varphi; \lambda x \lambda y. \text{ate}(x)(\mathbf{lunch})(y); \text{VP} \uparrow \text{NP}$		brkfst;	he;
		NP	3 <i>masc</i> ;
	$\text{did} \circ \text{breakfast}; \lambda y. \text{ate}(x)(\mathbf{brkfst})(y); \text{VP}$		S
	$\text{he} \circ \text{did} \circ \text{breakfast}; \mathbf{ate}(\mathbf{brkfst})(3\mathit{masc}); \text{S}$		

Figure 13: Non-extraction pseudogapping

This would then be an ordinary instance of pseudogapping as in *John ate lunch much faster than he did breakfast*. The upshot is that apparent extraction from ellipsis sites as in (18) is nothing other than the interaction of Muskens-style wh-operators with the object of a transitive auxiliary – a possibility that we would predict in advance on the analysis given above.

The reader might suppose that the possibility of this kind of extraction depends on some kind of parallel interpretation between the antecedent and the ellipsed clauses in (18), based on the extraction already visible in the former. But we also have examples where there is no extraction in the antecedent, such as (28) and (32):

- (32) John is certain he would buy *this* kind of sports car, but I have no idea what kind *I* would.

To obtain such examples, we derive the antecedent by a derivation which includes the subproof in Figure 14.

From this point on, the proof for the ellipsed clause would proceed in exactly the same fashion as in the derivation of (31), with the free variable P' instantiated as the grayed-in term in (14).

The above (re)analysis of “extraction out of an elided VP” as extraction of a pseudogapping remnant gives us, in effect, a proof-of-concept argument for rejecting the assumption that covert structures in VP ellipsis necessarily exist in order that a “site of origin” be available for filler/gap linkages that appear to implicate material missing from deleted VPs.¹⁵ There is, on the analysis presented

¹⁵While this approach has been challenged in Johnson (2001), on the grounds that apparent extraction from ellipsis sites is subject to different constraints from pseudogapping, counterexamples to his claims are already familiar from, inter alia, examples from corpora or naturally occurring data presented in Levin (1979). For detailed discussion of this point, see Kubota & Levine (2020: Section 8.4.2).

buy;	φ_1 ;	
buy ;	x ;	
VP/NP	NP	
<div style="display: flex; justify-content: space-between;"> <div style="width: 45%;"> <p style="margin: 0;">buy \circ φ_1;</p> <p style="margin: 0;">buy(x);</p> <p style="margin: 0;">VP</p> </div> <div style="width: 45%; text-align: center;"> <p style="margin: 0;">⋮</p> <p style="margin: 0;">⋮</p> </div> </div>		
$\lambda\varphi_1$. buy \circ φ_1 ;	this \circ kind \circ of \circ sports \circ car;	
λx.buy (x);	ι (kind (spr));	
VP \uparrow NP	NP	
<div style="display: flex; justify-content: space-between;"> <div style="width: 60%;"> <p style="margin: 0;">buy \circ this \circ kind \circ of \circ sports \circ car;</p> <p style="margin: 0;">buy(ι(kind(spr)));</p> <p style="margin: 0;">VP</p> </div> <div style="width: 35%;"> <p style="margin: 0;">would;</p> <p style="margin: 0;">$\lambda P \lambda y$. WD $P(y)$;</p> <p style="margin: 0;">VP/VP</p> </div> </div>		
<p style="margin: 0;">would \circ buy \circ this \circ kind \circ of \circ sports \circ car;</p> <p style="margin: 0;">λy. WD buy(ι(kind(spr)))(y);</p> <p style="margin: 0;">VP</p>		

Figure 14: Pseudo-extraction antecedent without parallel extraction

in this section, no extraction from a subsequently deleted (or phonologically suppressed) subpart of some structural arrangement of linguistic expressions, as in a phrase structure tree. Rather, an auxiliary is licensed whose type and semantics correspond to a VP missing an NP, and which composes by hypothetical reasoning to the type of a clause missing an NP. A *wh*-operator along the lines proposed by Muskens can then take this clause as an argument. The appearance in (18) of an extraction from a subsequently ellipsed constituent is, on this view, an illusion due to the string-identity of a VP ellipsis on the one hand and displacement of a pseudogapping remnant on the other.

4 Conclusion: Peirce's linguistics, logic, and mathematics and the sources of type logical grammar

It is important at this point to consider how the results reported above have been achieved. Fundamentally, treatment of syntactic categories as valence specifications means that grammatical rules and operations can map the combinatorial possibilities of signs to different possibilities without ever requiring those possibilities to be realized as actual structures e.g., the operators for auxiliary type-shifting given above. But just as basic to this kind of solution is the fact that in

type-logical systems, the “categories” of phrase structure grammar are replaced by types which specify the argument requirements of their own arguments. The *what* operator discussed above can apply to a sign typed $S \downarrow NP$, an object itself seeking an NP to yield a clause of arbitrary depth. Since on the analysis in Section 3.1.2 the auxiliary *did* in (18) is a VP missing an NP and thus, by hypothetical reasoning, *Bill did* is an S missing an NP, a wh-operator such as *what* can take the latter as an argument without there ever having been any material in its licensing corresponding to the transitive verb *eat* per (31). The interpretation of (18) involves the sign *eat* only in the antecedent; in the ellipsis clause, the predicate *eat* is understood in the meaning only as a result of anaphoric retrieval from the antecedent clause. The heavy lifting in this proposal is carried out entirely by valence-shifting operators and the treatment of extraction as just one more instance of a dependency mediated by valence satisfaction.

The possibilities of this kind of framework depend on a RESIDUATED logic, i.e., a logic in which the connectives, viewed as type-constructors, have the property that, in the notation of classical implication (but necessarily modulo the directionality of the type-constructor slashes), and with \iff denoting metalogical equivalence, is shown in (33):

$$(33) \quad (\psi \vdash \psi \supset \varrho) \iff (\psi, \phi \vdash \varrho) \iff (\phi \vdash \psi \supset \varrho)$$

(For detailed discussion, see Restall 2018). Residuation is a property of the type-constructors $/, \backslash$ introduced in Lambek (1958), for all practical purposes the founding document of contemporary type-logical formalisms, and so far as type-logic is concerned, can be understood in the following way: there is a natural relationship between the entailment/equivalence relations in (33), whereby if inhabiting a given type τ_1 entails inhabiting some other type τ_2 , then $\tau_1 \vdash \tau_2$, i.e., $\vdash \tau_1 \rightarrow \tau_2$. Suppose that, given two types A, B , we can compose each member of A with each member of B to yield a term belonging to type C , i.e., $A \cdot B \vdash C$. Then necessarily every member of A belongs to the set of terms which form a member of C when they compose with a member of B on the right; if we call this set C/B then $A \vdash C/B$, and likewise for B . We thus have the relations

$$(34) \quad (A \cdot B \vdash C) \iff (A \vdash C/B) \iff (B \vdash A \backslash C)$$

(34) is nothing more than the residuated implication relationship of standard logic displayed in sequent notation. But as discussed at length in Pratt (1992), Peirce himself developed a theory of binary relations that incorporated the key components of residuated relationships between terms, including a kind of proto-version of the left and right “division” relations that, per (34), are formally entailed by each of the arguments of the type composition operator \cdot (and which are

in essence the upper adjoints of the monotone Galois connection which frames residuation in terms of partial orderings).¹⁶ As Pratt notes, the upper and lower adjoint operators are effectively the functions corresponding to the composition and division connectives (which Peirce wrote with a semicolon and a horizontal-line fraction notation respectively).

It seems fair to say, then, that – to extend Peirce’s original chemical metaphor only slightly – we can plausibly view Lambek’s seminal work in his 1958 and 1961 papers as the reaction product of an imagined catalyst bonding Peirce’s ideas about valence as the basis of linguistic combinatorics to his work on the algebra of relations. Any doubt about the correctness of such a view should be immediately dispelled by Lambek’s own words; in one of his papers on pregroup grammars – an algebraic reformulation of type-logical grammar he developed in order to make transparent the logical foundation of his earlier systems as instances of (a fragment of) intuitionistic noncommutative linear logic – he comments of a very basic skeleton for the pregroup grammar formalism that the essential combinatorics “may even be implicit in the ideas of C.S. Peirce [i.e. Peirce (1897) – RDL]”, noting that certain combinators in this “rudimentary” version may have been seen by Peirce as comparable to “the unsaturated bonds of an atom. *I believe pregroup grammars developed from this rudimentary setup.*” (Lambek 2007: 352; emphasis added).

The system exhibited in (4) combines Lambek’s earliest formulation of a type-based logic for linguistic composition with the version of type-logic developed in Oehrle (1994); but note that Oehrle’s system is presented as itself an outcome of enriching the associated type-logic of Lambek’s (1958) paper with the structural rule of permutation; this of course then requires word order to be somehow separated from type combinatorics, and Oehrle’s own deep insight was to allow the prosody to contain functional operators. It is not unreasonable to see Lambek’s 1958 paper as the fountainhead for the two separate research traditions that have developed under the broad heading of type-logical grammar, and as I hope to have made clear, Peirce’s work in both linguistics and the algebra of relations had already provided the materials for Lambek’s profound synthesis, as Lambek himself stressed. It is to be hoped that future overviews of the history of type-logical systems along the lines of e.g. Moortgat (2014) will take due note of

¹⁶Specifically, assume that for any two types A, B , $A \cdot B \leq C$, i.e., every inhabitant of the concatenation of the types A, B is an inhabitant of C . Then with $f_* = \lambda\alpha.\alpha \cdot B$ and $f^* = \lambda\beta.\beta/B$, there is a Galois connection between f_* and f^* iff $f_*(A) \leq C \Leftrightarrow A \leq f^*(C)$, which, if we also define an upper adjoint $f^{**} = \lambda\gamma.A \setminus \gamma$, and take the entailment relation $X \vdash Y$ to define a partial ordering $X \leq Y$, gives us exactly the “trivalence” in (34).

Peirce's right to ancestral status in the lineage of the Lambek calculus, and therefore of all contemporary versions of type-logical grammar. And it strikes me as extremely likely that Peirce would have been particularly glad had he known the degree to which his key linguistic principles – valence satisfaction as the driver of grammatical composition and language as an extension of logic – would be unified so precisely and rigorously in Lambek's brilliant fusion of developments in logic and mathematics that can be traced, to a large extent, back to Peirce himself.

Acknowledgments

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Chapter 8

Hierarchical syntactic structure in Malay/Indonesian, between Pirahã and Had Gadya

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This paper presents an exploration of hierarchical syntactic structure in Malay/Indonesian. An analysis by Jackendoff & Wittenberg suggests that the Riau dialect of Indonesian may, like Pirahã, lack syntactic recursion. This paper poses the question whether Riau Indonesian is syntactically recursive, and answers “yes and no”. In Riau Indonesian, the grammar does indeed permit the formation of sentences with arbitrarily many levels of embedding; in this respect, it appears to exhibit syntactic recursion. However, in Riau Indonesian, the use of embedding is vanishingly rare: in a preliminary study involving comparable language registers, the frequency of embedding is found to be several orders of magnitude less in Malay/Indonesian than in English. To represent the status of recursion in Malay/Indonesian, reference must be made not only to grammar but also to the ways in which speakers choose to express their conceptualization of reality: linguistic perspective. A rough and ready indication of the propensity for a language to make use of syntactic recursion is provided by an examination of cumulative tales such as the Aramaic/Hebrew Passover song Had Gadya. Such tales occur throughout the world; however, while their semantics is recursive, their syntax is often flat and concatenative, a preliminary survey suggesting that the distribution of syntactically recursive cumulative tales may be limited to a single area centered on Europe and the Middle East. The results of this paper suggest that syntactic recursion is more likely to be found in languages spoken by communities of greater societal complexity, with Malay/Indonesian occupying an intermediate position in this respect, between Pirahã and Standard Average European.



1 Introduction

Dan Everett's main claim to fame, among linguists at least, is describing a language, Pirahã, arguably lacking many of the supposedly necessary design features of contemporary human language, such as numerals and quantifiers, colour terms, reference to things and events outside immediate experience, and of course, the cherry on the top, syntactic recursion (Everett 2005). Meanwhile, on the other side of the world, far from the spotlight, I have worked on describing the Riau dialect of Indonesian, which turns out to be lacking in a rather different set of core features, including clearly-defined words, distinct open syntactic categories, systematic encoding of thematic roles, and a grammaticalized distinction between attribution and predication (Gil 2005, 2006, 2012, 2013, 2017, 2020). This festschrift to Dan Everett presents an ideal opportunity to indulge in a contrastive analysis of the two languages – to see what happens when Pirahã meets Riau Indonesian. With neither the time nor the space to deal with the two languages in their entirety, I shall cut to the chase and focus on that most renowned feature associated with Dan Everett's work, namely syntactic recursion.

2 Is Riau Indonesian syntactically recursive?

In earlier conversations, Dan Everett (p.c.) suggested that Riau Indonesian may also be a language lacking in syntactic recursion. However, in his writings, for example Futrell et al. (2016), Everett (2017), he makes it clear that he bases this position on the analyses proposed by Jackendoff & Wittenberg (2014, 2017), who characterize Riau Indonesian as instantiating their class of Multi-Word Phrase Grammars, in which words group together to form phrases, and phrases group together to form utterances, but without any possibilities for syntactic recursion. Since Jackendoff & Wittenberg (2014, 2017) themselves base their analysis on my own descriptions, let us now take a look at what I have written on this score, and also some additional facts that I have not yet had the opportunity to describe in print. Spoiler alert: The answer to the question “Is Riau Indonesian syntactically recursive?” is “yes and no”.

Indeed, in Riau Indonesian, the grammar permits the formation of sentences such as the following:

- (1) Ali pukul orang yang suka anjing yang kejar anak.
Ali hit person PRTC like dog PRTC chase child
'Ali hit the person who likes the dog that chased the child.'

Moreover, if one really wants to, one can go on adding relative clauses at the end of the construction indefinitely. So in this respect, Riau Indonesian is clearly syntactically recursive, like English, not like Pirahã.

In previous work, my focus was largely on the grammar of simple word combinations, such as *ayam makan* (chicken eat), and thus, while making it clear that combinations of words can be grouped together to form ever larger combinations, this particular aspect of Riau Indonesian was not emphasized. This, then, is the appropriate time to redress the balance, by presenting an explicit analysis of hierarchical structure in Riau Indonesian. A syntactic analysis of sentence (1), following the theoretical framework laid out in Gil (2000), is presented in Figure 1.

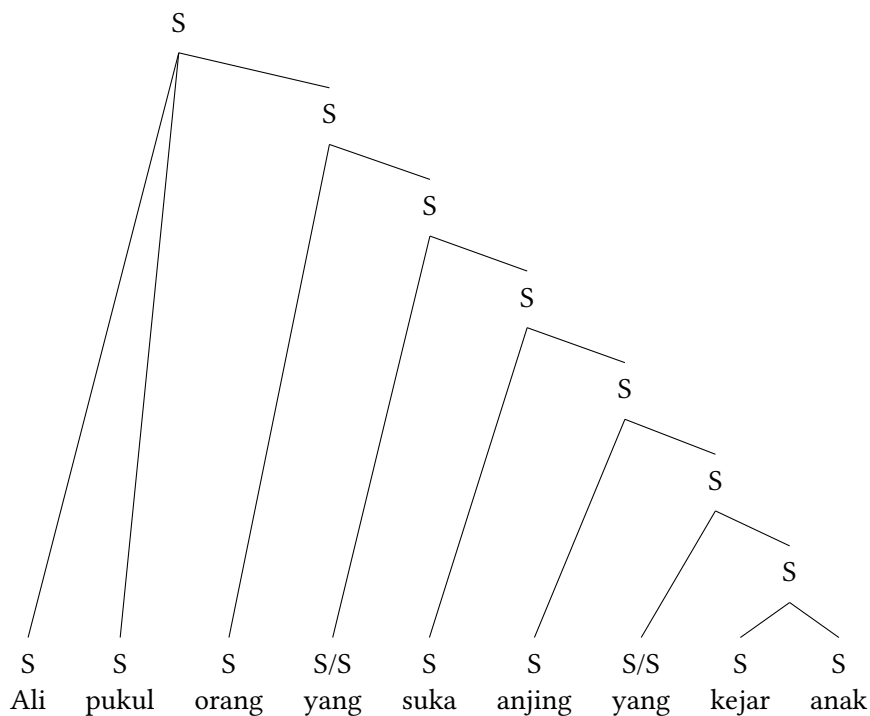


Figure 1: Syntactic structure of (1)

In Riau Indonesian, almost all words belong to the single open syntactic category S, for Sentence; this includes, among others, words denoting things, such as *anak* 'child', as well as words denoting activities, such as *kejar* 'chase'. The combination of the two, as in *kejar anak* above, is thus an instance of sentential

coordination. Similarly, complex Ss may combine with other simple or complex Ss to yield hierarchical structures such as that in Figure 1. While most words belong to the category S, a handful of grammatical items belong to a closed class, S/S, whose name, following the conventions of categorial grammar, specifies that they cannot occur on their own but rather must combine with Ss to yield superordinate Ss. Figure 1 contains two occurrences of the S/S word *yang*; in one case it combines with the S expression *kejar anak* to yield a superordinate S expression *yang kejar anak*, while in the other case it takes the S expression *suka anjing yang kejar anak* to yield a superordinate S expression *yang suka anjing yang kejar anak*. As suggested by the above, the syntax of Riau Indonesian is thus fully recursive, allowing for hierarchical syntactic structures of arbitrary depth.

As shown in Figure 2 below, the basic compositional semantics of sentence (1) makes reference to a hierarchical structure that is completely isomorphic to that of its syntax.

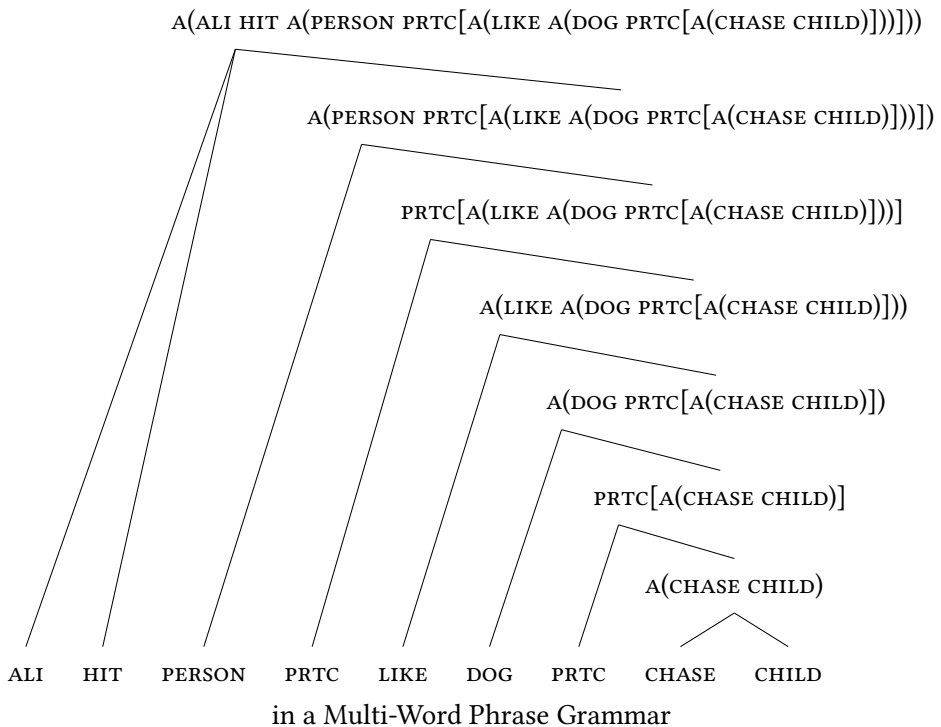


Figure 2: Structure of (1)

As represented in Figure 2, the semantics of sentence (1) makes reference to two operators. First and foremost is the association operator A, which underlies the

overwhelming majority of the compositional semantics. In its polyadic guise, illustrated above, it is a generalization of the monadic association operator, familiar as a semantic representation for genitive markers and various other possessive and associative constructions in many languages. Applying to two meanings, x and y , $A(x, y)$ simply means ‘entity associated with x and y ’, or ‘something to do with x and y ’. For example, in the above representation, $A(\text{CHASE CHILD})$, the meaning of *kejar anak*, means ‘entity associated with chase and child’; among many other things, it is unspecified for a variety of semantic categories such as number, definiteness, tense-aspect, and thematic role assignment (e.g. whether the child is the agent or patient of the chasing). The second operator is the participant operator PRTC , which, when applied to a meaning x , creates a new meaning $\text{PRTC}(x)$, picking out a participant in the semantic frame of x . The participant operator underlies the semantic representation of grammatical markers in several languages, which, under alternative analyses, are sometimes characterized as relativizers, nominalizers or reifiers. For example, in the above representation, $\text{PRTC}[A(\text{CHASE CHILD})]$ denotes a participant associated with the entity associated with *chase* and *child*, without any further specification of thematic role (i.e. whether it is the agent, the patient, or perhaps some other thematic role). The above analysis captures the fact that sentence (1) is massively underspecified with respect to categories such as number, definiteness, tense-aspect and thematic roles, with respect to which the English translation in (1), ‘Ali hit the person who likes the dog that chased the child’, is just one of myriad alternative available translations.

The above analysis may be contrasted with the alternative approach proposed by Jackendoff & Wittenberg (2014, 2017). One feature shared by both approaches is monocategoriality, the claim that Riau Indonesian has but a single open syntactic category. However, there are at least two significant differences between the two approaches. One pertains to the semantics, with respect to which Jackendoff & Wittenberg (2014, 2017) propose a more conventional analysis, based not on the association operator but rather on the familiar predicate-argument relationship. However, it is the second difference between the two approaches, pertaining to the syntax, that is of relevance to us here. In contrast to the above analysis, Jackendoff & Wittenberg (2014, 2017) assign Riau Indonesian to their class of Multi-Word Phrase Grammars, whose structures are flatter and non-recursive, making reference to just three categories, $W(\text{ord})$, $P(\text{hrase})$ and $U(\text{tterance})$. In fact, in their view, even these categories are more appropriately viewed as phonological, or prosodic, rather than syntactic. An analysis of sentence (1) in accordance with Jackendoff & Wittenberg (2014, 2017), whose approach is presented in Figure 3.

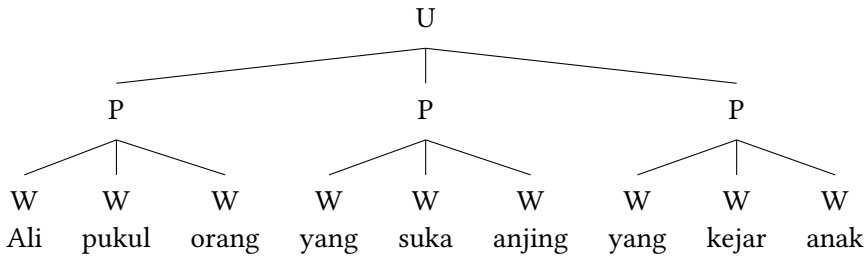


Figure 3: Semantic structure of (1)

In accordance with the analysis in Figure 3, sentence (1) does not exhibit multiple layers of hierarchical structure; instead it consists of a flat sequence of phrases, each consisting of a flat sequence of words. For Jackendoff & Wittenberg (2014, 2017), the obviously hierarchical nature of sentence (1) is a fact about its semantics, not its syntax.

How might one adjudicate between the alternative analyses of sentence (1) as represented in Figures 1 and 3 respectively? Given the flexibility of Riau Indonesian syntax as described in my earlier publications, Jackendoff & Wittenberg had good *prima facie* reason to invoke Occam’s Razor and assign Riau Indonesian to their class of Multi-Word Phrase Grammars, entailing analyses such as that represented in Figure 3. Thus, with reference to Riau Indonesian, Jackendoff & Wittenberg (p.c.) argue that:

its grammar relates the semantic structure of sentences directly to linear order and prosodic constituency within phonology, without the intervention of syntax. This sort of grammar relies on principles such as Behaghel’s First Law [...]. Agent precedes Patient, and Modifier precedes (or follows) Modified. None of these requires syntax.

Nevertheless, additional evidence suggests that the flat structures of Multi-Word Phrase Grammars are insufficient to account for the totality of facts pertaining to the syntax of Riau Indonesian.

As alluded to in the above passage by Jackendoff & Wittenberg (p.c.), one of the core characteristics of Riau Indonesian sentence structure is the extent, greater than in many other languages, to which it upholds Behaghel’s First Law, which, paraphrasing slightly, states that expressions whose meanings are closer to each other in conceptual space tend to occur closer to each other within the syntactic structure of the sentence (Behaghel 1932). One manifestation of the strongly

Behaghelian nature of Riau Indonesian is the relative infrequency with which expressions occur outside of their “expected” positions – the kinds of constructions that, within some theoretical frameworks, are often accounted for in terms of various kinds of movement rules. In the case at hand, Behaghel’s First Law provides clear-cut and unambivalent motivation for several of the intermediate levels of syntactic structure posited by the analysis in Figure 1 but absent from the flatter structure posited by the analysis in Figure 3.

Consider, for example, the substring of words *anjing yang kejar anak* (dog PRTC chase child) in (1), which forms a constituent in Figure 1 but not in Figure 3. This constituency reflects the fact that these four words are close to each other semantically, *anjing* and *anak* being participants in the activity denoted by *kejar*. In doing so, it makes correct predictions about possible reorderings of the words in (1). For example, it correctly predicts that *anjing yang kejar anak* can be moved, as a single chunk, to the beginning of the sentence, as in (2) below:

- (2) Anjing yang kejar anak Ali pukul orang yang suka.
 dog PRTC chase child Ali hit person PRTC like
 ‘The dog that chased the child, Ali hit the person who likes it.’

Conversely, it also correctly predicts that *yang suka anjing*, a constituent in Figure 3 but not in Figure 1, cannot be moved to the beginning of the sentence, as in (3) below, without a massive change in meaning:

- (3) Yang suka anjing Ali pukul orang yang kejar anak.
 PRTC like dog Ali hit person PRTC chase child
 ‘As for the one who likes the dog, Ali hit the person who chased the child.’
 * ‘Ali hit the person who likes the dog that chased the child.’ [= (1)]

As indicated above, the interpretation associated with sentence (1) is completely unavailable in (3). More dramatically, the hierarchical syntactic structure of Figure 1 predicts the total unacceptability, *salva veritate*, of various random reshufflings of the words in (1) such as in (4) below:

- (4) Orang pukul anjing yang suka anak yang kejar Ali.
 person hit dog PRTC like child PRTC chase Ali
 ‘A person hit the dog that likes the child who chased Ali.’
 * ‘Ali hit the person who likes the dog that chased the child.’ [= (1)]

Again, the interpretation associated with sentence (1) is completely unavailable in (4). Thus, as shown above, Riau Indonesian has nothing of the freedom of, say,

a language like Warlpiri, for which Hale (1979, 1983) posits a flat “W-star” grammar. Instead, the strongly Behagelian nature of Riau Indonesian, compensating for its flexibility and indeterminacy in various other domains, provides strong support for hierarchical syntactic structures of the kind represented in Figure 1, and in doing so for the presence of syntactic recursion in Riau Indonesian.

Jackendoff & Wittenberg (p.c.) propose to account for facts similar to these by positing a “Contiguity Condition”, whose effect is to ensure that what they call “semantic constituents” must be expressed by contiguous strings of words. For example, in order to account for the inseparability of the string *anjing yang kejar anak*, as evidenced in (1–4) above, they would invoke the Contiguity Condition to ensure that the semantic constituent A(DOG PRTC[A(CHASE CHILD)]) is realized by a contiguous string of words. Crucially, according to Jackendoff & Wittenberg, such strings do not form syntactic constituents; rather, they are semantic constituents that are then mapped onto phonological ones. Their analysis is in accordance with their general agenda, which is to avoid reference to syntax unless the phenomenon under question cannot be accounted for with reference to either semantics or phonology. While in general I am supportive of their agenda, in the case at hand there is an obvious problem, namely, their assumption that contiguity is a fact about phonology. While in some cases, indeed, contiguous words may interact phonologically, for example by forming an intonational phrase, this is not true more generally; for example, in (1), the string *anjing yang kejar anak*, argued in (2) to form a syntactic constituent, does not constitute a complete intonational phrase. Rather, contiguity must be viewed as a syntactic property, indeed perhaps the most quintessential one, pertaining to what syntax is all about, namely, the bringing together of expressions in order to constitute larger expressions. Thus, pace Jackendoff & Wittenberg, the strongly Behagelian nature of Riau Indonesian, as exemplified by the above examples, does indeed support the kind of hierarchical syntactic structure illustrated in Figure 1, and in so doing also the presence of syntactic recursion in Riau Indonesian.

With respect to the presence of syntactic recursion, then, Riau Indonesian is more like English than like Pirahã. But still, this is only half the story. A more fully adequate account of recursion in Riau Indonesian must acknowledge the fact that sentences such as in (1) are highly artificial, and that nobody actually speaks that way. Exactly what it means to say that nobody speaks that way is taken up in the next section.

3 Linguistic perspective (or, what it means to know a language)

Among the scholars who have addressed the broader implications of my analysis of Riau Indonesian is James Hurford. In Hurford (2011: 410–413), an email conversation is reproduced, in which the author asks me various questions about the apparent extreme simplicity of Riau Indonesian and its implications for the evolution of language. Towards the end of the conversation, the following exchange takes place:

Hurford: [If Riau Indonesian grammar is as simple as you describe,] what is there to learn, beside vocabulary? How come you need a full-time teacher?

Gil: The grammar, in the narrow Chomskyan sense of ‘set of well-formed strings’, can be learned in less than an hour. But still, in order to be able to be mistaken for a native speaker down a dark alley, you’d need to spend years learning: lexicon, phonetics, and, most interestingly, that nebulous domain that is sometimes referred to as idiomaticity – being able to say something that is not just grammatical but also stylistically felicitous in the appropriate context.

Hurford: I’m pondering what you mean by “idiomaticity” ...

Hurford’s trouble with the term “idiomaticity” is understandable, as I was not very clear back then with regard to what I meant by this particular term. This, then, is the appropriate place to try and clarify matters.

The notion in question is one that has been put forward, in various guises, by a number of different scholars. Grace (1987) talks of the “linguistic construction of reality”, Pawley (1993) – from whom I adapted the above term – refers to “idiomatic competence”, Slobin (1996: 91) alludes to a “subjective orientation of the world of experience”, while Ross (2001) discusses the different “ways of saying things” associated with different languages. In this paper I propose the term “linguistic perspective” – see Gil (2023) for a detailed application of this notion to the field of diachronic syntax and language contact.

The leading idea is as follows. The reality in which we find ourselves is of overwhelming complexity, as also is our conceptualization of it, which is what we express by means of language. However, any natural human language can only express a small proportion of this complexity. Hence, using language involves choosing which aspects of our conceptualization of reality are worthy of expression, and which others are to be left unexpressed. Or, in other words, adopting a

perspective. While in some instances, such choices involve on-the-fly decisions by individual speakers, in many other cases, these choices are conventionalized, at the level of the language and the community of which it forms part. And of course, different languages and the communities in which they are embedded conventionalize different choices, as a result of which they may be said to be associated with different linguistic perspectives.

Some of the differences in linguistic perspective between Riau Indonesian and English may be illustrated with reference to an everyday situation, in which you are walking down the street with your friend and notice that he has just dropped his wallet. What might you say? Here are two natural utterances in Riau Indonesian and English respectively:

- (5) Dompét jatuh bang.
wallet fall HYP/elder.brother
- (6) You dropped your wallet.

While the situation is the same, the two languages choose to express different aspects of it; they adopt different perspectives. In Riau Indonesian, interpersonal relationships feature prominently, and are typically expressed by kinship terms, such as the hypocoristic form *bang* in (5). In contrast, in English, concepts such as time and number are commonly encoded, as is exemplified in (6) by the past-tense *-ed* suffix on *drop* and the absence of a plural marker on *wallet*; in addition, the notions of participant and possession are also expressed by means of the pronominal forms *you* and *your*. Although the expression of these different perspectives makes use of lexicon and grammar, the perspectives themselves are not part of the lexicon or the grammar, but rather a completely separate component of the language, which speakers have to master in order to be able to speak the language properly.

The independence of linguistic perspective from lexicon and grammar can be seen most clearly by consideration of the following variants of (5) and (6), in which the utterances are couched in the perspective of the “wrong” language:

- (7) # Kamu tadi jatuh dompet kamu satu.
2 PST.PROX fall wallet 2 one
- (8) # Wallet drop bro.

The strangeness of examples (7) and (8) is indicated above by the use of the symbol #. Sentence (7) in Riau Indonesian is as precise a rendition as is possible of sentence (6) in English, leaving out the term of address, but including expression of time, with *tadi*; number, with the numeral *satu*, participant, with the

pronoun *kamu*, and possession, with the second occurrence of *kamu* in a possessive construction. Sentence (7) is perfectly grammatical in Riau Indonesian, but is hopelessly long-winded; nobody would ever say anything like that in real life. Conversely, sentence (8) in English is perhaps as close as one can get to sentence (5) in Riau Indonesian, leaving out the expression of time, number, participant and possession, but including instead the kinship term *bro*. While the bare verb *drop* is of marginal grammaticality in the given syntactic environment, a further major problem in (8) involves the use of inappropriate and clashing registers: whereas the bare noun phrase *wallet* is associated with the telegraphic language of newspaper headlines and the like, the address term *bro* is restricted in its occurrence to certain speech styles of particular subcultures. In summary, what is strange about sentence (7) and in large part also (8) is a matter of linguistic perspective, not lexicon or grammar.

The notion of linguistic perspective provides the basis for a proper understanding of how Riau Indonesian can be syntactically recursive in one respect but not in another. Specifically, sentence (1) is strange in a similar way to sentences (7) and (8) above; it should also have been marked with a #. While illustrating the recursive potential of Riau Indonesian syntax, it violates the linguistic perspective of Riau Indonesian. Specifically, in Riau Indonesian, speakers systematically choose not to afford overt morphosyntactic expression to the kind of logical subordination that is commonly expressed in English by multiple syntactic embedding. Thus, in a situation that might prompt a speaker of English to say something like ‘Ali hit the person who likes the dog that chased the child’, a speaker of Riau Indonesian is much less likely to produce a sentence such as (1), and instead more likely to produce one that might broadly resemble the following:

- (9) Ada anak kan, dia kejar anjing, terus ada orang suka anjing
 exist child Q 3 chase dog continue exist person like dog
 tu, orangnya kena pukul Ali.
 DEM.PROX person:ASSOC undergo hit Ali
 ‘There was a child, right, he was chased by a dog, then there was a man
 who liked the dog, and the man got hit by Ali’.

Sentence (9) above consists of four clauses strung out one after the other in a loose paratactic relationship, in which the logical subordination expressed in English, as well as in the bizarre Riau Indonesian sentence (1), by means of syntactic embedding, is here manifest mostly by relationships of coreference between pairs of expressions, namely *anak* and *dia*, *anjing* and *anjing tu*, and *orang* and *orangnya*, with just a single instance of embedding, in which the expression *suka*

anjing tu is subordinate to *ada orang*. Thus, the reluctance of Riau Indonesian speakers to make use of the grammatical devices available to express relationships of multiple logical subordination may be viewed as another aspect of the linguistic perspective of Riau Indonesian, determining which aspects of reality are linguistically encoded and which others are left unexpressed.

The contrasting linguistic perspectives of Riau Indonesian and English may be represented diagrammatically as in Figure 4.

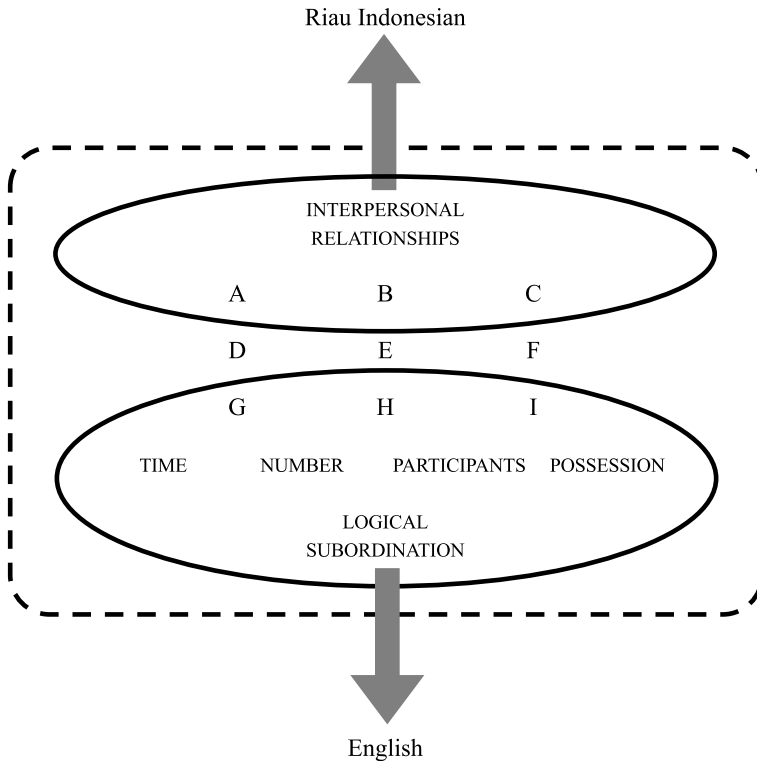


Figure 4: Linguistic perspective in Riau Indonesian and English

In Figure 4, the area enclosed by the dashed line represents our conceptualization of reality. Within it, represented in small caps, are a variety of individual aspects of our conceptualization of reality which may potentially be expressed in language. While some of these aspects are the ones discussed above, a potentially much larger number of other aspects are suggested by the letters A–I. The two ovals show how Riau Indonesian and English select different subsets of these variegated aspects of reality; they thus represent the contrasting linguistic perspectives of the two languages. In particular, they capture the fact that even

though both languages possess formal devices for the expression of logical subordination, actually using them to form multiple embeddings is something that speakers of English often do while speakers of Riau Indonesian typically do not.

The preceding discussion thus provides an answer to Jim Hurford's question posed at the beginning of this section, namely, what was meant by "idiomaticity". In doing so, it underscores the fact that knowing a language involves mastery not only of lexicon, phonetics, phonology, morphology, syntax, semantics and discourse/pragmatics, but, crucially, also linguistic perspective. In particular, in order to be able to speak Riau Indonesian properly, it is not enough to know how to construct multiple embeddings; in addition, one must also know that the actual use of such multiple embeddings is, in almost all cases, inappropriate – it's just not the Riau Indonesian way of speaking.

4 Quantitative comparison

It may reasonably be objected that it is unfair to compare a standardized language such as English with a colloquial variety such as Riau Indonesian. After all, there is substantial evidence that within many a language, subordination occurs more frequently in higher registers than in lower ones, and in written as opposed to oral modalities (Givón 1979, Deutscher 2000, Karlsson 2009a,b, and others). Indeed, corpora of spoken English are also likely to contain constructions similar to the English rendition of (9), 'There was a child, right, he was chased by a dog, then there was a man who liked the dog, and the man got hit by Ali'.

Nevertheless, abstracting away from such language-internal variation, English as a whole is much more tolerant of multiple embedding than Malay/Indonesian, the macro-language of which Riau Indonesian is just one typical exemplar. In order to support this claim empirically, a rough and ready corpus study was conducted making use of Google searches. In both English and Malay/Indonesian, Google searches produce material representative not only of a range of different dialects, but also of a variety of registers ranging from the colloquial language of social media to the more formal language of written works. Thus, it is reasonable to believe that comparing the results of Google searches in English and Malay/Indonesian abstracts away not only from modality effects but also from the effects of language-internal register-governed variation; such searches may thus potentially offer a fair and well-founded comparison of the two languages.

Some results of a comparative corpus study of English and Malay/Indonesian are presented in Tables 1–4 below.

Tables 1–4 present two comparisons of English and Malay/Indonesian, the first in Tables 1 and 2 and the second in Tables 3 and 4. In Tables 1–4, the first column

Table 1: First comparison: English

he thinks he said	13,840,000	
he	4,870,000,000	7×10^{-4}
thinks	4,260,000,000	9×10^{-4}
said	4,560,000,000	8×10^{-4}

Table 2: First comparison: Malay/Indonesian

dia pikir dia bilang	7	
dia (3SG)	4,060,000,000	2×10^{-9}
pikir (think)	38,000,000	2×10^{-7}
bilang (say)	1,210,000,000	6×10^{-9}

Table 3: Second comparison: English

he said he thinks	10,200,000	
he	4,870,000,000	2×10^{-3}
thinks	4,260,000,000	2×10^{-3}
said	4,560,000,000	2×10^{-3}

Table 4: Second comparison: Malay/Indonesian

dia bilang dia pikir	37,800	
dia (3SG)	4,060,000,000	9×10^{-6}
pikir (think)	38,000,000	1×10^{-3}
bilang (say)	1,210,000,000	3×10^{-5}

presents the search criterion, and the second column the rough number of hits (accessed on the 5th of June 2022). Within each table, the first row presents a string, part of a complex construction which, when adding some following text, involves two levels of embedding, while the subsequent rows present the individual words occurring within the complex construction.

By examining the ratio of hits for the complex construction in the first line to that of the individual words in the subsequent lines, it is possible to abstract away both from the different sizes of the two corpora and also from possible frequency effects associated with the individual words, and in so doing measure the propensity of the language to form multiple embeddings making use of the words in question. These ratios are presented in rounded form in the third column of each table. For example, in Table 1, the ratio of *he* in line 2 to *he thinks he said* in line 1 is $3,840,000/4,870,000,000 = 7 \times 10^{-4}$. On its own, this figure does not mean much; what is significant is its comparison to the corresponding figure in Table 2, in which the the ratio of *dia* in line 2 to *dia pikir dia bilang* in line 1 is $7/4,060,000,000 = 2 \times 10^{-9}$. Comparing these two figures, 7×10^{-4} and 2×10^{-9} , shows that with respect to the particular words chosen, English is 4 to 5 orders of magnitude more likely to form the multiple embedding construction than Malay/Indonesian. Comparable order-of-magnitude differences are present for the remaining five comparisons between Tables 1 and 2, and for five out of six of the corresponding comparisons between Tables 3 and 4 – the only case where there is not a order-of-magnitude difference being in the third lines of Tables 3 and 4, where the English is “only” about twice as likely to form a multiple embedding construction than the Malay/Indonesian.

Thus, Tables 1–4 show that English as a whole is massively more likely to form multiple embedding constructions than Malay/Indonesian. In yet another comparison aimed at testing this generalization, the English string *who do you think is going to win* yielded a total of 841,000 hits, while there were no hits whatsoever for numerous potential equivalent sentences in Malay/Indonesian, the only exception being 3 hits for *siapa anda pikir akan menang* (who 2 think FUT win), which actually occurred in an Indonesian website explaining the meaning of a similar English sentence – the exception that proves the rule.

The above Google searches show that multiple embedding is massively more common in English than in Malay/Indonesian. The similar nature of the searches in the two languages shows that the greater propensity for subordination in English as compared to Malay/Indonesian is a cross-linguistic difference that is independent of both register and modality. In particular, the more widespread use of embedding in English is observable notwithstanding a significant body of literature (Karlsson 2007b,a, 2009a,b) showing that such constructions are more

highly constrained, and occur less frequently in real-life language use, than is commonly assumed to be the case under a simplistic characterization of English and other similar languages as syntactically recursive.

The results of the comparison between syntactic recursion in Malay/Indonesian and English is summarized in Figure 5.

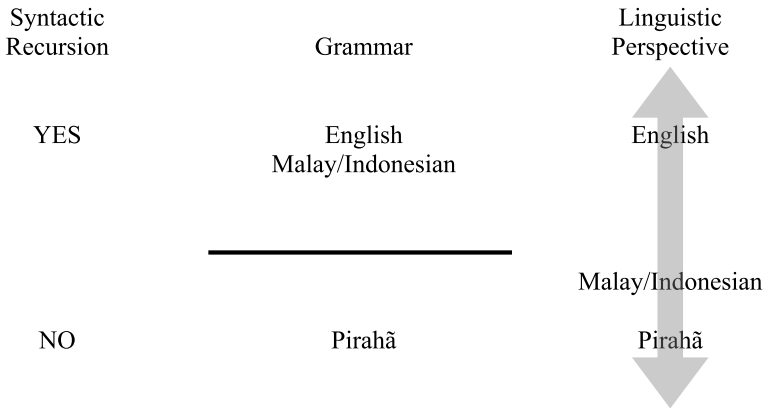


Figure 5: Recursion in Riau Indonesian and English

In terms of grammar, the distinction between having recursion and not having it is categorical; Malay/Indonesian (including Riau Indonesian) like English, has it, while Pirahã does not. In contrast, with respect to linguistic perspective, the propensity for syntactic subordination and the use of recursive strategies to effect such subordination is a scalar property; while English makes frequent use of such embedding, Malay/Indonesian does not, though perhaps – this remains an open question – not to quite the same extent as Pirahã. Thus, Figure 5 shows why the question posed earlier, whether Riau Indonesian has syntactic recursion, is appropriately answered with a “yes and no”. Answering the question in this way underscores the importance, in any description of a language, not only of its lexicon and grammar, the things that one can say, but also of its linguistic perspective, the things that one actually does say.

5 Towards a cross-linguistic typology

How do other languages fit into the above schema: are more languages like English, like Malay/Indonesian, or like Pirahã? In addition, since linguistic perspective offers a scalar rather than a categorical take on recursion, there is also the potential for languages to occupy other points on the scale, possibly higher

than English, or in-between English and Malay/Indonesian, with respect to their propensity for multiple embedding and their associated degrees of recursion.

At present, we simply do not know enough to provide a systematic answer to the above question. Recursion is in the eyes of the beholder, with different analyses pointing towards alternative conclusions with regard to whether a given construction constitutes an instantiation of subordination or not. If linguist A says that language X is syntactically recursive, while linguist B argues that language Y is not, then is this a real difference between the two languages, or merely a difference in the ways the two linguists choose to pursue their trade? Indeed, for many individual languages, different linguists offer different answers to the question whether the language is syntactically recursive, as we saw earlier with the contrasting analyses of Riau Indonesian offered by myself and by Jackendoff & Wittenberg. In fact, in some cases, it is not different linguists proposing different analyses but the same linguist modifying their views over time, an apparent instance of this being Dan Everett on Pirahã, as described in detail by Pullum (2024), Chapter 2 of this volume.

As reflected by the evolution of Everett's writings on Pirahã, whether or not linguists choose to analyze a particular language as exhibiting syntactic recursion may be affected in a systematic fashion by their methodology, and, in particular, the relative weights that they attribute to data deriving from elicitation as opposed to naturalistic corpora. In general, elicitation is more likely to lead to insights into grammar, and what people *can* say, whereas work based on naturalistic corpora stands a greater chance of yielding a better understanding of linguistic perspective, and what people actually *do* say. Thus, elicitation, and asking speakers whether they would be willing to accept a long and unwieldy construction, is more conducive to the positing of syntactic recursion, whereas observation of naturalistic texts, in which such constructions may occur rarely, if at all, is more likely to lead to a claim to the effect that syntactic recursion is absent. In particular, claims by scholars such as Sandalo et al. (2018), and indeed for that matter also the early Everett (1986), to the effect that the grammar of Pirahã has at least some syntactic subordination, tend to be the products of elicited data, motivated by theoretical concerns, and as shown in detail by Pullum (2024), Chapter 2 of this volume, a desire to fit the language into a predetermined descriptive template. In contrast, the later and more famous claims by Dan Everett that Pirahã lacks syntactic recursion are mostly based on the use of naturalistic data. While for Pirahã it may indeed be the case, as argued by Pullum (2024), Chapter 2 of this volume, that the work based on elicitation is of inferior quality to the work based on naturalistic data, this is a contingent fact and not an inherent property of elicitation as opposed to the use of naturalistic corpora – there

can be good or bad work based on elicitation just as there can be good or bad work making use of naturalistic data. Thus, in view of the diverse methodologies underlying the available descriptions and analyses of different languages, it is not yet possible to paint a systematic picture of cross-linguistic variation with regard to syntactic recursion, but only to offer some observations and conjectures.

To begin with, one may ask whether there are languages in which syntactic recursion is even more prevalent than in English. One phenomenon that comes to mind is that of clause chaining in many of the languages of New Guinea, as described by Foley (2010) and others. In such languages, a sequence of several monoclausal sentences in English are rendered in the form of a single complex clause, in which all but the last of the clauses constitute a chain of embedded clauses marked by various morphosyntactic devices as subordinate. However, as suggested by the term “chaining”, the non-final clauses are all of equal status to one another, and therefore do not exhibit the kind of multiple embedding that is of concern to us here.

A perhaps more promising case is argued by Cysouw (2023) to be provided by German, as illustrated by sentences such as the following (from *Juristische Schulung, Zeitschrift für Studium und Referendariat*, 10/2012, Verlag C.H. Beck, p. 866; the English translation was provided by Boban Arsenijević with the assistance of DeepL Translate.):

- (10) Solange sich der Gläubiger noch durch die nachgeholte Leistung in Natur, ggf. ergänzt durch den Ersatz von Verzögerungs- und sonstigen Schäden, vollständig in die Lage versetzen lässt, in der er sich bei pflichtgemäßer Leistung befände, und die Leistung für den Schuldner weniger kostspielig ist als die Zahlung von Schadensersatz statt der Leistung, gibt es keinen Grund, dem Gläubiger von vornherein die Entscheidung zwischen Erfüllung in Natur und Schadensersatz zu überlassen und ihm zu erlauben, dem Schuldner durch die Wahl des Schadensersatzes den Kostenvorteil der Leistungserbringung zu nehmen.

‘As long as the creditor can still be fully put in the position he would be in if he had performed dutifully, by the subsequent performance in nature, supplemented, if necessary, by the compensation for delay and other damages, and the performance is less costly for the debtor than the payment of damages instead of performance, there is no reason to leave the creditor the choice between performance in nature and damages from the outset and to allow him to deprive the debtor of the cost advantage by choosing the way of damage compensation.’

According to Cysouw (2023), such sentences, complex to the point of unintelligibility for the average reader, are more common in German than in English.

To the extent that this observation holds water, it would seem to indicate that German may be characterized by a greater propensity for multiple embedding than English. Some further discussion of stylistic variation with respect to the propensity for various kinds of subordination in the languages of Europe can be found in Karlsson (2007b,a), suggesting, *inter alia*, that contemporary English might represent the outcome of recent processes of reduction in the extent to which such constructions, involving multiple embedding, are used.

At the other end of the scale represented in Figure 5 are languages whose grammars do not allow syntactic recursion, or whose linguistic perspectives and associated patterns of usage disfavour it. Again, one may ask whether and to what extent Malay/Indonesian and Pirahã are weird outliers, and the answer, surprising perhaps only to those whose primary concern is English and its representation within certain contemporary syntactic theories, is that they are not at all exceptional. Thus, Givón (1979: 298) writes that “there are some languages extant to this day – all in preindustrial, illiterate societies with relatively small, homogenous social units – where one could demonstrate that subordination does not really exist ...”. Givón’s assertion is cited approvingly by Pullum & Scholz (2010) and Pullum (2024), Chapter 2 of this volume, who go on to adduce several descriptions of languages reported as lacking subordination, among which are Amazonian languages such as Hixkaryana (Derbyshire 1979) and Apalaí (Koehn & Koehn 1986), Australian languages such as Dyirbal (Dixon 1972) and Wargamay (Dixon 1981), and various ancient languages, either attested, such as Old Akkadian (Deutscher 2000), or reconstructed, such as Proto-Uralic (Collinder 1960). And doubtlessly there are many more such languages, whose existence may have been obscured by a common analytical bias that leads us to seek out complex structures where none actually exist. Thus, with respect to syntactic recursion, at least, Pirahã is in good company, and is anything but some kind of strange outlandish creature, or, as intimated by some of Dan Everett’s critics, something even worse than that. Indeed, given the large number of languages still associated with such “preindustrial, illiterate societies”, one is even led to wonder whether such languages might constitute the cross-linguistic norm.

And what of Riau Indonesian? As suggested above, its extreme disfavouring of subordination is quite unexceptional; this is true not only within Malay/Indonesian (see Englebretson 2003 for a similar take on another colloquial variety of Indonesian, spoken on the island of Java) but also cross-linguistically. Nevertheless, the availability, however dispreferred, of constructions such as that in (1) shows that Riau Indonesian is not quite like Pirahã or any of the other languages cited above. This rather extreme clash between what people can say and how people do actually speak is due, at least in part, to the much more complex

sociolinguistic circumstances associated with Riau Indonesian, and other similar colloquial varieties of Malay/Indonesian. Such colloquial varieties of Malay/Indonesian occupy the bottom reaches of a lectal cline, at whose upper end are the two standardized versions of the language, Standard Indonesian and Standard Malay. Although structurally quite distinct from one another, colloquial and standard versions of Malay/Indonesian are both present in the minds of diglossic speakers, and as a result, each of the two ends of the cline, colloquial and standard, exerts a substantial effect on its counterpart at the opposite end. On the one hand, it is the presence of multiple embedding in Standard Indonesian that allows a speaker of Riau Indonesian to accept, however reluctantly, constructions such as that in (1), thereby supporting the characterization of Riau Indonesian grammar as syntactically recursive. In this respect, then, Riau Indonesian differs from Pirahã and other similar languages lacking a standardized acrolect that might be more conducive to such recursion. On the other hand, it is the extreme disfavouring of subordination in Riau and other colloquial varieties of Indonesian that percolates upwards along the lectal cline, resulting in a relative disfavouring of subordination also in Standard Indonesian and Malay, as reflected by the differential results of the Google searches reported on in Section 4 earlier. In this regard, Malay/Indonesian presents a clear contrast to English. Standard Malay and Indonesian constitute special registers, not acquired natively by speakers through the usual processes of first language acquisition; they are thus parasitic on their colloquial counterparts (Gil, to appear). In contrast, Standard English is a more natural language variety that is indeed acquired natively by a large population of speakers, and is therefore relatively more resistant to influences from basilectal varieties of English, as, for example, might be manifest in the disfavouring of subordination.

The existence of cross-linguistic variation with respect to syntactic recursion and the proclivity for subordination raises the question what the determining factors might be that underlie such variation. Givón, in the above-cited passage, alludes to “preindustrial, illiterate societies with relatively small, homogenous social units” – a plausible hypothesis, but one still in need of solid empirical cross-linguistic support. As suggested earlier, a major challenge faced by any attempt to seek such support is that whether or not a language has syntactic recursion is very much dependent on how it is analyzed. What is needed, therefore, is a common yardstick by which different languages can be uniformly and objectively assessed with respect to their relative proclivities for syntactic subordination.

6 Cumulative tales

A simple rough and ready measure for the assessment of syntactic recursion across languages is provided by the analysis of an easily-identifiable genre of verbal art, namely cumulative tales (Thompson 1946: 230–234, Aarne & Thompson 1961: 522–536).

A well-known example of a cumulative tale is the Jewish Passover song *Had Gadya*, mostly in Aramaic though with some Hebrew interspersed, shown below transcribed in accordance with a Modern Hebrew pronunciation, followed by an English translation (by Eve Levavi, in *Haggadah for Pesah, an English translation*, hosted on the Open Siddur Project):

- (11) Veata hakadoš barux hu vešaḥat lemalʔax hamavet,
dešaḥat lešoḥet,
dešaḥat letora,
dešata lemaya,
dexava lenura,
desaraf leḥutra,
dehika lekalba,
denašax lešunra,
deaxla legadya,
dezabin aba bitrey zuzey.
‘Then the Holy One, Blessed be He, came and slaughtered the angel of
death,
who slaughtered the butcher,
who slaughtered the ox,
that drank the water,
that put out the fire,
that burned the stick,
that beat the dog,
that bit the cat,
that ate the goat,
that my father bought for two zuzim.’

In (11) above, the last verse is presented, containing a total of 10 clauses, of which the last 9, introduced by the Aramaic relativizer *de-*, are embedded, each within the clause immediately preceding it, like a set of Matryoshka dolls.

The clear and well-defined structural properties of cumulative tales provide an objective criterion enabling their cross-linguistic and cross-cultural distribution

to be gauged. In the absence – as far as I was able to determine – of any existing systematic cross-linguistic study of cumulative tales, a query was posted on the LINGTYP list¹ in which its diverse readership, encompassing typologists familiar with a wide range of the world's languages, were asked if they were familiar with examples of cumulative tales or songs from their respective regions of expertise. The responses that came in demonstrated that cumulative tales are indeed widespread across the world's languages and cultures, but with a crucial qualification. It turns out that the Had Gadya type, involving syntactic recursion, is apparently the exception, and that in most cases, the recursion is of a purely semantic nature, and is not reflected by syntactic subordination – the formal relationship between the successive clauses instead being one of parataxis, or flat chaining. An instance of the latter state of affairs is provided by the following example from an Alemannic dialect of German (cited in Meier 1851: 65 and Newell 1905: 39, and translated into English by Claudia Wegener):

- (12) Gestern haun i fegelt, I haun e Kreuzer gwonne;
De Kreuzer haun u 'r Mutterb gean,
Mutter hat mir Kerne gean,
Kerne haun i 'm Müller gean,
Müller hat mir Mehl gean,
Mehl haun i 'm Becke gean,
Becke hat mir Wede gean,
Wede haun i 'm Vater gean,
Vater hat mir e Stöckle gean,
Stöckle haun i 'm Lehrer hean.
Lehrer hat mir Tatze hean ...
'Yesterday I went bowling, I won a kreutzer;
The kreutzer I gave to my mother,
My mother gave me corn,
The corn I gave to the miller,
The miller gave me flour,
The flour I gave to the baker,
The baker gave me a bun,
The bun I gave to my father,
My father gave me a stick,
The stick I gave to my teacher,
My teacher hit my hand ...'

¹<https://listserv.linguistlist.org/pipermail/lingtyp/2023-February/thread.html>

Although in terms of its semantic structure, example (12) bears a close resemblance to its predecessor in (11), in the Allemanic case there is no syntactic subordination, but instead a sequence of independent clauses, connected to each other by successive relationships of coreferentiality between the subject NP of each clause and the object NP of the clause preceding it. Syntactically, then, rather than Matryoshka dolls, the clauses in (12) are like beads on a string.

While the results of the LINGTYP survey cannot be considered more than preliminary, the emerging picture is one in which cumulative tales involving parataxis, as in example (12), are of widespread distribution, occurring in a variety of unrelated languages and cultural regions, and hence not accountable for in terms of a single ancestral case subsequently spreading by means of vertical inheritance or horizontal diffusion. Some of the languages in which such syntactically flat cumulative tales are attested include Laal, an isolate language of southern Chad (Florian Lionnet p.c.); the Timimoun dialect of Arabic (Mammeri 1985, Lameen Souag p.c.), Basque (Peter Bakker p.c.), the Nakh-Daghestanian language Agul (Timor Maisak p.c.), Tamil (Siva Kalyan p.c.), three at best distantly related languages of New Guinea, Pa, Northeastern Kivai and Asmat (Voorhoeve 2010), and Yucatec Maya (Smailus 1975: 180–186, Jürgen Bohnemeyer p.c.). In contrast, no examples emerged of syntactically recursive cumulative tales, as in (11), from outside the Middle-Eastern/North African/European cultural area – even though, in many such languages, syntactic recursion is permitted by the grammar, and at least some syntactic subordination is used in ordinary speech.

If indeed the limited distribution of syntactically recursive cumulative tales is found to hold up under more extensive empirical scrutiny, then this would potentially point towards one or both of the following two hypotheses governing their distribution. The first hypothesis is diachronic, suggesting that in accordance with this hypothesis, all cases of syntactically recursive cumulative tales can be traced back to a single common origin, with their current distribution the outcome of an interplay of vertical inheritance and horizontal diffusion. While it is beyond doubt that literary genres such as cumulative tales do indeed spread across time and space, what is at issue is whether such diachronic processes are the whole story, or whether other factors might also play a role in the observed distribution of syntactically recursive cumulative tales.

The second hypothesis offers one such factor of a sociolinguistic orientation, appealing to the covariance of societal and grammatical complexity. Specifically, the occurrence of syntactically recursive cumulative tales correlates positively with the complexity of the society with which it is associated, as measured by any of a number of potential yardsticks, in accordance with Chen et al. (2024). In alternative formulations of the hypothesis, syntactically recursive cumulative

tales would be more likely to occur in exoteric as opposed to esoteric societies (Thurston 1987, Wray & Grace 2007); societies characterized as Western, Educated, Industrialized, Rich and Democratic, or WEIRD (Henrich et al. 2010, Henrich 2020) or as Literate, Official, and with Lots of speakers, or LOL (Dahl 2015); societies high on Ethnologue's Expanded Graded Intergenerational Disruption Scale, or EGIDS (Eberhard et al. 2020); and societies of greater complexity as reflected in a variety of features in the D-PLACE database (Kirby et al. 2016), such as, for example, population size, population density, and the number of jurisdictional levels above the local community. While the results of the informal LINGTYP survey are consistent with this hypothesis, more work is necessary before such a hypothesis can be empirically supported.

7 Conclusion

However interesting in its own right, the examination of the distribution of syntactically recursive cumulative tales is of course intended here as a mere proxy for a more general investigation into the cross-linguistic distribution of syntactic recursion. While much harder to ascertain in an objective systematic manner, there is good reason to believe that the distribution of syntactic recursion more generally may be sensitive to the variegated aspects of societal complexity alluded to in the second hypothesis above. That is to say, syntactic recursion is more likely to be found in the grammars of languages spoken in societies associated with greater complexity.

One centrally important aspect of societal complexity mentioned earlier is modality. As already noted, syntactic recursion is more commonly found in writing than in speech; moreover, this tendency may be manifest in two distinct ways, online and conventionalized. To begin with, multiple embedding and associated syntactic recursion are more likely to occur in writing than in speech within a single language or even speaker. But in addition, within the same modality, languages with an overall higher rate of literacy are more likely to make use of multiple embedding and allow syntactic recursion. Evidence for this latter correlation was provided earlier by the contrasting results of the Google searches in English, with a higher rate of literacy, and Malay/Indonesian, which, as suggested in Gil (2009: 30), is characterized by a lower rate of literacy and correspondingly lower functional range of written communication.

Nevertheless, literacy is just one of an array of features generally associated with societal complexity, and there is good reason to believe that several of these other features may also be associated with a greater propensity for recursion. A

series of recent studies have demonstrated positive correlations between various aspects of societal and linguistic complexity. Thus, recent experiments by Raviv et al. (2019, 2020) and Raviv (2020) show that in artificial languages, larger speech communities create more highly compositional languages, entailing increased complexity in the domain of combinatorial syntax. Similarly, in sign languages, Meir et al. (2012) and Ergin et al. (2020) argue that an increase in the size of the signing community entails a greater degree of conventionalization. In phonology, Hay & Bauer (2007), Atkinson (2011), Wichmann et al. (2011) and Nettle (2012) argue that larger languages tend to have larger phonemic inventories than smaller languages. In the domain of metaphor comprehension, Gil & Shen (2021) present evidence to the effect that more highly complex polities tend to be associated with languages whose metaphors are of more complex directional structure. With respect to Tense-Aspect-Mood marking, Gil (2021) demonstrates that languages belonging to larger families, the product of demographic spread, are associated with more complex systems characterized by obligatory as opposed to optional marking. Finally, in the realm of basic clause structure, work in progress, some initial results of which are summarized in Gil & Shen (2019), shows that more highly complex polities tend to be associated with languages endowed with a greater degree of grammaticalization of thematic-role assignment. Admittedly, though, a range of other studies support an opposite negative correlation between societal and linguistic complexity. As argued by McWhorter (2018, 2005, 2011), Dahl (2004), Wray & Grace (2007), Lupyán & Dale (2010), Trudgill (2011) and others, larger political entities, typically associated with various modes of exoteric communication, and in particular imperfect adult second-language acquisition, are conducive to linguistic simplification, whereas smaller societies, generally characterized by more esoteric forms of communication, are fertile grounds for the accretion of linguistic complexity. A way of reconciling these apparently divergent results is proposed in Chen et al. (2024), who demonstrate, in a quantitative study based on data from the World Atlas of Language Structures, or WALS (Haspelmath et al. 2005), that many of the linguistic features associated with a positive correlation between linguistic and societal complexity tend to be of a syntactic rather than morphological nature; in addition, they speculate that such features will tend to rely more on procedural as opposed to declarative memory. Since recursion was not included in the features examined in WALS, it is not mentioned in the Chen et al. (2024) study. However, the generalizations emerging from that study strongly suggest that, as manifestations par excellence of syntactic complexity, syntactic subordination and recursion should also exhibit a positive correlation with societal complexity.

Viewed in the light of the covariance of societal and syntactic complexity, the facts regarding recursion in Malay/Indonesian, Pirahã and English, as summarized in Figure 5, make perfect sense. In terms of societal complexity, Malay/Indonesian occupies an intermediate position between Pirahã and English. Whereas Pirahã is spoken by a single small group of people with relatively little contact with the outside world, Malay/Indonesian, in all of its varieties, is spoken by well over two hundred million people and is an official language of four different countries. No wonder, then, that, unlike Pirahã, Malay/Indonesian allows for syntactic recursion, a fact that is true also for colloquial varieties such as Riau Indonesian. However, in contrast to Malay/Indonesian, English is a world language, a vehicle not only of national but also international communication; moreover, unlike Malay/Indonesian, its standardized versions are spoken natively by large populations of speakers. It is this difference that accounts for the massively greater frequency of subordination, a manifestation of syntactic recursion, in actual English usage, as contrasted with Malay/Indonesian.

Abbreviations

ASSOC	associative
HYP	hypocoristic
PRTC	participant

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Chapter 9

The first-person perspective and second-order desires

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In this paper I argue that persons who entertain non-instrumental second-order desires – desires to desire something – must have the deliberative first-order desires they wish to have. The necessary connection between these types of desires consists of the fact that the subjects that have them are not self-blind, i.e., if they have the second-order desires, they must be consciously self-acquainted with them. I demonstrate that since the difference between the second-order and first-order deliberative desires does not concern their intentional content, it is merely a verbal difference. The upshot of this discussion is that it is not really possible to recursively generate a set of non-instrumental deliberative desires. This significant cognitive constraint is implied by the role of first-person self-acquaintance within rational cognition.

I first present the characteristics of non-instrumental second-order desires. I then explain why entertaining a desire that belongs to this type necessarily implies that the person who has it must have the first-order deliberative desire she wishes to have. I continue by illustrating the cognitive difference between the first-person and third-person ascription of deliberative second-order desires. I conclude by singling out the differences and the connections between deliberative desires and spontaneous desires that have the same content.

1 Introduction

A second-order desire is a desire that the person having it will desire that p be the case or that she will act in a certain way. The formation of second-order desires involves uses of the first-person pronoun or its mental analogue. First-person



uses of “I” serve indispensable roles in human cognition (Castañeda 1966, 1967, 1968, Perry 1979, Lewis 1979, Chisholm 1981).¹ According to Burge (1998), the indispensability of the first-person concept to rational cognition consists among other things in the fact that reasons must sometimes be rationally applicable to immediately affect an attitude or an action.² According to Shoemaker (1996), first-person self-acquaintance determines a fundamental characteristic of rational mental states. As he notes, considerations related to Moore’s paradox (Moore 1903) require one to acknowledge the fact that rational human creatures cannot be self-blind. A self-blind creature can know his mental states only in the third-person mode of knowledge (Shoemaker 1996: 30–31). According to Shoemaker (1996: 31),

To deny the possibility of self-blindness is to hold that it is implicit in the nature of certain mental states that any subject of such states that has the capacity to conceive of itself as having them will be aware of having them when it does, or at least will become aware of this under certain conditions (e.g., if it reflects on the matter).

That self-blindness is not possible implies imperative constraints relevant to the cognitive and epistemic architecture of rational beings.

Shoemaker’s main concern is the impossibility of self-blindness with respect to the concept of belief. My goal in this paper is to point out the fundamental role of first-person self-acquaintance with respect to another kind of mental state – non-instrumental, second-order desires. First-person self-acquaintance with this type of second-order desires entails that the person having them necessarily has the relevant deliberative first-order desires. The third-person attribution of deliberative second-order desires does not involve a similar kind of necessary connection.³

In the sections below, I first briefly state the commonly accepted features of desires and similar pro-attitudes. I then continue by clarifying the nature of deliberative non-instrumental second-order desires. Finally, I explain why realizing

¹Perry pointed out the cognitive value of the first-person use of “I” which functions as an essential indexical. According to Lewis, the intentional content of a mental state consists of the self-attribution of properties to oneself. Chisholm explicates the content of intentional states in a similar way. However, these accounts do not address the ways in which the rational constraints related to first-person self-acquaintance determine the necessary connection between some mental states.

²In what follows, I clarify why the feature pointed out by Burge is linked to the rational constraints related to first-person self-acquaintance.

³As will be clarified in what follows, my argument for the necessary connection between the relevant second-order and first-order desires does not depend on Shoemaker’s argument.

the relevant second-order desire entails that the same person who has the second-order desire must have the desired first-order desire. As I demonstrate below, the upshot of this discussion is that it is not really possible to recursively generate a set of deliberative desires. This seems to be a fact about human cognition. For the difference between the second-order deliberative desire and the first-order deliberative desire does not concern their intentional content; it is merely a verbal difference.

2 The features of desires

Desires are pro-attitudes that have at least the following features:

- (a) The content of a desire determines its conditions of satisfaction.
- (b) Desires are individuated by the subject having them and by their content.
- (c) A person may have conflicting desires, that is, desires that cannot be satisfied together.
- (d) Desires move the persons having them to aspire for their satisfaction.

The content of a desire represents what one desires. The content of a particular desire is only one part of its individuating conditions. Two desires with the same content are not identical *qua* particular mental states if different persons have them. Condition (d) requires some clarification. In contrast to beliefs, desires do not represent how things are but how one wishes them to be. Aiming at being satisfied is a necessary (reflexive) characteristic of desires. It cannot consist in having a separate desire [S]D_x the content of which is that D_x be satisfied. For given that [S]D_x also aims at being satisfied, if aiming that D_x be satisfied consists in a different desire [S]D_x, each desire must be connected to an infinite set of separate desires. Since this supposition is incoherent, aiming at being satisfied must be an intrinsic (reflexive) constituent of desires.

Aiming at being satisfied is equivalent to being moved to act in ways that satisfy the content of the desire. However, an agent may have conflicting desires D_x and D_y, desires that cannot be satisfied together. Therefore, being moved to act in ways that satisfy the content of a given desire D_x does not entail that the agent will eventually do something in order to satisfy D_x. She could choose to satisfy a conflicting desire D_y. However, this is irrelevant to the fact that desires intrinsically aim at their own satisfaction, i.e., that aiming at being satisfied is

a necessary constituent of desires. Desires, all kinds of desires, move the agent having them to pursue the realization of what their content represents.

In what follows I will assume that (a–d) are sufficient conditions for characterizing a mental state as a desire. There is, however, an additional condition that seems to be part of the concept of desire:

- (e) Desires do not necessitate the realization of the states of affairs that satisfy them.

There is a strong presumption to the effect that both first- and second-order desires must satisfy condition (e) above. Yet, as I demonstrate below, at least one type of second-order desires does not satisfy (e).

3 Features of deliberative second-order desires

I begin by briefly explicating the nature of non-instrumental desires, in order to single out the relevant kind of second-order desire. An instrumental desire is a desire the satisfaction of which is required in order to attain something else one desires. The satisfaction of a non-instrumental desire is conceived as being good in itself. The person having it does not desire what she desires for the sake of something else.

Deliberative desires are desires persons have upon a process of deliberation. After such a process, a person may choose to realize a given state of affairs or to carry out a given course of action. Being moved to realize a given state of affairs upon a process of deliberation satisfies the sufficient conditions of desires noted above.

Deliberative desires differ from spontaneous desires. The starving person's desire for food, the caring mother's desire to assist her suffering infant, the aroused person's desire for sexual intercourse, and the desire to distance oneself from precarious situations are all examples of spontaneous desires.

Some spontaneous desires are animalistic or instinctual. However, spontaneous desires could also have been acquired in a process of training or education.

An irrational desire is a desire one *knows* to consist of an impossible state of affairs and which would therefore be unsatisfiable. In what follows I assume that deliberative desires are *rational* desires.

Finally, second-order desires are *conscious* desires. A person having a second-order desire consciously desires that she herself desire that *p* be the case. She reflectively knows that she has the second-order desire. Second-order desires are

deliberative desires. Although some of them are instrumental, some are desired for their own sake. For example, a person addicted to heroin has the *spontaneous* desire to take the drug. She may also have the *deliberative* second-order wish to desire to refrain from taking it. The desire she wishes to realize – to refrain from taking the drug – could be something she conceives to be good in itself and not merely for the sake of something else she desires.

4 Difference of content and separate existence

Second-order desires seem to be states that unanimously satisfy condition (e) above. Consider first the connection between the individuation of a particular desire and its conditions of satisfaction. Let D_p be Jill's desire that her mother will be separated from her violent, abusive father, and let DD_p be Jill's wish to desire that her mother will be separated from her abusive father. D_p and DD_p do not have the same content, for the state of affairs that satisfies DD_p does not entail the state of affairs that satisfies D_p , and the state of affairs that satisfies D_p does not entail the state of affairs that satisfies DD_p . The state of affairs that satisfies D_p is that Jill's mother will be separated from her abusive father, and the state of affairs that consists of Jill's desire that her mother will be separated from her abusive father does not entail that Jill's mother will be separated from her abusive father. Jill can desire that her mother will be separated from her abusive father, even if Jill's mother is not separated from her abusive father. Similarly, the state of affairs that satisfies D_p does not entail the state of affairs that satisfies DD_p . Even if Jill's mother is separated from Jill's abusive father at some future time, this does not necessitate the existence of Jill's desire that her mother will be separated from her abusive father.

According to condition (e), entertaining a desire D_p does not entail that the state of affairs that satisfies it is realized. The fact that the state of affairs that consists of *having* D_p and the state of affairs that *satisfies* D_p are different states of affairs seems to provide a substantial reason for the unrestricted application of condition (e). S having the second-order desire DD_p and S having the first-order desire D_p seem to be two distinct states of affairs. It seems that as far as their *objective content* is concerned, there is no reason to claim that if a deliberative second-order desire DD_p is realized by S, S must also realize D_p . It seems that deliberative second-order desires and the first-order desires that are their objects could exist separately. As Hume (2000) (book I, part III, sect III) already claimed, the distinguishability of the content of two mental states entails their separate existence.

The last claim seems to be supported also by the following consideration. It is evidently possible to rationally desire that *someone else* desire to perform an action X, or that she desire that p be the case. For example, after considerable contemplation, Jill may conclude that her mother should be separated from her abusive father. Say that Jill's mother does not want to be separated from her husband. Upon deliberation, Jill forms the desire that her mother will desire to be separated from her abusive father. Clearly, her desire that her mother will desire to be separated from her abusive father does not entail that her mother desires this. But if one of Jill's mother first-order desires could be the object of Jill's deliberative desire without entailing the existence of Jill's mother's pertinent desire, why can't a similar desire – a desire that has the same objective content – be the object of one of Jill's second-order desires without entailing the existence of her relevant first-order desire? In this case, the difference between the respective content of the first-order and second-order desires also seems to entail their separate existence.

5 Self-knowledge and deliberative second-order desires

Frankfurt (1988) distinguished between two situations that, in his view, may be described by “A wants to want to X”. In one of these situations, the fact that A wants to want to X does not entail that A wants to X. The case of the psychotherapist who wants to be moved by the desire to take the drug to better understand his narcotic addict patients, but without desiring to X, exemplifies the first situation. It should be noted that the case of the psychotherapist is clearly one of an instrumental desire. The desire to take the drug is not desired for its own sake, but to provide a better understanding of narcotic addicts. The other situation that could be described by “A wants to want to X” is a situation in which according to Frankfurt (1988: 15), A wants the desire to X “to provide the motive for what he actually does”. As Frankfurt notes (*ibid.*), in this case “A wants to want to X” entails that “A already has the desire to X”. Frankfurt's unwilling addict – the narcotic addict that desires to desire to refrain from taking the drug – exemplifies the second situation described by “A wants to want to X”. It should be noted, however, that in Frankfurt theory the concept of the will is not identical to the concept of desire. In his view, the will is the desire that leads all the way to action. My concern here is with cases similar to Frankfurt's unwilling addict. I wish to clarify why in this type of case “A wants to want to X” entails that A wants to X.

Let DDx represent a deliberative non-instrumental, second-order desire, and let Dx represent the desired desire. Does the fact that a given subject S has DDx

entail that S has Dx? As I wish to suggest now, there are reasons to claim that if a given person has DDx, she must also have Dx. Consider first the following statements:

- (1) Jill has the deliberative second-order desire to desire to refrain from smoking.⁴
- (2) Jill has no first-order deliberative desire to refrain from smoking.

On first approximation it seems that (1) and (2) are compatible. Yet, since Jill is first-personally conscious that she desires to desire to refrain from smoking, she must know what is involved in her desire to desire to refrain from smoking. She must know the content of her desire, and she must be consciously moved to realize what the content of her desire represents. The following statement must therefore be added:

- (3) Jill knows the content of her deliberative wish that she herself desire to refrain from smoking, and she is consciously moved to realize what the content of her second-order desire represents.

Are (1–3) compatible together? It is indisputable, I suppose, that having the second-order *deliberative* desire to desire to refrain from smoking does not entail that one has the first-order *spontaneous* desire to refrain from smoking. The question is whether entertaining the relevant second-order desire entails having the deliberative first-order desire to refrain from smoking. It can be shown that if the desire is a deliberative first-order desire, (1–3) do not cohere together. In order to see why, consider the two sets of statements below:

- (4) Jill desires to drink water.
- (5) Jill knows that water is not the same liquid as oil.
- (6) Jill does not desire to drink oil.

and

- (7) Jill desires to drink water.
- (8) Jill knows that water = H₂O, i.e., that it contains hydrogen.
- (9) Jill does not wish to drink a portion of liquid containing hydrogen.

⁴I assume that Jill's desire to desire to refrain from smoking is a deliberative non-instrumental second-order desire.

Statements (4–6) cohere together. However, if Jill is a rational person (7–9) is not a coherent set. If Jill knows that the water she wishes to drink is a liquid that necessarily contains hydrogen, her desire to drink water is rationally bound to the desire to drink a liquid containing hydrogen. She cannot rationally desire to drink water and desire not to drink a liquid that contains hydrogen.

Is (1–3) similar to (4–6) or to (7–9)? It can be shown that (1–3) is as incoherent as (7–9). We may begin by noting that the claim that Jill could rationally entertain the non-instrumental second-order wish to desire to refrain from smoking without having the deliberative first-order desire to refrain from smoking in fact means that she could be *moved* to have the first-order non-instrumental desire to refrain from smoking while being *indifferent* as to whether she herself will refrain from smoking. This means that although Jill wishes to be in a state in which she herself is moved to realize the act of refraining from smoking, a state that she conceives to be good in itself, she is nevertheless *not moved* to refrain from smoking. But is it possible for Jill to rationally desire to be in a state in which she is moved to refrain from smoking, qua something desired for its own sake, while being indifferent as to whether she refrains from smoking? The claim that this is possible in fact means that Jill could be self-consciously and rationally moved to be in a state which as she knows aims at being satisfied and is satisfied only if the person that has it refrains from smoking, and nevertheless, her being in this state does not involve her being moved to refrain from smoking. However, this is incoherent. It seems that Jill could deliberately wish to desire to refrain from smoking qua something she conceives to be good in itself only if she conceives *the act of refraining from smoking* to be a good act. However, given that a rational person is moved to realize what she conceives to be good, this characterization is in fact equivalent to depicting Jill as being moved not to refrain from smoking.

We can tackle this issue also from a different viewpoint by raising the following question: What could *explain* Jill's conscious wish to desire not to smoke? Since Jill's second-order desire is not an instrumental desire, and since it is a rational self-conscious desire, the only thing that is able to explain this is her being moved not to smoke. Stated differently, Jill could not self-consciously and rationally be moved to desire not to smoke as something desired for its own sake without being moved to realize what the desire she wishes to have is about. For she must know that it is not possible to desire not to smoke without being moved to realize a state of not smoking. In other words, by being rationally moved to be moved to refrain from smoking she must be self-consciously moved to realize what the desired first-order desire is about. She must be moved to refrain from smoking; she must have the first-order deliberative desire she wishes to have.

Jill could desire something and nevertheless fail to satisfy her desire. She might also desire something and nevertheless act in ways that do not aim to satisfy it, if she prefers to satisfy a conflicting desire, or due to the weakness of her will. But the idea that Jill could *fail to be moved* to refrain from smoking even if she is rationally moved to desire this qua something desired for its own sake is erroneous. Although she could fail to refrain from smoking, by being rationally moved to desire what she desires, Jill must be rationally moved to realize what the content of the desire she wishes to have represents. She must have the first-order deliberative desire that she desires to have.

6 First-person self-acquaintance and deliberative second-order desires

What could be the grounds that motivate the supposition that it is possible for Jill to deliberately desire that she herself desire to refrain from smoking even if she is not moved to refrain from smoking? I suggest that there are at least two such grounds. First, one is motivated to hold this supposition if one disregards the crucial role of first-person self-acquaintance in deliberative second-order desires. Second, this view seems to unjustifiably identify the mere concept of desire with that of *spontaneous* desires. It disregards the fact that deliberative desires are pro-attitudes similar to spontaneous desires, and that deliberative desires could have the same *intentional content* as a given spontaneous desire, although they differ from them qua mental state.

In this section, I address the first ground and in the next section I address the second ground.

It is, I suppose, undeniable that Jill could entertain the deliberative desire that her husband Ben desire to refrain from smoking, even if Ben is not moved to refrain from smoking. There is no reason to suppose that Ben is necessarily moved to refrain from smoking just because his wife desires this. Moreover, Ben could even know that his wife wishes that he will desire to refrain from smoking, and, nevertheless, he will not desire to refrain from smoking. The fact that he is left indifferent to what Jill desires does not indicate that he has any irrational desire that cannot be satisfied. Rather, he should be described as someone who has failed to implement Jill's reasons and to be rationally moved to refrain from smoking. In the same vein, let us presume, for the sake of argument, that it is possible to third-personally attribute a second-order desire to a person by means of some technique of mind reading that is based on some sort of perceptual evidence. If Jill were self-blind, she could have third-personally established in this

way that “this” person wishes to desire to refrain from smoking. In addition, she could have established in a similar way that “this” person is identical to a given person that does not desire to refrain from smoking, and that “Jill” refers to “this” person. Let us presume for the sake of argument that this possibility to attribute a second-order desire to a person on the basis of third-personal, perceptual information without attributing to her the corresponding first-order desire is coherent. It seems that this type of case does not differ from Jill’s deliberative wish that her husband Ben will refrain from smoking. Nevertheless, Jill’s knowledge that a given person that deliberately wishes to desire to refrain from smoking is identical to the person that does not wish to refrain from smoking differs from the self-knowledge that her utterance, “I wish to desire to refrain from smoking, but I do not desire to refrain from smoking,” expresses. If Jill is self-blind, she will fail to know that “I am Jill”, i.e., that she is the person that wishes to desire this, the person that her use of “I” designates. As in the case of her deliberative wish that her husband Ben desire to refrain from smoking, in this case, too, she may fail to be immediately moved to refrain from smoking, even if she grasps the reasons that motivate her deliberative second-order desire. However, this is not the case if Jill knows that she herself is the person that wishes to desire not to smoke, that is, if she says or thinks “I wish to desire not to smoke”. As Perry (1979) explained, reference to oneself by means of the indexical expression “I” cognitively differs from reference to oneself by means of other types of singular expressions. But if Jill is first-personally conscious that she herself non-instrumentally wishes to desire to refrain from smoking, her wish is irrational if she is moved to desire this while deliberately remaining indifferent as to whether she will refrain from smoking. Consciously and rationally desiring that a given state of affairs be realized is identical to being moved to realize it. Since desires necessarily move the person that have them to satisfy them, it is irrational for Jill to self-consciously desire to entertain a cognitive state that is satisfied only if she refrains from smoking – a state desired for its own sake – while being indifferent to her refraining to smoke.

According to Burge (1998: 253), “The first-person concept fixes the locus of responsibility and marks the immediate rational relevance of a rational evaluation to rational implementation on the attitude being evaluated – to epistemic or practical agency”. Deliberative second-order desires exemplify the fact that reasons must sometimes be applicable to affect an attitude or an action. Also in the case of deliberative second-order desires, the first-person concept and its role in rational agency is involved in determining the cognitive structure of rational agents.

7 Deliberative and spontaneous desires

Spontaneous desires have animalistic or instinctual character. One does not choose to have them. In contrast, deliberative desires are based on reason and reflection; they are generated by means of reflective deliberation, i.e., by examining how a given course of action is related to what one conceives to be good. So far, I have claimed that a person who has the deliberative wish to desire something must have the corresponding *deliberative* first-order desire that she wishes to have. However, this does not rule out the possibility of deliberately wishing to spontaneously desire that *p* be the case or to perform an action *X* without spontaneously desiring this. A person may have a deliberative second-order desire without having the relevant first-order spontaneous desire. She could fail to *spontaneously* instantiate the first-order desire she deliberately wishes to have. If there are some courses of action that one can carry out only if one spontaneously desires to carry them out, it seems that a person could have a deliberative second-order desire without having the (spontaneous) first-order desire that satisfies it.

It should be noted, however, that this possibility could be regarded as a counterexample to my main claim here only if the person that has the deliberative second-order desire does not entertain the corresponding deliberative first-order desire. Yet, as I clarify in what follows, the paradigmatic examples of persons that deliberately wish to spontaneously desire to act in a certain way and nevertheless do not spontaneously desire this are not cases in which the person at stake does not have the pertinent *deliberative* first-order desires.

Consider the following example: Jane and Ralph have been married for fifteen years. Jane remembers having been physically attracted to her husband. However, although Ralph continues to be physically attracted to Jane, Jane now does not spontaneously desire to have any sort of intimate physical relations with her husband. She loves him deeply and has great respect for him. She also finds him interesting and amusing and likes the life they share, but does not spontaneously desire to have intimate physical relations with him. When she now sees him expectantly waiting for her, she remorsefully thinks to herself: "How I wish I desired to have sex with Ralph!" Let us suppose that Jane's wish to desire to have intimate relations with her beloved husband is a non-instrumental desire. She desires this because having intimate physical relations with Ralph is part of what Jane conceives to be good in itself. But it seems that in her current state Jane does not desire to have intimate physical relations with her husband, even though she deliberately desires to spontaneously desire this. Hence, it seems that she could first-personally self-ascribe the rational wish to desire to have

sexual intercourse with her husband even though she does not desire to have intimate physical relations with him.

In order to examine what is involved in this objection, it should first be noted that although deliberative desires differ from spontaneous desires, they can have the same content. A person may spontaneously desire not to smoke and deliberately desire not to smoke. Spontaneously desiring not to smoke differs, qua mental state, from deliberately desiring not to smoke. Each of these mental states has a different phenomenological character – they do not “feel” the same. Nevertheless, they could be together the states of one and the same person. The fact that the deliberative and the spontaneous desires are distinct mental states explains why a person can fail to spontaneously instantiate the desire she deliberately wishes to have. But must she have the corresponding deliberative first-order desire, *even if* she fails to spontaneously desire what she wishes to desire?

As noted earlier, wishing to desire that a given state of affairs will be realized while remaining utterly indifferent as to whether what its content represents is realized is as irrational as consciously desiring to drink water, knowing that water necessarily contains hydrogen, without desiring to drink a liquid containing hydrogen. It seems to follow that if one is rationally motivated to desire a given desire, one is necessarily moved to satisfy its content. In addition, if the second-order desire is a non-instrumental one, the agent considers the realization of what its content represents to be something that is good in itself. An agent could wish to spontaneously desire to realize a state of affairs represented by a given content, and she could fail to be spontaneously moved to realize it. But if her wish to spontaneously desire something is a non-instrumental desire, she must also conceive the object of the desire she wishes to have as something that is good in itself. The goodness of what she desires does not depend on how she is moved to realize its content. The fact that she fails to be spontaneously moved to realize the content of the desire she wishes to have therefore does not entail that she is not *rationally motivated* to realize its content. On the contrary, if her deliberative second-order desire is non-instrumental, she must be rationally motivated to realize the content of the first-order spontaneous desire she wishes to have, even if she is not spontaneously motivated to realize it.

For example, Jane fails to realize the spontaneous first-order desire to have sexual intercourse with her husband. But does she also fail to have the corresponding first-order *deliberative* desire, if she has the rational non-instrumental desire to desire this? Say that a psychiatrist offered Jane a harmless drug that would enable her to be physically, spontaneously, attracted to her husband whenever the occasion arose. Would she be motivated to take the drug? I suppose that if

she deliberately wishes to desire to have intimate physical relations with Ralph as part of her motivation to realize and enhance her love for Ralph, she would be motivated to take the drug, even if the conflicting feeling of indifference persisted in her. Moreover, even if she is overpowered by her spontaneous feeling of indifference and ultimately does not take the drug, she is rationally moved to take it. But she is moved to take the drug because she is moved to realize a process of maintaining voluntary and pleasing intimate physical relations with Ralph. In other words, Jane's mental state exemplifies the constitutive properties of a deliberative first-order wish to have intimate physical relations with Ralph. If Jane's deliberative wish to desire to have intimate physical relations with Ralph moves her to take the drug, she must also be ascribed the deliberative desire to have intimate physical relations with him. Although there are, on the one hand, manifest similarities between Jane's past spontaneous attraction to her husband Ralph and her deliberative preference to have voluntary and pleasing intimate physical relations with him, there are also manifest differences between the two states. Jane's former physical attraction to Ralph was a spontaneous state that she did not choose to have. She simply had it. In contrast, her current interest in having pleasing intimate physical relations with Ralph consists in the reasons that would move her to take the drug. Nevertheless, if she has the non-instrumental deliberative second-order desire to spontaneously desire to have sexual intercourse with Ralph, she must have a deliberative first-order desire with the same content.

In view of the above, the case of the indifferent wife and similar cases are therefore not cases in which the person who deliberately wishes to spontaneously desire something does not have a relevant deliberative first-order desire.

8 Conclusion

We are naturally constituted to spontaneously desire certain kinds of goals. We do not choose to have the spontaneous desires that we do in fact have, and we choose not to have some of the spontaneous desires that we have. But we also possess the capacity for rational deliberation that could move us to pursue the realization of certain goals. Jill has not chosen to experience her current spontaneous desire to smoke. But the claim that her deliberative desire to refrain from smoking is not based on her rational capacity to choose is mistaken. Although her spontaneous desires to smoke or to refrain from smoking cannot be depicted as states she *deliberately* realizes, she could certainly be depicted as a person who deliberately prefers to refrain from smoking. However, it is incoherent to

suppose that a person who self-consciously prefers to desire not to smoke qua something desired for its own sake could fail to realize the deliberative wish to refrain from smoking. No similar incoherence characterizes the case in which a person third-personally ascribes to an agent a desire that someone will desire something.

The difference between the first- and third-person ascriptions of the relevant second-order desires therefore indicates the limits of the third-personal relation to oneself. It reveals one sense in which first-person self-acquaintance has an indispensable role in rational cognition and rational conduct.

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Chapter 10

The causative alternation in Italian: A case study in the parallel architecture of grammar

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In this article I discuss corpus evidence from Italian which suggests that the causative alternation cannot be reduced to a single principle, be it semantic, syntactic or at the interface between these two levels of analysis. I argue that the boundaries of the causative alternation are established in grammar through (i) the acquisition of inchoative and causative logical structures, which are stored in the lexicon alongside non-templatic facets of meaning, (ii) general semantics-syntax mapping principles, which are subject to alignment variation, and (iii) constructional instructions, which determine which subclasses of verbs can enter the constructions that are relevant to the causative alternation in each individual language. Two such constructions are identified in my study of Italian, the one being marked by the morpheme *SE*, the other being *labile*, though I suggest that the *-SE* intransitives of alternating verbs are not necessarily anticausative. My study reveals the causative alternation to be a prime illustration of the parallel architecture of grammar, where the default principles that govern the interplay of a rich lexical module with syntax interact with language specific constructional requirements.

1 The problem

The study of grammatical patterns which are restricted to specific semantic classes of verbs provides strong evidence for the status of the lexicon as an independent, and analysable, module of grammar (Pinker 1989, 2013, Levin 1993).



The causative alternation is no exception.¹ Since Jespersen (1927: 332–337) the lexical-semantic properties that characterize the alternating verbs, or subclasses thereof, have received a great deal of attention (Haspelmath 1993, Levin & Rapaport Hovav 1995, Kiparsky 1997, Chierchia 1989, Comrie 2006, Schäfer 2009, Samardžić & Merlo 2012, etc.). The study of the Romance languages has made an important contribution to the understanding of the lexical underpinnings of the causative alternation. The debate has centred around the distribution of the morpheme *SE*, which marks the intransitive member of the causative-intransitive pairs, dividing the alternating verbs into three formal groups (Zribi-Hertz 1987, Labelle 1992, Alexiadou et al. 2006, 2015, Legendre & Smolensky 2010, Cennamo & Jezek 2011, Cennamo 2012, Kailuweit 2012, Martin & Schäfer 2014, Vivanco 2021, etc.). One of these groups exhibits *SE* obligatorily; a second one has both marked and unmarked intransitive realizations; lastly, a third subclass alternates in a labile way. I provide Italian examples of verbs of the three groups in (1a–1c).

(1) Italian

- | | |
|--|--|
| a. + <i>SE</i> – <i>sparpagliare</i> ‘scatter’ (tr.) | <i>sparpagliarsi</i> ‘scatter’ (intr.) |
| b. ± <i>SE</i> – <i>riscaldare</i> ‘heat’ (tr.) | <i>riscaldare-riscaldarsi</i> ‘heat’ (intr.) |
| c. – <i>SE</i> – <i>aumentare</i> ‘increase’ (tr.) | <i>aumentare</i> ‘increase’ (intr.) |

In a series of important contributions, Cennamo (1995, 1999, 2012, 2015, 2021, 2022) has argued that the *SE* morpheme, originally signalling an unexpressed causer, developed into a marker of lexical aspect. For Cennamo (2012), in Modern Italian the +*SE* class features accomplishment and achievement verbs, which lexicalize a specific result state (see the notion of quantized change of Hay et al. 1999, Beavers 2011); the –*SE* class features degree achievements (Dowty 1979: 88–90, see also Bertinetto & Squartini 1995, Hay et al. 1999, Beavers 2011), which entail that a goal state exists, but fail to lexicalize a specific result state (see the notion of non-quantized change); finally, the ±*SE* verbs describe change with an optional telos, with *SE* figuring when a result state is attained (for the last point see also Folli 2002, Jezek 2003: 161–163). A different lexical-semantic rationale was invoked by Alexiadou et al. (2015: 96, 114–120), who claimed the constituency of the three subclasses, in Romance and other languages, to depend on general principles governing the syntactic realization of roots with low spontaneity meanings (for spontaneity see Haspelmath 1993, 2016).

Romance *SE* figures not only in anticausatives, but also with non-alternating verbs of movement (Spanish *irse* ‘go.*SE*’) and in transitive constructions with

¹See Appendix A for a glossary of key terms with informal definitions which are compatible with the analyses proposed in the article.

verbs of ingestion and some ‘get’ verbs (Levin 1993: 48) (Catalan *La Llúcia s’ha menjat un albercoc* ‘Lucy ate an apricot’, Acedo-Matellán et al. 2022: 505). Furthermore, SE marks reflexives, impersonals and passives, where the lexical-semantic constraints that emerge from analysis of anticausatives do not apply. Consider by way of example the impersonal SE construction of Italian, which features verbs of all classes (e.g., the Vendlerian activity *dormire* ‘sleep’, the achievement *svenire* ‘faint’ and the state *star bene* ‘be well’: *si dorme* ‘one sleeps’, *si sviene* ‘one faints’, *si sta bene* ‘one is well’, etc.). In fact, the said constructions are marked with the same morphology in many languages (Lyons 1969: 371–374, Kemmer 1993, Beavers & Udayana 2023)

These facts suggest that to capture the pervasiveness of SE in Romance grammar it is necessary to look beyond the lexicon, and indeed beyond the causative alternation, and draw generalizations across a wide range of constructions, while at the same time factoring lexical meaning into the distribution of anticausative SE. In this article I aim to provide such an account, focusing exclusively on Italian, and building on the unified analysis of SE as a marker of argument suppression which I proposed in Bentley (2006) (see also Van Valin 1990, Centineo 1995). The analysis of 8,000 lines of authentic data from the itTenTen20 corpus (Jakubiček et al. 2013) reveals that most of the verbs in my sample are subject to the \pm SE variation, contrary to expectation, although a different rationale underlies the absence of SE in the intransitive of different verb classes. The examination of this rationale reveals the existence of two constructions with causer suppression, which I call *overt* (+SE) and *labile* (–SE) *anticausativization*. My main claim is that the causative alternation cannot be reduced to a single principle, whether semantic, syntactic or at the interface between these two levels of analysis. In fact, the causative alternation testifies to the modularity of grammar, which is best analysed in terms of parallel dimensions, which interact in accordance with principles that are part of the grammatical architecture (Bresnan & Kanerva 1989, Van Valin & LaPolla 1997, Jackendoff 2002, Van Valin 2005, 2023). I adopt the framework of Van Valin & LaPolla (1997) and Van Valin (2005, 2023), where the general principles that govern the interplay of lexical and compositional meaning with syntax are complemented by language-specific constructional instructions, stored as templatic Constructional Schemas (CSs). The CSs constrain the range of meanings admitted by each of the anticausativization constructions.

The article is organized as follows. After a brief overview of the existing approaches to anticausative SE and their predictions (Section 2), I introduce my findings (Sections 3 and 4). I then provide my parallel architecture account of the causative alternation in Italian (Section 5) and draw some conclusions on the theoretical significance of my results and analyses (Section 6).

2 Existing analyses and predictions

The existing accounts of the causative alternation in Romance can be divided into those that claim the distribution of SE to be underpinned by aspectual factors and those that analyse SE as the manifestation of an argument realization strategy. Within the aspectual accounts one can subsume, on the one hand, the analyses which associate SE with lexical facets of meaning (Cennamo 2012, 2015, 2021, Vivanco 2021), and, on the other hand, the analyses of SE as the marker of a resultative construction, i.e., a construction which entails or implicates the attainment of a result state. The latter hypothesis, originally put forward for French (Zribi-Hertz 1987), was then extended to other Romance languages in work of various theoretical persuasions (Jezek 2003, Folli 2002, Folli & Harley 2005, Manente 2008, Labelle & Doron 2010, Cennamo & Jezek 2011, Cennamo 2012). The observation that the SE-marked intransitives can be incompatible with expressions of non-completion (cf. 2) has been key for the development of these analyses.

(2) Italian (Cennamo 2012: 401)

Il querceto si bruciò / si è bruciato (*per giorni).

the oak_wood SE burn.PST.3SG SE be.3SG burn.PTCP for days

‘The oak-wood burned down (*for days).’

A clear strength of the aspectual approach is that it can capture SE marking with verbs of change which do not participate in the causative alternation (Section 1) (De Miguel & Fernández-Lagunilla 2000, Acedo-Matellán et al. 2022). On the other hand, the SE marking of reflexives, passives and impersonals lacking the relevant aspectual properties remains unexplained in this approach, as indeed does the absence of SE with many verbs of quantized change (though see Vivanco 2021 for a discussion of the latter issue with relevance to Spanish).

As for the accounts of SE as an argument realization strategy, for some scholars, SE is the marker of a causer, which is not expressed as an overt argument of the verb because it is suppressed or reflexivized (Rothenberg 1974, Van Valin 1990, Labelle 1992, Cennamo 1995, Centineo 1995, Bentley 2006, Koontz-Garboden 2009). For others, SE is the filler of the syntactic projection of the external argument, Voice (Alexiadou et al. 2006, 2015). A key strength of the argument realization approach is that it can in principle be extended to the passive, impersonal and reflexive domains. Thus, in analyses which I shall build upon in this work, it has been claimed that the common denominator of these constructions is the suppression of the highest-ranking argument in semantic representation, which results in a deviation from the pattern of subject assignment that is canonical in accusative alignment (Van Valin 1990, Centineo 1995, Bentley 2006, 2023,

González-Vergara 2006). In addition, since the argument realization account is not in principle limited to alternating verbs with specific lexical-aspectual properties, it does not predict that all verbs of quantized change should exhibit anticausative SE (in fact, not all such verbs do; see, e.g., Italian *apparire* ‘appear’, *svenire* ‘faint’, etc., which lexicalize quantized change and only take impersonal SE). However, the analysis of SE as an argument realization strategy does not explain the intuitions in (2). Furthermore, unless combined with lexical or constructional restrictions (see, by way of example, the spontaneity principle invoked by Alexiadou et al. 2015: 96, 114–120), the argument realization analyses predict that all alternating verbs should bear SE in the anticausative, contrary to fact (cf. 1c).

The insights that underlie the two principal approaches are foundational in the analysis that I propose for Italian. However, in the discussion to follow it will become clear that the distribution of SE is not captured by a direct correlation between each intransitive realization (+/–SE) and a single syntactic or lexical-semantic configuration. While the facts escape the reductionist approaches, they can be accommodated in a modular analysis, which relies on general principles for the construction of word meaning and for the mapping of separate levels of semantic and syntactic representation, and which factors in the role of constructions.

3 The survey

3.1 Methodological preliminaries

Availing myself of the itTenTen20 corpus of Sketch Engine (Jakubíček et al. 2013), I examined 500 randomized occurrences of 16 change of state verbs, amounting to 8,000 excerpts of text.² In selecting the verbs to be included in the sample, I drew on the secondary literature (Folli & Harley 2005, Cennamo 2012, 2021, Cennamo & Jezek 2011, Alexiadou et al. 2015, Bentley 2023) with the purpose of including representatives of each of the three formal groups in (1a–c). In Table 1 I list the chosen verbs, alongside their English translations and the expected distribution of SE.³

²These data were gathered by the Author in the period between 19 August and 27 November 2022. The exact same dataset can be retrieved from Sketch Engine by conducting the following searches: `method=Concordance; corpus=itTenTen20; concordance size=500; query=lemma “infinitive form of verb x”; random sample=500`. Additional examples from the itTenTen20 and itTenTen16 corpora (Jakubíček et al. 2013) will occasionally be cited, providing the date of retrieval.

³I use brackets in the case of *sparpagliare* ‘scatter’ to indicate that the expectation of +SE behaviour was based in this case on my native-speaker intuitions.

Table 1: Verbs included in the sample: Expected intransitive marking.

Verb (It)	Verb (En)	Expected intransitive marking
sparpagliare	scatter	(+SE)
sbriciolare	crumble	+SE
aprire	open	+SE
chiudere	close	+SE
rompere	break	+SE
cuocere	cook	±SE
bruciare	burn	±SE
congelare	freeze	±SE
sgonfiare	deflate	±SE
riscaldare	heat	±SE
asciugare	dry	±SE
arrugginire	rust	±SE
migliorare	improve	-SE
aumentare	increase	-SE
marcire	rot	-SE
sbocciare	blossom	-SE

The verbs were searched as lemmas with Verb as Part of Speech (see note 2). The hits were classified in accordance with the following grammatical domains: transitive, passive, non-passive intransitive (+SE vs. -SE), infinitive embedded in periphrastic causative, participial adjective, other. The focus of this article will be on the transitive and non-passive intransitive domains.⁴

Both the +SE and the -SE intransitive tokens included examples which were irrelevant for the analysis of the causative alternation. To begin with, on a par with other Romance languages, Italian has both an impersonal and a passive structure formed with SE and an active form of the verb (Sections 1 and 2). While the unexpressed argument is [+human] in both cases, in the impersonal structure there is no overt argument (e.g., *si dorme* ‘one sleeps’). Instead, the SE passive exhibits a controller of verb agreement, thus being formally indistinguishable from SE anticausatives. Since the inference of an agent arises from the passive, I relied on the presence of an adverbial indicating manner, purpose, or agency in the same clause to diagnose passive SE, distinguishing it from anticausative SE.

⁴See Appendix B for the percentages of occurrence recorded for each verb in each of the grammatical domains under consideration

Alternatively, hits were classified as passive when another unambiguous passive occurred in the previous, same or following clause. Relevant examples are given below: (3a) exemplifies passive SE and (3b) anticausative SE.

(3) (itTenTen20, 02/09/2022)

- a. La lanterna LED ... in previsione di un periodo di inutilizzo si
 ... SE
può sgonfiare per essere riposta ...
 can.3SG deflate.INF for be.INF put.away.PTCP
 ‘When it will not be used for a while, the LED lantern can be deflated to be put away.’
- b. ... permetterà al vostro corpo di sgonfiarsi ed
 allow.FUT.3SG to.the POSS body of deflate.INF.SE ...
 eliminare i liquidi in eccesso.
 ...
 ‘It will allow your body to deflate and get rid of any excess liquids.’

That SE is passive in (3a) is suggested by the presence of the purpose clause *per essere riposta* ‘to be put away’, which is in turn unambiguously passive because of the passive auxiliary ‘be’. The relevant clues are not present in (3b), which I classified as anticausative. In categorizing the examples, I included the SE passives (cf. 3a) in the passive counts. Therefore, the SE passives are not included in the proportions of +SE hits introduced in the next section.

The morpheme SE also figures in intransitive reflexives, which differ from anticausatives in that the unexpressed argument signalled by the reflexive clitic SE is coreferential with the overt argument of the verb (Van Valin 1990, Bentley 2006: 257, 126–136). Due to this property, intransitive SE reflexives can be paraphrased with transitive reflexives where atonic *si* is replaced by its stressed counterpart *sé* (*stesso/a*) ‘it/him/herself’. Authentic examples of intransitive reflexives are given in (4a) and their non-clitic paraphrases are added in (4b).

(4) (itTenTen20, 09/06/2023)

- a. Lui entra in bagno, si pettina, si spettina ...
 he enter.3SG in bathroom SE comb.3SG SE ruffle.3SG
- b. Lui entra in bagno, pettina sé stesso, spettina sé stesso ...
 he enter.3SG in bathroom comb.3SG SE self ruffle.3SG SE self
 ‘He enters the bathroom, combs his hair, ruffles his hair ...’

Although reflexives typically have [+animate] arguments, and anticausatives [-animate] ones, Italian does admit SE anticausatives with a human argument, which in turn means that ambiguity between anticausatives and reflexives is possible. In my corpus meaningful transitive paraphrases of the SE intransitives were only available with very few tokens of four verbs (*asciugare* ‘dry’, *bruciare* ‘burn’, *migliorare* ‘improve’, *riscaldare* ‘heat’). A relevant example is given here, where *riscaldarsi* can be paraphrased with *riscaldare se stessi*.

(5) (itTenTen20, 19/08/2022)

... quanto costa riscaldarsi con un caminetto a legna.
how.much cost.3SG heat.SE with a stove at wood
‘... how much it costs to keep warm using a wood-burning stove.’

Since in most cases an anticausative reading – i.e., a reading with no coreferentiality – could not be ruled out, I made the methodological decision not to leave out the putative intransitive reflexives from the +SE intransitive counts. I did, however, return to the distinction between the two structures in the qualitative analysis (Section 3.2). As for the –SE intransitives, they included tokens where the unexpressed argument is not the causer, but rather the argument undergoing change. These structures are irrelevant for the analysis of the anticausative, which, by definition, features the causee, i.e., the argument undergoing change, as its overt argument. The irrelevant –SE tokens, which occurred frequently in culinary recipes, are exemplified in (6).

(6) (itTenTen20, 23/08/2022)

Distribuite i porri ... Cuocete nel forno già caldo a 200°.
lay.out.IMP the leeks cook.IMP in.the oven already hot at 200°
‘Lay out the leeks. Cook in a preheated oven at 200°C.’

I thus distinguished two sets of –SE intransitives, the true anticausatives, on the one hand, and the examples in which the unexpressed argument is the causee, on the other. I should mention that in this latter group I included the majority of the –SE intransitive occurrences of *rompere*, which mean ‘finish/break up with’ and leave unexpressed a figurative undergoer (a relationship, a habit). When –SE *chiudere* had the same meaning, it was treated in the same way.

3.2 The distribution of anticausative SE in the sample

Figure 1 shows the proportion of ±SE tokens in the non-passive intransitive subset of each verb in the sample. The relevant evidence is given in blue (+SE non-passive intransitives) and orange (relevant –SE intransitives), whereas I represent

in grey the –SE intransitives with a covert causee (cf. 6), which will be disregarded in the analysis.

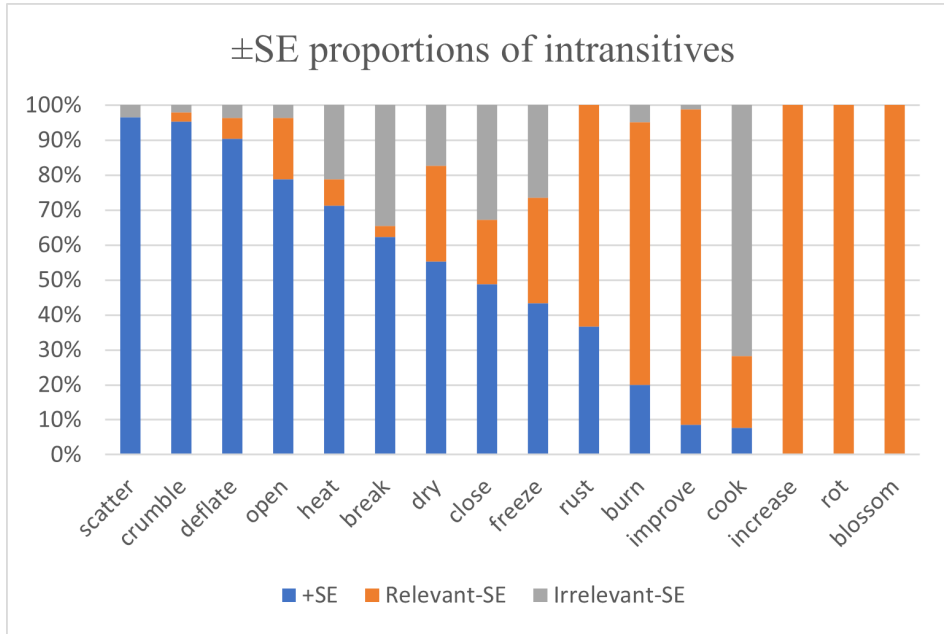


Figure 1: Proportions of +SE and –SE tokens in the non-passive intransitive subset of each verb

The key result emerging from observation of Figure 1 is that most of the verbs in the sample yielded both +SE and relevant –SE intransitives. Although the ±SE variation was expected with *cuocere* ‘cook’, *bruciare* ‘burn’, *congelare* ‘freeze’, *sgonfiare* ‘deflate’, *riscaldare* ‘heat’, *asciugare* ‘dry’, *arrugginire* ‘rust’ (see Table 1), it was not expected with *sbriciolare* ‘crumble’, *aprire* ‘open’, *chiudere* ‘close’ and *rompere* ‘break’, which are normally classified as +SE verbs, or with *migliorare* ‘improve’, which is classified as –SE. In Table 2 (page 235) I contrast the expected distribution of SE with the one recorded in my dataset, highlighting in grey the unexpected results. For completeness, I add the percentages of +SE and relevant –SE tokens in the non-passive intransitive subset of each verb and the intransitive counts per verb.

To be sure, the incidence of SE varied considerably, yielding proportions below 10% with *cuocere* ‘cook’ and *migliorare* ‘improve’ and above 90% with *sbriciolare* ‘crumble’, *sgonfiare* ‘deflate’, and the sole verb patterning exclusively as +SE, namely *sparpagliare* ‘scatter’ (the very few –SE tokens of this verb were of the

irrelevant type, Section 3.1). The low SE proportions with *cuocere* ‘cook’ depend on the frequent occurrence of this verb in culinary recipes: 71.8% of the –SE non-passive intransitives are irrelevant (cf. 6), while only 20.5% are anticausatives (see Figure 1 and Section 3.1).

As for *migliorare* ‘improve’, despite first appearances it turned out to be a –SE verb, in accordance with my expectations (see Table 1). Indeed, only one of its +SE tokens arguably lent itself to an anticausative analysis (cf. 7a), the others being reflexive, as suggested by the replaceability of atonic SE in (7b) with the tonic reflexive *sé stesso* (...*per migliorare me stesso* ‘to improve myself/my performance’) (Section 3.1).

(7) (itTenTen20, 27/11/2022)

- a. Questo studio ha realizzato, col passare degli anni, molti servizi
...
matrimoniali, specializzandosi e migliorandosi sempre
... specialize.GERUND.SE and improve.GERUND.SE ever
di più.
of more
‘Over the years, this photographic studio has specialized and improved more and more in producing wedding albums.’
- b. ... continuerò ad impegnarmi al massimo per
continue.FUT.1SG to do.best.INF.SE at.the maximum for
migliorarmi
improve.SE
‘I will continue to do my very best to improve (myself/my performance).’

Observe also that *migliorare* ‘improve’ occurs in a purposive infinitival clause in (7b) and its missing argument is controlled by a human subject. This indicates intentionality, a facet of meaning which does not pertain to anticausatives but does characterize proper reflexives. Similar considerations were made about the remainder of the +SE examples of this verb. As for the verbs with high SE proportions, the prefix *s-*, associated with verbs that describe the attainment of a result state (Iacobini 2004: 112, 146, 159), might at first sight be thought to play a role SE marking. However, this hypothesis is challenged by –SE *sbocciare* ‘blossom’, which exhibits the same prefix. Therefore, it must be concluded that this morphological feature cannot alone be responsible for SE marking. The other unexpected findings, highlighted in grey in Table 2, will be discussed in the following sections.

Table 2: Intransitive counts of the verbs in the sample and their expected and actual intransitive marking.

Verb (It)	Verb (En)	Intransitive		% in intr. subset		Count
		Expected	Actual	SE	-SE	
spargliare	scatter	(+SE)	+SE	96.6	0.0	87
sbriciolare	crumble	+SE	±SE	95.4	2.6	153
aprire	open	+SE	±SE	78.9	17.6	143
chiudere	close	+SE	±SE	48.7	18.6	156
rompere	break	+SE	±SE	62.3	3.3	151
cuocere	cook	±SE	±SE	7.7	20.5	117
bruciare	burn	±SE	±SE	20.0	75.2	145
congelare	freeze	±SE	±SE	43.4	30.2	53
sgonfiare	deflate	±SE	±SE	90.4	6.0	301
riscaldare	heat	±SE	±SE	71.3	7.5	80
asciugare	dry	±SE	±SE	55.3	27.4	179
arrugginire	rust	±SE	±SE	36.6	63.4	101
migliorare	improve	-SE	±SE	8.5	90.4	94
aumentare	increase	-SE	-SE	0	100	179
marcire	rot	-SE	-SE	0	100	314
sbocciare	blossom	-SE	-SE	0	100	350

3.3 Further findings

The following results are of note. First, a subset of the -SE verbs participate in the causative alternation, which is not explained by the argument realization analyses (Section 2). Second, the -SE verbs can lexicalize quantized change, while the ±SE ones may fail to exhibit SE in resultative contexts, which is problematic for the aspectual analyses (Section 2). Lastly, the rationale of the ±SE variation is not the same with all verbs and reveals a contrast between two subclasses of verbs. This contrast will turn out to correlate with the selection of different perfect auxiliaries in the -SE intransitives, a fact which has not yet received due attention (though see Bentley 2021). I shall discuss these issues in turn.

3.3.1 Alternating –SE verbs

The existence of alternating verbs which never exhibit SE in the intransitive is not a novel result (Section 1). The purpose of this section is to identify which verbs from my sample belong in this group and to discuss the properties which set them apart from the alternating verbs that admit SE marking. In Figure 2 I show the proportions of transitive hits obtained for each verb.

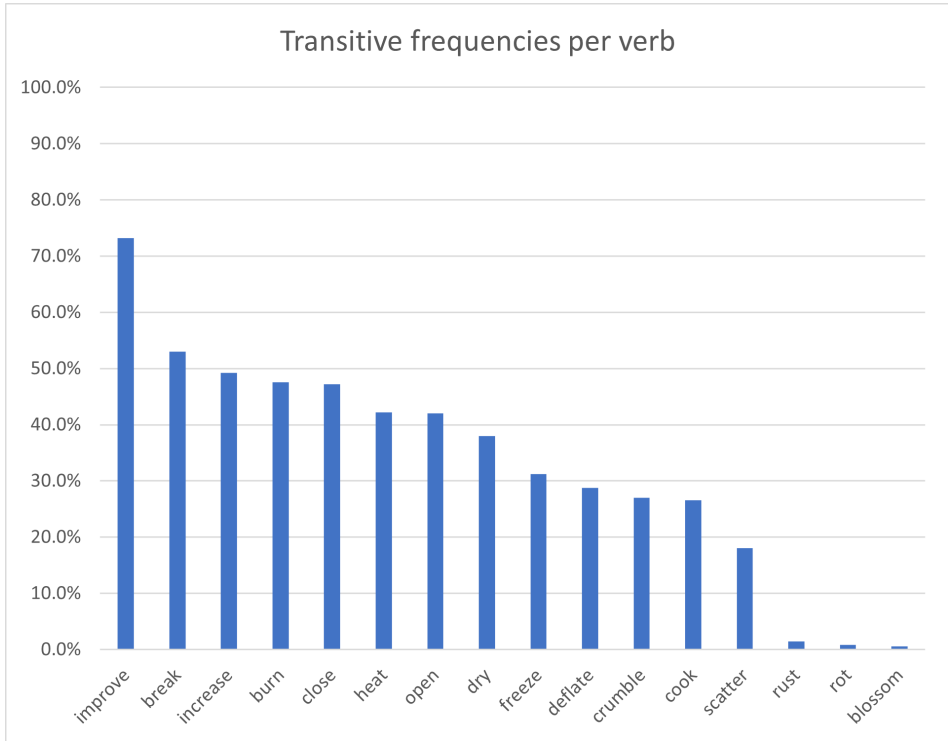


Figure 2: Proportions of transitive attestations

Observation of Table 2 and Figure 2 suggests that *aumentare* ‘increase’ is an alternating –SE verb. I provide an intransitive and, respectively, a transitive example of this verb here.

- (8) (itTenTen20, 29/10/2022)
...i casi di tumore del pancreas sono *aumentati* del 59% ...
the cases of tumour of.the pancreas be.3PL increase.PTCP of.the 59%
‘The incidence of pancreatic cancer has increased by 59% ...’

- (9) [L]a crisi economica avrebbe *umentato*
 the crisis economic have.COND.3SG increase.PTCP
 l'incidenza delle discriminazioni per età...

...

'The economic crisis would increase the incidence of age discrimination.'

The intransitive example illustrates the absence of SE marking, which is exceptionless with this verb in my dataset. As for the transitive example, the fact that the higher argument (*la crisi economica* 'economic crisis') is inanimate supports a causative – as opposed to agentive – analysis. This is further corroborated by the possibility of paraphrasing this structure with a causative periphrasis (Zribi-Hertz 1987: 26–27 and references therein).

- (10) La crisi economica avrebbe *fatto* *umentare*
 the crisis economic have.COND.3SG make.PTCP increase.INF
 l'incidenza delle discriminazioni per età...

...

'The economic crisis would increase the incidence of age discrimination.'

Non-causative agentive verbs are not synonymous with their causative periphrastic counterparts, as shown by the different meanings of the authentic example in (11a) and its constructed counterpart in (11b): an agent, and not the *fare* 'make' causer, is understood to be the higher argument of the embedded infinitive in (11b).

- (11) (itTenTen20, 09/06/2023)
- a. In due giorni ho *ricamato* un cuscinetto ...
 in two days have.1SG embroider.PTCP a cushion.DIM
 'I embroidered a small cushion in two days.'
- b. In due giorni ho *fatto* *ricamare* un cuscinetto ...
 in two days have.1SG made.PTCP embroider.INF a cushion.DIM
 'I had someone embroider a small cushion in two days.'

The contrast between *umentare* 'increase' and *ricamare* 'embroider' supports the hypothesis that the transitive of the former verb is a causative structure. Thus, this verb participates in the causative alternation. *Migliorare* 'improve' can also be classified as an alternating –SE verb, despite the findings reported in Table 2, because its SE marked intransitive tokens are mostly reflexive (Section 3.2, cf. 7b). I should also note that this verb has the highest transitive percentages (see

Figure 2) and the transitive tokens pass the causativity test mentioned above, as suggested by the synonymy of (12a) with its constructed paraphrase in (12b).

- (12) a. Il divieto di fumo ha *migliorato* la salute dei
the ban of smoke have.3SG improve.PTCP the health of.the
baristi ...
bartenders
- b. Il divieto di fumo ha fatto *migliorare* la salute
the ban of smoke have.3SG make.PTCP improve.INF the health
dei baristi.
of.the bartenders
- ‘The ban on smoking has improved the health of bartenders.’

In Table 3 I show the results on transitivity and anticausative marking, highlighting the two alternating –SE verbs. (In the next section, I shall explain why the other –SE verbs, *marcire* ‘rot’ and *sbocciare* ‘blossom’, are classified as –transitive.)

Table 3: ±Transitive vis-à-vis ±SE behaviour: the alternating –SE verbs

Verb (It)	Verb (En)	Transitive	Anticausative marking
sparpagliare	scatter	+	+SE
sbriciolare	crumble	+	±SE
aprire	open	+	±SE
chiudere	close	+	±SE
rompere	break	+	±SE
cuocere	cook	+	±SE
bruciare	burn	+	±SE
congelare	freeze	+	±SE
sgonfiare	deflate	+	±SE
riscaldare	heat	+	±SE
asciugare	dry	+	±SE
arrugginire	rust	+	±SE
migliorare	improve	+	–SE
umentare	increase	+	–SE
marcire	rot	–	–SE
sbocciare	blossom	–	–SE

Having ascertained that both *umentare* ‘increase’ and *migliorare* ‘improve’ belong in the alternating –SE class, I can explore the properties that differentiate

them from the alternating verbs that admit SE marking. Previous accounts have relied on lexical aspect or a loose principle of spontaneity (Sections 1 and 2). A problem with the idea that alternating verbs do not take SE if they are degree achievements is that many \pm SE alternating verbs (*cuocere* ‘cook’, *asciugare* ‘dry’, *congelare* ‘freeze’, *bruciare* ‘burn’, *sgonfiare* ‘deflate’, *riscaldare* ‘heat’, etc.) are also degree achievements, as suggested by the application of the relevant tests (Dowty 1979, Bertinetto & Squartini 1995, Hay et al. 1999). Among such diagnostics are the entailment of the perfect by the progressive; the compatibility with expressions of measure like ‘a lot’ and with both ‘in’ and ‘for’ temporal adverbials; the fact that when the verb combines with ‘almost’, two readings may be obtained: according to the one, the event has occurred but has not been completed; according to the other, the event has not occurred at all. The context can coerce the one or the other reading (for aspectual coercion see Jackendoff 1997: 51–53, Jackendoff 2002: 290–292; Pustejovsky 1991). I shall leave it for the reader to apply the tests by themselves. Here I should point out that the property of being a degree achievement cannot be the *explanans* of the absence of SE with the alternating –SE verbs since it does not pick out this subclass alone.

As for spontaneity, while increasing and improving may occur more spontaneously than heating and cooking, the consideration of other alternating –SE verbs of Italian does not support the hypothesis that spontaneity is the key factor in the absence of SE marking with these verbs. Alongside the antonyms of *aumentare* ‘increase’ and *migliorare* ‘improve’, *diminuire* ‘decrease’ and *peggiorare* ‘worsen’, the literature cites *affondare* ‘drown/sink’, *cambiare* ‘change’, *guarire* ‘heal’, *crescere* ‘grow’, and *bollire* ‘boil’ as alternating –SE verbs. It is hard to see why boiling events should be more spontaneous than heating events, and so on.

In an analysis of Romance verbs of internal causation (in the sense of Levin & Rappaport Hovav 1995), I argued that the alternating –SE subset of such verbs describes events which do not fully satisfy Wolff’s (2003: 4–5) conditions on direct causation. According to this author, causation is direct if:

- (13) a. There are no intervening entities at the same level of granularity as initial causer and final causee OR
- b. Any intervening entity is construable as an enabling condition rather than a cause.

The alternating –SE verbs of internal causation describe events which can have multiple causes at the same level of granularity (Bentley 2023). A prime example is that of ‘ferment’ verbs, which can describe events caused by humans relying

on the intervention of organisms such as yeast and bacteria. Without such organisms the change cannot occur, and, in fact, yeast and bacteria can cause the change by themselves. Thus, these interveners are not optional enabling conditions, but rather causes in their own right.

The alternating –SE verbs listed above can also describe complex causation chains with indirect causes and necessary interveners, which alone cause the relevant changes. Think of the role of seawater in the sinking of a ship or heated water in the boiling of an egg and contrast these roles with the participation of a key in the opening of a door or a hammer in the smashing of a chair. The last two are optional instruments, which enable the causer to achieve their goal. The first two are not: they cannot be dispensed with and, in fact, they are the true causes of the change (even when the ship has been scuppered and the egg has been put on the hob). To return to the alternating –SE verbs in my sample, economic crises are not the immediate cause of age discrimination (cf. 9), but they can have such discrimination among their ultimate consequences if they result in poorer job offer or similar intervening scenarios, which will involve discriminators. Similarly, the ban on smoking results in the bartenders' reduced exposure to smoke, which in turn will reduce the likelihood of bartenders contracting smoke-related ailments (cf. 12a). Of course, it could be argued that these events occur at different levels of granularity. The key issue, however, is that with these verbs the level of granularity is irrelevant to the encoding of causation chains. As Kiparsky (1997: 477) puts it, "there is no question that the boundaries of direct causation are fuzzy." Alternating –SE verbs in Italian provide a clear illustration of this fuzziness, while also suggesting that indirect causation can be encoded in the lexicon. Key evidence that it is this property that underlies the lack of SE is provided by pairs such as –SE *cambiare* 'change' and its +SE counterpart *cambiarsi*. The latter only means 'to change one's clothes or one facial expression' (<https://www.treccani.it/vocabolario/cambiare/>), these being changes which do not require or admit interveners. +SE *cambiarsi* thus describes direct causation, unlike –SE *cambiare*.

In Section 5.4.1 I shall argue that a constructional constraint rules out SE anticausativization in Italian when the conditions on direct causation are not satisfied. In this perspective, the verbs discussed here do not challenge the argument realization analyses of SE anticausativization, after all.

3.3.2 –SE verbs of quantized change and resultative –SE constructions

The other –SE verbs in my sample were *marcire* 'rot' and *sbocciare* 'blossom' (see Figure 3), exemplified below.

- (14) (itTenTen20, 25/08/2022)
 ...la tettoia ...l' hanno fatta in bambù ed è *marcita* in una
 ... and be.3SG rot.PTCP in one
 stagione.
 season
 'The roof ...they made it with bamboo and it rotted away in one season.'
- (15) (itTenTen20, 12/06/2023)
 I fiori vengono *sobbociano* ed appassiscono in una sola
 the flowers come.3PL blossom.3PL and wither.3PL in one single
 giornata.
 day
 'The flowers come, blossom and wither in a single day.'

Unlike *aumentare* 'increase' and *migliorare* 'improve', *marcire* 'rot' and *sobbocciare* 'blossom' were classified as non-transitivizing because their transitive hits amounted to less than 1% of their overall attestations (see Appendix B). These results are highlighted in Table 4.

I should also mention that the few transitive tokens of these verbs have figurative meanings (cf. 16), i.e., do not describe change in the physical world, or non-conventional meanings (cf. 17), which are not reported in dictionaries and are deemed to be unacceptable by native speakers.⁵

- (16) (itTenTen20, 25/08/2022)
 ...il malcostume ...abbia guastato e *marcito* la questione meridionale ...
 ... rot.PTCP the issue southern
 '...corruption has ruined and rotted the issue of the South.'
- (17) (itTenTen20, 25/08/2022)
 Chiaro senza *sobbocciare* bottiglie in discoteca ogni sera.
 of.course without *sobbocciare* bottles in disco every evening
 'Of course, without opening (?) bottles in the disco every evening.'

Sobbocciare 'blossom' was also unattested in the passive, whereas *marcire* 'rot' yielded four passive tokens (<1% of its overall attestations), one of which translates as 'rust' rather than 'rot'. Overall, the evidence does not support a causative analysis of these verbs, although as was suggested in my study of verbs of internal causation (Bentley 2023), *marcire* 'rot' may have a causative realization in some varieties of Italian.

⁵Drawing on McNally & Spalek (2022), I assume that the grammatically relevant aspects of the meaning of a verb can be carried over to its figurative extensions. Therefore, the evidence in (16) is relevant to the causative alternation, although there is very little of it. I found no evidence of literal *marcire* 'rot' used transitively.

Table 4: \pm Transitive vis-à-vis \pm SE behaviour: The non-alternating $-SE$ verbs

Verb (It)	Verb (En)	Transitive	Anticausative marking
sparpagliare	scatter	+	+SE
sbriciolare	crumble	+	\pm SE
aprire	open	+	\pm SE
chiudere	close	+	\pm SE
rompere	break	+	\pm SE
cuocere	cook	+	\pm SE
bruciare	burn	+	\pm SE
congelare	freeze	+	\pm SE
sgonfiare	deflate	+	\pm SE
riscaldare	heat	+	\pm SE
asciugare	dry	+	\pm SE
arrugginire	rust	+	\pm SE
migliorare	improve	+	$-SE$
aumentare	increase	+	$-SE$
marcire	rot	$-$	$-SE$
sbocciare	blossom	$-$	$-SE$

The two verbs under discussion provide important evidence because they suggest that neither the lexicalization of quantized change nor the resultativity of a construction can alone explain the occurrence of *SE*. I shall begin by noting that neither verb takes *SE* when modified by an ‘in’ temporal adverbial, as can be seen in (14) and (15). Assuming such adverbials coerce a resultative reading (Dowty 1979: 332–336, Hay et al. 1999) these are examples of resultative constructions that lack *SE* marking.

As for lexical aspect, both verbs are attested as participial adjectives, which indicates that they lexicalize a result state of some sort (van Hout 2004, Bentley 2006: 355, Legendre 2017: 283–284). Relevant percentages are given in Appendix B.

- (18) (itTenTen20, 25/08/2022)
 tubature *marcite* da sostituire.
 pipe.FPL rot.PTCP.FPL to replace.INF
 ‘rotten pipes to be replaced’

- (19) (itTenTen20, 25/08/2022)
 un piccolo fiore *sbocciato*
 a small flower.MSG blossom.PTCP.MSG
 ‘a small, blossomed, flower’

However, *marcire* ‘rot’ tests out as a degree achievement and is thus unproblematic for the aspectual hypothesis or a version thereof. Instead, *sbocciare* ‘blossom’ does not combine with expressions of measure or with ‘for’ temporal adverbials, nor does its progressive entail the perfect. Since this verb was attested in the progressive and in combination with ‘begin’, ‘finish’ and the adverb ‘completely’, I shall tentatively analyse it as an accomplishment. Further investigation may reveal an achievement analysis to be more fitting.⁶ However, what matters in the present context is that this verb lexicalizes quantized change and fails to take SE.

In sum, the behaviour of the non-alternating –SE verbs suggests that neither the lexicalization of a specific result state nor the occurrence in a construction that coerces resultativity are sufficient conditions for SE marking in the absence of causation (a point previously made by Jiménez-Fernández & Tubino-Blanco (2017) with reference to Southern Peninsular Spanish).

3.3.3 Principles underlying the ±SE variation

Turning now to the verbs which exhibited variation in the marking with anticausative SE, the evidence suggests that different principles underlie the –SE realization of different subclasses of such verbs. With one subclass, only comprising degree achievements, the absence of SE loosely correlates with the description of properties or processes. Instead, the –SE realization of the other subclass is underpinned by indirect causation. This latter subclass includes accomplishment verbs which have so far been labelled as +SE in the literature. The verbs of the first subclass may also fail to exhibit SE when the cause is not direct, whereas the verbs of the second subclass never fail to exhibit SE when describing properties. The remaining cases of ±SE variation will be subsumed under the label of polysemy. In Section 4 I discuss these findings in depth.

⁶In combination with ‘almost’, *sbocciare* ‘blossom’ seems to us to yield a counterfactual reading, which suggests that it might be an achievement.

4 The ±SE variation

4.1 Absence of SE: Properties and processes

Scrutiny of the verbs which are known from the literature to exhibit ±SE variation (see Tables 1 and 2) reveals that the –SE intransitives of these verbs can describe properties or processes. The cause is irrelevant in property descriptions because what is at issue is a feature of the causee.

(20) (itTenTen20, 23/08/2022)

*Cuociono e si sfaldano lentamente (come tutti i legumi della stessa
cook.3PL and SE flake.3PL slowly ...
consistenza.)*

...

‘[Yellow split peas] cook and flake slowly like all pulses with the same texture.’

(21) (itTenTen20, 30/08/2022)

*Realizzato in una microfibra leggera, che asciuga rapidamente.
make.PTCP in a microfiber light REL dry.3SG quickly*

‘Made in a light microfiber, which dries quickly.’

(22) (itTenTen20, 02/09/2022)

*I tuberi congelano a –2°C.
the bulbs freeze.3PL at –2°C*

‘The bulbs freeze at –2°C.’

The lack of a specific time reference and the occurrence of manner adverbials are hallmarks of the construction that is sometimes referred to as “facilitative” (Kemmer 1993: 147–149, see also Levin 1993: 25–26). SE is not ruled out from this construction, as evidenced by an example with a verb from the putatively +SE subclass.

(23) (itTenTen20, 02/09/2022)

*... le chiome risultano secche, asciutte e si rompono facilmente.
... the hair become.3PL dry dry and SE break.3PL easily*

‘Hair becomes dry and breaks easily.’

Therefore, the absence of SE in (20–22) cannot be the spell-out of a property description construction. Instead, let us temporarily assume that it is a strategy

that leaves out irrelevant facets of verb meaning, namely the cause. The –SE intransitives of the verbs that are normally labelled as ±SE can also describe the unfolding of events of change of state.

(24) Description of processes:

- a. (itTenTen20, 23/08/2022)
 Mentre le uova *cuociono*, prepariamo la salsa olandese.
 while the eggs cook.3PL ...
 ‘While the eggs are cooking, we make the hollandaise sauce.’
- b. (itTenTen20, 30/08/2022)
 ... lo chassis ... che già *sta asciugando* in terrazzo.
 ... the chassis ... REL already be.3SG dry.GERUND in balcony ...
 ‘The chassis which is already drying on the balcony.’
- c. (itTenTen20, 02/09/2022)
 Infilare nel Freezer a *congelare* per una mezzoretta.
 put.in.INF in.the freezer at freeze.INF for one half.hour.DIM ...
 ‘Put it in the freezer (to freeze) for half an hour.’

Similar considerations can be made for *bruciare* ‘burn’, *sgonfiare* ‘deflate’ and *riscaldare* ‘heat’, although for brevity I shall not provide any exemplification here. The evidence in (24) contrasts with that in (25), where what is being described is change leading to meat rolls being cooked, hair being dry, and mammoths being frozen. In this case the verb is marked with SE.

(25) Descriptions of change leading to result state:

- a. (itTenTen20, 23/08/2022)
 Dopo che si *sono cotti*, ... tagliarli a fettine.
 after that SE be.3PL cook.PTCP ... slice.them to slices.DIM
 ‘After they [meat rolls] have cooked, slice thinly.’
- b. (itTenTen20, 30/08/2022)
 Quando i capelli si *sono asciugati* ... appaiono come “incollati”.
 when the hair SE be.3PL dry.PTCP seem.3PL like glued
 ‘When the hair has dried, it will seem glued together.’
- c. (itTenTen20, 02/09/2022)
 ... i mammut, mentre mangiavano ... si *sono congelati*
 ... the mammoths while eat.PST.3PL SE be.3PL freeze.PTCP
 all’istante.
 at.the.instant
 ‘While the mammoths were eating, they froze instantly.’

I note that the –SE intransitives are not incompatible with expressions that coerce the notion of a result state being attained (cf. 26), while the +SE ones can also describe the unfolding of events of change (cf. 27–28).

- (26) (itTenTen20, 23/08/2022)
È pasta fresca, quindi *cuocerà* *nell' arco di pochi minuti.*
is pasta fresh so cook.FUT.3SG in.the arch of few minutes
'It is fresh pasta, so it will cook in a couple of minutes.'
- (27) (itTenTen20, 30/08/2022)
Bisogna riportare il disegno ... e lasciare che *si asciughi* *per circa 2*
... that SE dry.SBJV.3SG for circa 2
giorni.
days
'One needs to take the drawing back ... and let it dry for about two days.'
- (28) (itTenTen20, 02/09/2022)
Mentre l' acqua si congela, si espanderà
as the water SE freeze.3SG SE expand.FUT.3SG
allargando la pelle nel processo ...
...
'As the water freezes, it will expand, extending the skin in the process.'

Example (26) shows that 'in' temporal adverbials do not necessarily trigger SE marking, while the following examples indicate that the description of duration rather than completion is not incompatible with SE marking: see the 'for' temporal adverbial in (27) and the temporal subordinator *mentre* 'as/while', as well as the reference to a process, in (28).

Overall, the evidence jars with the analysis of SE as the spell-out of a resultative construction, i.e., a construction that provides a resultative meaning component. However, it does not rule out the hypothesis that a facet of the meaning of the relevant verbs underpins the general tendencies mentioned above. Application of the diagnostics mentioned in Section 3.3.1 reveals *cuocere* 'cook', *asciugare* 'dry', *congelare* 'freeze', *bruciare* 'burn', *sgonfiare* 'deflate' and *riscaldare* 'heat' to be degree achievements. I noted that lexical aspect, and in particular the properties of degree achievements, cannot alone explain the distribution of SE. Nonetheless, these properties turn out to have a bearing on the ±SE variation described in this section.

4.2 Absence of SE: Indirect causation

The –SE attestations of *chiudere* ‘close’, *aprire* ‘open’, *rompere* ‘break’ and *sbriciolare* ‘crumble’ do not describe properties or processes, as is suggested by perfect aspect in (29–31) and the temporal adverbials indicating when the change has been or will be completed in (29), (30) and (32).

- (29) (itTenTen20, 15/01/2023)
 I seggi hanno chiuso ieri sera alle 22.
 the polling.stations have.3PL close.PTCP yesterday night at.the 10
 ‘The polling stations closed at 10pm last night.’
- (30) (itTenTen20, 31/10/2022)
 La casa ha aperto 4 mesi fa,
 the home have.3SG open.PTCP 4 months ago
 però il lavoro con la comunità è iniziato prima.
 ...
 ‘The home opened four months ago, but the work of the community began before then.’
- (31) (itTenTen20, 27/11/2022)
 Giaceva sul letto e la struttura del letto ha rotto a tutti e
 ... have.3SG break.PTCP at all and
 quattro gli angoli.
 four the corners
 ‘He was lying on the bed and the frame of the bed broke at all four corners.’
- (32) (itTenTen20, 31/08/2022)
 Entro massimo due anni sbriciolerà.
 within maximum two years crumble.FUT.3SG
 ‘Within two years it will crumble.’

A clue to understand the absence of SE with these verbs is provided by the observation that these –SE intransitives are incompatible with the addition *da sé/da solo* ‘by itself’ in the sense of ‘without outside help’. Since Chierchia (1989) this adverbial has been cited as a causative diagnostic. Alexiadou et al. (2015: 76–79) claim that *da sé/da solo* ‘by itself’ does not identify a causer, but rather rejects the participation of a causer in the event. Since the verbs under discussion participate in the causative alternation, they should readily combine with ‘by itself’. Indeed, they do, when SE marked.

(33) (itTenTen20, 17/01/2022)

- a. Mi sono solo avvicinato alla maniglia e la porta *si è*
... and the door *SE be.3SG*
aperta da sola.
open.PTCP by alone
'I simply approached the handle and the door opened by itself.'
- b. Dopo due mesi la ferita *si è chiusa da sola.*
after two months the wound *SE be.3SG close.PTCP by alone*
'After two months the wound closed by itself.'

An effect of the rejection of the participation of the causer is that it strengthens an inference of responsibility on the part of the causee, which arises with *SE* marking (Zribi-Hertz 1987, and references therein, Kailuweit 2012, Martin & Schäfer 2014, see Section 5.4.1 for further discussion). This inference is irreconcilable with the construction discussed in this section because this gives rise to another inference, i.e., that any direct cause is irrelevant and there may be a remote cause. Example (29) is about the time when it was no longer possible to exercise the right to vote: the closing, and closers, of the physical doors of the polling stations are irrelevant. Similarly, example (30) does not describe the physical opening of the doors of a house, but rather makes the point that the community under discussion has had a home for four months. In (31) the implication is that the bed must have been faulty and, therefore, the cause of its breaking, presumably the manufacturers, is more remote than the person who lay on it and the bed itself. Lastly, (32) is part of a complaint for inaction and lack of maintenance, which is likely to result in the crumbling of a building: there is no physical “crumbler” in this event, nor is the building itself responsible for the crumbling.

The addition of *da sé/da solo* ‘by itself’ is non-sensical in concomitance with the perfect auxiliary *avere* ‘have’, which characterizes the *-SE* pattern under discussion here (cf. 29–21), because ‘by itself’ and the *-SE*, +perfect ‘have’ pattern trigger contradictory inferences: responsibility of the causee and, respectively, indirect causation and irrelevance of any direct cause. I should note that verbs of change of state are expected to select *essere* ‘be’ in the perfect in Italian (cf. 8, 14 and see Perlmutter 1989 and the subsequent literature on the semantic parameters of perfect auxiliary selection). Therefore, the selection of *avere* ‘have’ is a constructional feature, which is not predicted by the general rules of the morphosyntax of Italian.

The construction described in this section is not subject to lexical-aspectual constraints. Starting from the verbs in (29–32), *chiudere* ‘close’ and *aprire* ‘open’

are accomplishments: they are compatible both with the progressive and with ‘in’ temporal adverbials; when combined with ‘for’ temporal adverbials, they describe the duration of the result state (being closed or opened); lastly, their progressive does not entail the perfect. *Rompere* ‘break’ is best analysed as an achievement, in that it does not easily combine with ‘for’ temporal adverbials, and it only admits the progressive in a delayed or slow-motion reading (Bertinetto & Squartini 2016, Vivanco 2021). Lastly, *sbriciolare* ‘crumble’ tests out as a degree achievement: it is compatible with expressions of measure, its progressive entails the perfect, etc. A relevant example with another degree achievement verb, *bruciare* ‘burn’, is given here. (This verb was also found to alternate in accordance with the pattern described in 4.1.)

- (34) (itTenTen20, 19/06/2023)
 Ho fatto un pò [sic.] tutto, ristorante, pizzeria ...
 Io: Ah ok. E poi sei venuta in Irlanda.
 Lei: Sì sì, poi il ristorante ha *bruciato* ...
 ... yes yes then the restaurant have.3SG burn.PTCPG ...
 Io: ... bruciato?
 Lei: eh ... sì, lui proprio stronzo, non pagava mai ... poi il ristorante ha *bruciato* ...
 ‘I did all sorts, restaurant, pizzeria ...
 Me: Ah ok. And then you came to Ireland.
 Her: Yes, yes, then the restaurant burned down ...
 Me: Burned down?
 Her: And ... yes, he was a real [insult], he never paid ... then the restaurant burned down.’

The selection of the –SE, +perfect ‘have’ pattern in (34) indicates that the real cause of the burning of the restaurant is the behaviour of the owner, who failed to pay the employees. Although the +SE pattern would be grammatical in this context, it would not be as felicitous, because it would not satisfy the communicative intentions of the speaker. In fact, the +SE pattern could be argued to flout the Gricean Maxim of Manner, delivering a message which is not as perspicuous as possible.

In conclusion, analysis of the ±SE variation has allowed us to identify two –SE patterns, the one being underpinned by an aspectual principle, the other describing indirect causation. The former pattern, unlike the latter, was found to be restricted to a lexical-aspectual subclass of alternating verbs, namely degree achievements.

be construed as a verb of emission and its selection is expected in contexts that coerce non-completion and admit the emission construal.

I should also mention figurative polysemy, which was observed with *congelare* ‘freeze’, *sgonfiare* ‘deflate’, *riscaldare* ‘heat’ and *asciugare* ‘dry’. Although the literal meanings of these verbs were attested both in the –SE and in the +SE intransitive, the same verbs turned out to take SE consistently in their figurative extensions describing change that does not occur in the physical world.

- (39) (itTenTen20, 02/09/2022)
 ... i vostri sentimenti *si erano congelati* senza motivo.
 ... the POSS feelings SE be.PST.3PL freeze.PTCP without reason
 ‘Your feelings had petered out (lit. frozen) for no reason.’
- (40) (itTenTen20, 30/08/2022)
 ... la scrittura *si asciuga*.
 ... the writing SE dry.3SG
 ‘The inspiration to write dries up.’
- (41) (itTenTen20, 19/08/2022)
 Il nostro animo *si riscalda* e la nostra avventura può continuare.
 the POSS soul SE heat.3SG and ...
 ‘Our souls warm up and my adventure can continue.’
- (42) (itTenTen20, 02/09/2022)
 Poi la polemica *si sgonfiò*.
 then the controversy SE deflate.3SG
 ‘Then the controversy petered out (lit. deflated).’

In such cases the cause seems to us not to be at issue (see the addition of ‘for no reason’ in 39) and the effect of the change on the causee to be in focus. I return to this point in Section 5.4.1.

5 A parallel architecture account of the causative alternation in Italian

5.1 Against *reductio ad unum*

The facts presented in the previous sections challenge the analyses that reduce the causative alternation to a single principle, regardless of whether this is conceived of in lexical or syntactic terms or, indeed, in terms of the mapping of

the lexicon with syntax. The analyses that correlate the occurrence of SE with a facet of lexical meaning are not supported by evidence from verbs that do not behave as predicted. Similar problems are encountered by the analyses which take SE to spell out the aspectual properties of a syntactic projection, a point previously made by Alexiadou et al. (2015: 82–88). Such analyses also have no explanatory power when it comes to capturing the occurrence of SE in constructions which are not constrained in terms of aspect (passives, impersonals and reflexives). On the other hand, the accounts of SE as an argument realization strategy do not explain the different behaviour of different classes of verbs, unless they are combined with a theory of the lexicon. Significantly, the aspectual principle has turned out to be but one of the factors correlated with SE marking (Section 4.1), another factor being the type of causation described by the verb or the construction (Sections 3.3.1 and 4.2). Lastly, the non-causative verbs in the sample turned out not to take SE in the intransitive (Section 3.3.2).

Our contention is, therefore, that a *reductio ad unum* approach is not helpful in the analysis of the distribution of SE. Instead, a parallel architecture account is needed to grasp both the general properties of the semantics-syntax interface which are relevant to the causative alternation and the properties of the lexicon which explain the different marking and behaviour of different subclasses of alternating verbs, including their occurrence in, or ban from, the subconstructions that can be subsumed under the label of anticausativization (Sections 4.1, 4.2, and 4.3). Parallel architecture theories are modular (Jackendoff 2002): each level of representation is formalized independently, and grammatical competence comprises the competence in the principles which govern the interplay of the different modules. The parallel architecture framework in which I couch my account of the causative alternation in Italian, Role and Reference Grammar (Van Valin 2023 and references therein), makes two analytical assumptions which will prove to be particularly helpful for my purposes: (i) lexical items are stored with lexical-semantic representations which comprise both idiosyncratic and grammatically relevant facets of meaning; (ii) grammar includes both general principles that have cross-linguistic or cross-constructional validity and an inventory of Constructional Schemas, which provide construction-specific instructions.

5.2 A typology of verbs of change of state

According to Van Valin & LaPolla (1997: 82–129), Van Valin (2005: 32–49), and Van Valin (2023: 94–107), the semantic representation, or Logical Structure (LS), of predicates is built following decompositional principles introduced by Vendler

(1967) and Dowty (1979). The lexical entry of a verb stem can be associated with a single LS or with two or more LSs, which will be related to each other in terms of the general principles of lexical decomposition. Within this system, it is possible for a stem describing change to be stored in the lexicon as inchoative or causative or, alternatively, to be underspecified for cause and thus be associated with both an inchoative and a causative LS (Brocher & Van Valin 2017). Non-templatic meaning has a bearing on the storing of a stem as one of the three types. While this threefold possibility may at first seem uneconomical, it only includes two LS types, inchoative and causative, the latter being built upon the former, following general principles for the construction of verb meaning. In the remainder of this section, I discuss three verbs from the sample, which, I claim, are stored as inchoative, underspecified and, respectively, causative.⁷

A survey of verbs of internal causation (Bentley 2023) revealed that the Italian verbs which describe the genetic predisposition of specific botanical entities to undergo specific changes as part of their life cycle do not transitivize or passivize. Alongside *sbocciare* ‘blossom’, which is in my current sample and has yielded results which are consistent with those collected earlier (see Appendix B), *fiorire* ‘flower’, *gemmare* ‘gemmate’ and *attecchire* ‘take root’ also belong to the same class.⁸ In contrast, verbs describing the propensity for certain entities to react to specific triggers producing a result that is lexicalized – and often named – in the stem can transitivize and passivize, although they do so infrequently. Examples of this subclass are *arrugginire* ‘rust’, which is in my sample, *ammuffire* ‘(make something) become mouldy’ and *infeltrire* ‘felt’. In that study I claimed that the former class is inchoative, while the latter is underspecified for cause, which means that the relevant stems are associated with inchoative and causative LS pairs. I thus represented the meaning of *sbocciare* ‘blossom’ with the following accomplishment LS.

(43) PROC becoming.higher.on.[blossom]scale' (x) ∧ FIN blossomed' (x)

The representation in (43) includes the defining components of accomplishment LSs: a process (PROC becoming.higher.on.[blossom]scale' (x)) and a spe-

⁷Although this proposal is not vastly dissimilar from the claim of Piñon (2001) that all causative alternations are equipollent (in the terms of Haspelmath 1993), with both inchoative and causative-inchoative verbs deriving from alternating stems, for us SE is not the spell-out of a derivation. Rather, it must be the marker of an operation on the highest argument position in semantic representation, as indicated by its occurrence not only in anticausatives but also in the other grammatical domains listed in Section 1.

⁸However, *gemmare* ‘gemmate’ (<https://www.treccani.it/vocabolario/gemmare/>) can be transitive in a figurative sense.

cific result state (FIN **blossomed'** (x)). The concomitance symbol \wedge indicates that the result state is the final stage of the process.

In contrast with *sbocciare* 'blossom', *arrugginire* 'rust' can be represented with the two logical structures in (44a-b), where FIN **rusted.by.some.amount'** (x) is the non-specific result state lexicalized by a verb of non-quantized change (a degree achievement).

- (44) a. PROC **becoming.higher.on.[rust]scale'** (x) \wedge FIN
rusted.by.some.amount' (x)
 b. [**do'** (x, [Ø])] CAUSE PROC **becoming.higher.on.[rust]scale'** (y) \wedge
 FIN **rusted.by.some.amount'** (y)

That *arrugginire* 'rust' is a degree achievement is suggested, among the other diagnostics introduced in Section 3.3.1, by its compatibility with expressions of measure like 'a little'.

- (45) ... quel campanello ... Forse *si era* un po' *arrugginito* ...
 ... that bell ... perhaps SE be.PST.3SG a little rust.PTCP ...
 'That bell ... perhaps it had become a little rusty.'

The two LSs in (44a-b) differ in that the latter includes the representation of a cause, [**do'** (x, [Ø])]. The argument of [**do'** (x, ...)] is an effector (Van Valin & Wilkins 1996), which means that the verb is not lexically agentive. Indeed, it takes inanimate transitive subjects.

- (46) (itTenTen20, 24/08/2022)
 Le impurità dell'acqua e l'ossigeno in eccesso potevano ...
 ... can.PST.3PL
arrugginirli.
 rust.INF.them
 'The impurities in the water and excessive oxygen could rust them.'

In addition, the transitive of *arrugginire* 'rust' can be paraphrased with a periphrastic *fare* 'make' causative, which is further evidence against an agentive analysis (Section 3.3.1). The symbol \emptyset in the square brackets in (44b) indicates that the lexicon does not specify exactly how the cause brings about the change.

As for verbs that are lexicalized as causative, these describe change that is not "self-sustaining", in the sense of Kiparsky (1997: 496), i.e., change that "requires

the initiation and continuous participation of a causing Agent”: *rompere* ‘break’ is one such verb and I assume it to be stored as a causative achievement.⁹

(47) [*do*’ (x, [∅])] CAUSE INGR *broken*’ (y)

Sparpagliare ‘scatter’, *sbriciolare* ‘crumble’, *chiudere* ‘close’ and *aprire* ‘open’ are also potential candidates for the class of verbs that are stored as causatives.¹⁰

5.3 –SE inchoatives of alternating degree achievements

The assumption that alternating stems can be associated with the pair of logical structures in (44) is key to the analysis of the ±SE variation that was introduced as aspectually principled (Section 4.1). Recall that the evidence challenges the predictions of the constructionist analyses which associate SE with resultativity and the absence of SE with lack of resultativity (cf. 26–28).

However, the same evidence does not rule out the hypothesis that a facet of the meaning of the relevant verbs underpins the general tendency towards the lack of SE in descriptions of properties and processes. In fact, this tendency was only found with verbs of change that do not lexicalize a specific result state (Cennamo 2012), i.e., degree achievements.

The evidence presented in Section 4.1 is neither contradictory nor problematic if the –SE occurrences of the degree achievement verbs *cuocere* ‘cook’, *asciugare* ‘dry’, *congelare* ‘freeze’, *bruciare* ‘burn’, *sgonfiare* ‘deflate’ and *riscaldare* ‘heat’ are realizations of an inchoative LS. Starting from the logical structure in (48) (cf. 44a), the PROC ... component alone may be aspectually coerced.

(48) PROC *becoming.higher.onscale*’ (x) ∧ FIN ... *ed.by.some.amount*’ (x)

This type of construal is compatible with the non-specific result state of degree achievements but difficult with verbs that lexicalize a specific result state.

⁹See Van Valin (2023: 114) for the LS of achievements. The representation of the causer as the argument of [*do*’ (x, [∅])] indicates that agentivity is not lexicalized, although agentivity inferences arise with human transitive subjects. The fact that the change described is not self-sustaining may be another factor in these inferences.

¹⁰The prefix *s-*, mentioned in Section 3.2, can be reversative, as in *sgonfiare* ‘deflate’, which is the antonym of *gonfiare* ‘inflate’ (Iacobini 2004). Alternatively, the same prefix is the marker of parasynthetic denominal or deadjectival verbs, for which the corresponding non-prefixed verb or prefixed noun/adjective are not attested. There is no corresponding verb **briciolare* or noun **sbriciola* for Italian *sbriciolare* ‘crumble’, derived from the noun *briciola* ‘crumb’. Such parasynthetic formations are compatible with both inchoative (e.g., *sbocciare* ‘blossom’) and causative LSs (*sbriciolare* ‘crumble’).

The logical structure in (48) is also compatible with a resultative construal, i.e., with the predication of the attainment of a non-specific result state. Indeed, such examples are found, as suggested by the perfect aspect and the ‘in’ temporal adverbial in (49).

- (49) (itTenTen20, 02/09/2022)
Un ascesso *é* [sic] *sgonfiato* in venti minuti.
an abscess be.3SG deflate.PTCP in twenty minutes
‘An abscess went down (lit. deflated) in twenty minutes.’

In my analysis (49) is inchoative and not anticausative. Note that the perfect auxiliary is *essere* ‘be’, as is expected with verbs of change of state in Italian. This is a key morphosyntactic clue that differentiates –SE inchoatives from the –SE structures with an indirect cause introduced in Section 4.2.

As for property descriptions, when an inchoative LS is available, this will be retrieved from the lexicon (cf. 20–22). When only a causative LS is available, the cause must be suppressed. Causative lexicalization is, thus, the reason why *rompere* ‘break’ was found to take SE in property descriptions (cf. 23). The causative analysis of *sparpagliare* ‘scatter’, *sbriciolare* ‘crumble’, *chiudere* ‘close’ and *aprire* ‘open’ predicts that these verbs also fail to occur in unmarked (i.e., –SE) property descriptions.

In sum, a theory of the lexicon which, on the one hand, differentiates between verbs that lexicalize quantized and non-quantized change and, on the other, assumes that the lexical entries of verbs of change can be underspecified for cause, and thus be associated with both an inchoative and a causative LS, allows us to capture the fact that only a subset of alternating verbs are found in –SE descriptions of processes and properties: these are degree achievements that are underspecified for cause.

5.4 Anticausativization as causer suppression

Drawing on the causer suppression analyses (Section 2), I treat anticausativization as a monotonic operation, which does not delete any component of meaning, but rather stops the causer from being treated as an argument of the predicate in syntax.¹¹ This operation occurs in the lexical phase of the semantics-syntax

¹¹A cause can, however, appear in an adjunct as the argument of a predicative adposition (e.g., *La porta si chiuse per la forte corrente* ‘The door shut because of the strong draught’).

linking (Van Valin 2023: 116–125), when values are assigned to the argument variables in the semantic representation of the clause.¹² Anticausativization amounts to the assignment of a silent value to the causer variable, as seen in [*do*' (\emptyset , ...)] in (50), which is a simplified representation of (45) (*quel campanello si era arrugginito* ‘that doorbell had rusted’).¹³

- (50) [*do*' (\emptyset , [\emptyset])] CAUSE PROC **becoming.higher.on.** [*rust*] **scale**'
(campanello) \wedge FIN **rusted.by.some.amount**' (campanello)

Following suppression, the causer cannot be linked to syntax as an argument of the verb. The next argument down, the causee, is thus treated as the subject.¹⁴

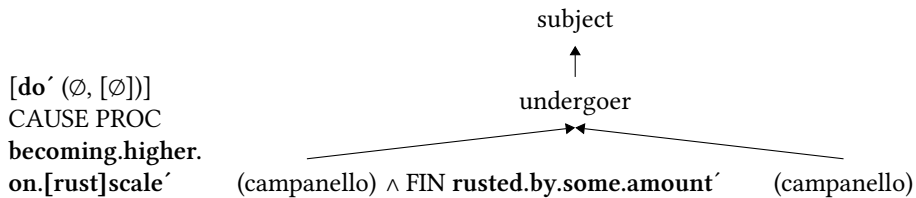


Figure 3: Causer suppression and treatment of the causee as the subject.

The representation in Figure 3 includes a step of the linking, macrorole assignment, which is important to grasp generalizations that other frameworks formalize in terms of argument structure. There are only two macroroles, actor and undergoer, which are assigned in accordance with explicit principles grounded in the hierarchical relations between the five argument positions that are possible in Logical Structure (Van Valin & LaPolla 1997: 82–198). The overt argument of Figure 3 (*campanello* ‘doorbell’) is an undergoer because it is the lower candidate for macrorolehood of a two-place predicate. The status of the subject as the lower argument in logical structure will turn out to be relevant to SE marking.

¹²The semantic representation of the clause is built from composition of the LSs of the verb stem and of any other predicating elements, for example, any predicative adpositional phrases.

¹³The definiteness properties of the argument and the aspectual properties of the predicate would be reflected in the full representation of the clause, which I do not provide here.

¹⁴While rejecting the grammatical relations subject and object as primitives or universals of syntactic theory, my framework does allow for construction-specific grammatical relations, which are restrictions on the semantic relations or discourse roles that are admitted in the given construction (Van Valin 2023: 242–309, LaPolla 2023). For simplicity, I shall use the term “subject”, referring primarily to the controller of the person and number agreement specifications on the main verb in the clause.

Having introduced anticausativization as an operation that assigns a silent value to the causer variable in the lexical phase of the linking, I can now distinguish two anticausativization constructions, which I call overt and, respectively, labile.

5.4.1 Overt anticausativization

Causer suppression can be marked by the morpheme *SE*, in which case anticausativization is overt. To understand the rationale of this marking it is necessary to consider the other domains where *SE* figures systematically, namely passives, impersonals and reflexives (Sections 1 and 3.1). In all cases, *SE* marks the suppression of the highest-ranking argument in the semantic representation of the clause. Consider the *SE* passive in (51a), with its semantic representation in (51b), and the *SE* impersonal in (52a), with its representation in (52b).

(51) *Passive SE*

- a. Si vedono le immagini (di ...)
SE see.3PL the images of
'The images (of ...) are seen.'
- b. *see'* (\emptyset , immagini)

(52) *Impersonal SE*

- a. Si sviene.
SE faint.3SG
'One faints.'
- b. INGR **fainted'** (\emptyset)

The suppressed argument in the passive in (51) and the impersonal in (52) is the highest argument of a state and, respectively, the only argument of an achievement. In (51), argument suppression results in the next argument down serving as the controller of person and number agreement on the verb. In (52), there is no argument available for this syntactic function. *SE* marking is independent of the lexical aspectual properties of the predicate and the related thematic properties of the suppressed argument. Instead, what *SE* signals is a deviation from accusative alignment, since the highest argument in semantic representation, which is unavailable in (51–52), is the unmarked selection for subject in accusative alignment (Van Valin & LaPolla 1997: 175). The role of *SE* marking in Italian morphosyntax is thus comparable to that of the perfect auxiliary *essere* 'be', which also marks a deviation from accusative alignment in the selection of

the subject (La Fauci 1988, Bentley 2006, Ledgeway 2012, Loporcaro 2016, etc.). This analysis explains why non-alternating verbs fail to exhibit anticausative SE in the intransitive: since there is no causer position in LS there can be no anticausativization or anticausative SE marking (see Section 3.3.2 and the Logical Structure in (42)).

The overt making of causer suppression has the effect of backgrounding the cause component of event structure ([do' (...)] CAUSE ...) and highlighting the components that have an overt argument in them. This triggers an inference of responsibility on the part of the causee, hence the compatibility with 'by itself', which rejects the participation of the causer in the event (Sections 3.3.1 and 4.2).

- (53) Il cofano ... deve *chiudersi* da solo automaticamente ...
 the trunk ... must.3SG close.SE by alone automatically
 'The trunk must close by itself automatically.'

Example (53) states that the trunk should close automatically: the intervention of a causer is not needed.

The case of the alternating –SE verbs of Section 3.3.1 (*umentare* 'increase' and *migliorare* 'improve') can be explained in terms of the interaction of the type of causation described by these verbs and the backgrounding of the cause and responsibility inferences that arise in SE anticausativization. Recall that the alternating –SE verbs describe events where the conditions on direct causation are not satisfied, in that there can be both indirect causes and interveners which are causes in their own right. This type of complex causation does not easily combine with the backgrounding of the cause and responsibility inferences, which highlight the role of the causee as a direct cause in the event. I thus propose that the Constructional Schema (CS) for overt anticausativization includes an explicit instruction, in Italian, which stops these verbs from participating in it, unless they describe direct causation in a given context (see the case of *cambiarsi*, meaning 'change one's clothes or expression').

- (54) Constructional Schema of overt anticausativization (preliminary)
 SEMANTICS
 <Causer is realized as \emptyset >
 <Conditions on direct causation are satisfied>
 ...
 MORPHOSYNTAX
 <Suppression of highest-ranking argument is marked with SE>
 ...

DISCOURSE-PRAGMATICS

<Backgrounding of cause>

<Inference of responsibility of causee>

...(see below)

The intransitive and transitive occurrences of *aumentare* ‘increase’ and *migliorare* ‘improve’ are realizations of the inchoative and, respectively, causative LSs associated with their stem, as suggested by the selection of the expected perfect auxiliary *essere* ‘be’ in the intransitive (cf. 8).¹⁵

Alongside the backgrounding of the cause, another inference arises in overt anticausativization. Drawing on Lyons’ (1969: 373) treatment of the middle voice (“the ‘action’ or ‘state’ affects the subject of the verb or his interests”), I call this affectedness. Whether the context brings to the fore the process (cf. 27, repeated in 55) or the result (cf. 25b, repeated in 56), SE marking highlights the effects of the change on its undergoer.

(55) (itTenTen20, 30/08/2022)

Bisogna riportare il disegno ... e lasciare che si *asciughi* per circa 2
... that SE dry.SBJV.3SG for circa 2
giorni.
days

‘One needs to take the drawing back ... and let it dry for about two days.’

(56) (itTenTen20, 30/08/2022)

Quando i capelli si sono *asciugati* ... appaiono come “incollati”.
when the hair SE be.3PL dry.PTCP ... seem.3PL like glued
‘When the hair has dried, it will seem glued together.’

SE marking of the figurative extensions of ‘freeze’, ‘dry’, ‘heat’ and ‘deflate’ (Section 4.3) is explained in terms of the affectedness inference of overt anticausativization, since what is at issue in such cases is not the cause but the change and the effect that it has on the undergoer. Consider (39), repeated here as (57).

(57) (itTenTen20, 02/09/2022)

... i vostri sentimenti si erano *congelati* senza motivo.
... the POSS feelings SE be.PST.3PL freeze.PTCP without reason
‘Your feelings had petered out (lit. frozen) for no reason.’

¹⁵Not a single intransitive hit with *aumentare* ‘increase’ or *migliorare* ‘improve’ exhibited the perfect auxiliary *avere* ‘have’ in my corpus.

This example describes the petering out of feelings, qualifying the change as ‘unmotivated’. *SE* marking satisfies the Maxim of Manner, enhancing perspicuity, by backgrounding the cause and foregrounding the effect on the undergoer. The CS in (54) should thus be revised as follows.

- (58) Constructional Schema of overt anticausativization (abridged)
- SEMANTICS
 <Causer is realized as \emptyset >
 <Conditions on direct causation are satisfied>
 ...
 MORPHOSYNTAX
 <Suppression of highest-ranking argument is marked with *SE*>
 ...
 DISCOURSE-PRAGMATICS
 <Backgrounding of cause>
 <Inference of responsibility of causee>
 <Inference of affectedness of causee>

In sum, I have argued that causer suppression can be marked overly by the morpheme *SE*, which here and elsewhere spells out the suppression of the highest-ranking argument in the clause. In the overt anticausativization construction, *SE* marking backgrounds of the cause component of event structure and foregrounds the causee, with concomitant inferences of responsibility (Zribi-Hertz 1987: 24 and references therein, Kailuweit 2012, Martin & Schäfer 2014) and affectedness (Lyons 1969) of the causee. These inferences underpin the systematicity of *SE* marking with the figurative extensions of a cluster of $\pm SE$ verbs and a language specific constructional instruction, which stops descriptions of complex causation chains from occurring in overt anticausativization.

5.4.2 Labile anticausativization

What happens when a verb which is lexicalized as causative describes indirect causation in a given context? In such cases, Italian resorts to labile anticausativization (Bentley 2021), which is an operation of causer suppression that is not spelled out by *SE* marking and thus lacks the inferences that arise from such marking. The evidence was discussed in Section 4.2; consider (59), which is an abridged version of (31).

- (59) La struttura *ha* rotto.
 the frame have.3SG break.PTCP
 ‘The frame broke.’

That this example could not be the realization of an inchoative LS is indicated by the selection of the perfect auxiliary *avere* ‘have’, which contrasts with *essere* ‘be’ in (8), (14) and (49). The selection of *avere* in (59) must thus be analysed as a constructional device, which signals anticausativization (cf. Figure 4) without triggering the causer defocusing and causee responsibility inferences of overt anticausativization. As I mentioned, the implication of this example is that the bed must have been faulty and, therefore, the cause of its breaking, the manufacturer, is more remote than the person who lay on the bed or the frame itself. A simplified version of the semantics-syntax linking in (59) is given in Figure 4.

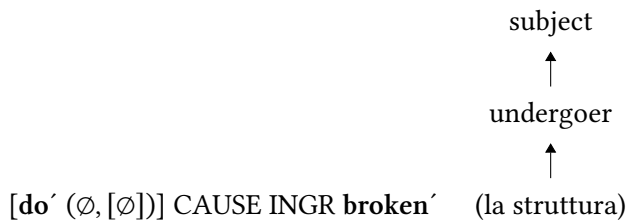


Figure 4: Semantics-syntax linking in (59)

Both the selection of ‘have’ and the lability of causer suppression are explicitly specified in the Constructional Schema of labile anticausativization, as otherwise the syntax of Italian would require that the flouting of the accusative principle in subject selection be flagged overtly with SE and/or, in the perfect, *essere* ‘be’ (Section 5.4.1).

- (60) Constructional Schema of labile anticausativization (abridged)
- SEMANTICS
- <Causer is realized as \emptyset >
- <Conditions on direct causation are not satisfied>
- ...
- MORPHOSYNTAX
- <Suppression of highest argument is not marked overtly>
- <Perfect auxiliary *avere* ‘have’ is selected>
- ...
- DISCOURSE-PRAGMATICS
- <Inference of irrelevance of direct cause>

Labile anticausativization is not the output of the linking of an inchoative LS with intransitive syntax, but rather a construction which does not impose any

lexical-aspectual restrictions on the verbs that appear in it. Indeed, it was illustrated with an achievement (cf. 31), two degree achievement verbs (cf. 32, 34), and two accomplishment verbs (cf. 29–30).¹⁶ A parallel architecture account that relies on lexical decomposition, general principles of semantics-syntax linking and language specific Constructional Schemas can capture why the aspectual \pm SE variation is constrained to a particular subclass of alternating verbs, degree achievements that are underspecified for cause, whereas the \pm SE variation that is underpinned by the indirect causation principle is not similarly constrained.

5.5 A note on the broader picture

Apart from a brief mention of SE passives and impersonals (Section 5.4.1), I have not examined SE marking beyond anticausativization, and space constraints do not allow me to do so here. An attempt at capturing the distribution of SE in Italian grammar was made in Bentley (2006), starting from similar analytical assumptions as I make here. The principal strength of that account is that it analyses SE as the marker of the suppression of the highest-ranking argument in the semantic representation of the clause, a feature which anticausatives share with impersonal, passives and reflexives, and which is ultimately motivated by the alignment preferences of Italian.¹⁷ A shortcoming is that it does not factor in the constructional aspects of anticausativization. Further work is also needed on benefactive SE marking of ingestion and ‘get’ verbs (Bentley 2006: 153–154) and on non-alternating verbs of change of location and change of state (De Miguel & Fernández-Lagunilla 2000, Gonzáles-Vergara 2006, Jiménez-Fernández & Tubino-Blanco 2017), although the SE marking of these verbs is less prevalent in Italian than in Spanish (Italian (**si*) *cadde* vs. Spanish *se cayó* ‘s/he fell’).

This leads me to the issue of microvariation. Overall, the incidence of SE is higher in some Romance languages than in others (Heidinger 2015), and the cognates or translational counterparts of a verb that admits or requires SE in a Romance language do not necessarily belong in the same formal group in the sister languages. Such discrepancies are in part motivated by morpho-lexical features such as the presence of prefixes which mark derived causatives overtly

¹⁶I note that if alternating *bruciare* ‘burn’ is underspecified for cause, as I have assumed (Sections 3.3.2 and 5.3), the fact that it was found to anticausativize in a labile way (cf. 34) suggests that this construction can be accessed by verbs that have inchoative, as well as causative, LSs.

¹⁷Nichols et al. (2004) call *valence orientation* the typological parameter according to which a language tends to treat the transitive member of causative-intransitive pairs as basic and the intransitive as derived, or vice versa, or, alternatively, it adopts other strategies. My study corroborates their finding that the treatment of the transitive as basic correlates with accusative alignment.

and may result in anticausativization being the only option in the intransitive. A relevant example is the contrast between Italian *migliorare* ‘improve’, which is in the –SE group, and its French cognate *améliorer*, which exhibits the causative prefix *a-* and belongs to the +SE group.¹⁸ In other cases, however, no such morphological triggers are present, as with the cognate lexemes for ‘ferment’, which corpus analysis has revealed to be –SE verbs in Italian and French and ±SE verbs in Spanish, despite having the same relevant lexical-semantic properties in the three languages (Bentley 2023). The case of ‘ferment’ suggests that different constructional instructions are at work in the sister languages. Taking a modular approach, it is possible to address the puzzle of the microvariation in the distribution of SE in the sister languages, disentangling the common semantics-syntax mapping principles which underlie SE marking, at least in a diachronic sense (Cennamo 1995), from the language-specific morpho-lexical and constructional issues.

6 Conclusion

The existing accounts have made significant contributions to knowledge and understanding of the causative alternation, identifying its principal semantic underpinnings and the pragmatic inferences that arise from SE marking, and advancing sophisticated hypotheses on the syntax of the alternation. Yet, these accounts are not sufficiently modular, and, therefore, they make generalizations which conflate morphosyntax with the lexicon or factor out key morphosyntactic or lexical information. As a result, the predictions of these theories are at the same time too narrow, thus failing to capture the broader distribution of SE, and insufficiently constrained, thus missing key empirical facts about the behaviour of specific subclasses of verbs. I have argued that the causative alternation, and the distribution of SE, cannot be reduced to a single module of grammar or indeed a single principle, be that a facet of meaning or of syntactic structure or a pattern in the semantics-syntax interface.

A fundamental question is: what are the boundaries of the causative alternation and how do they vary cross-linguistically? The languages which rely on the derivation of causatives from inchoatives have not been within the scope of this study. Focusing on Italian, a language where the alternation is primarily anticausative (Haspelmath 1993), I have argued that the said boundaries are established in grammar through (i) the acquisition of inchoative and causative logical structures, which are stored in the lexicon alongside non-templatic facets of

¹⁸In a sample of 500 tokens collected by the author in the frTenTen17 corpus (Jakubíček et al. 2013) in 2021, 100% of the 32 non-passive intransitive attestations of French *améliorer* ‘improve’ were SE marked.

meaning, (ii) general semantics-syntax mapping principles, which are subject to alignment variation, and (iii) constructional instructions, which determine which subclasses of verbs can participate in the constructions that are relevant to the causative alternation in each individual language.

Therefore, the causative alternation, a lexically motivated grammatical pattern underpinned by broad semantics-syntax mapping principles, is a prime illustration of the parallel architecture of grammar. Taking a modular approach has allowed us to discern which –SE occurrences of alternating verbs are anticausative and which ones are inchoative, i.e., do not involve causer suppression in Italian. I have advanced a hypothesis on the lexical-semantic constraints on this contrast, which is unambiguously manifested in perfect auxiliary selection. I have uncovered the existence of two anticausativization constructions in Italian, both involving causer suppression, but differing in morphosyntactic and discourse-pragmatic terms. Drawing on existing claims on the pragmatic inferences arising from SE marking, I have claimed that these underpin some of the constructional features of overt anticausativization in Italian and, ultimately, the division of labour between the two constructions.

Appendix A Key terms

A glossary of key terms is provided here, with informal definitions that are compatible with the assumptions made, and the analyses proposed, in the article. Full bibliographical information is provided within the article.

Achievement This is a Vendlerian lexical-aspect class characterised by a single, punctual, change into a specific result state. Relevant diagnostics are discussed in the article, where I analyse Italian *rompere* ‘break’ as an achievement.

Accomplishment This is a Vendlerian lexical-aspect class characterised by non-punctual change into a specific result state. Relevant diagnostics are discussed in the article, where I analyse Italian *chiudere* ‘close’ and *aprire* ‘open’ as accomplishments.

Anticausativization In my analysis anticausativization is a strategy of argument realization which suppresses, i.e., fills silently, the causer position of a causative verb or construction. As a result of anticausativization, the causee figures as the sole argument of the verb in syntax. The Italian example *Il vaso si rompe* ‘the vase broke’, where *il vaso* ‘the vase’ is the causee, is the output of anticausativization.

Causative alternation A stem participates in the causative alternation when it can occur as a causative transitive verb or as an inchoative or anticausative intransitive verb. The Italian stem for ‘break’ participates in the causative alternation, occurring as causative (*Il sasso rompe la finestra* ‘The stone broke the window’) and as anticausative (*La finestra si rompe* ‘the window broke’). The term inchoative is defined below.

Constructional Schema Following Van Valin (2023), I take Constructional Schemas (CSs) to be sets of instructions storing the defining syntactic, morphological, semantic and discourse-pragmatic properties of the constructions of a given language. I refer to examples (58) and (60) for the abridged CSs of overt and labile anticausativization in Italian.

Direct causation Drawing on Wolff (2003), I call direct a type of causation where the relation between the causer and the causee is either unmediated or facilitated by optional interveners whose role is to enable the causer to achieve their goal. Events of ‘breaking’ are normally directly caused, in that any interveners in such events are optional enablers.

Degree achievement This is a Dowtyan lexical class characterised by non-punctual change into a result state which can only be determined in relation to a term of comparison or a context of use. Relevant diagnostics are discussed in the article, where Italian *sgonfiare* ‘deflate’ and *riscaldare* ‘heat’ are analysed as degree achievements.

Impersonal In the article the term impersonal names a structure in which the highest-ranking argument position in Logical Structure (see below) is suppressed, i.e., filled silently, and remains unexpressed in syntax. The suppression is marked by the morpheme SE. The unexpressed argument of SE impersonals is obligatorily human. An example is *Si cammina* ‘one walks’, which does not spell out the x argument of the Logical Structure *do*‘ (x, [walk‘ (x)]).

Inchoative The intransitive member of causative-intransitive pairs is called inchoative in relevant literature. The Italian example *Il paziente è guarito* ‘the patient has healed’ exhibits inchoative *guarire* ‘heal’, whose causative counterpart is found in *La cura ha guarito il paziente* ‘the treatment healed the patient’. In the article I draw a distinction between inchoative stems, which are monovalent lexical entries describing change of state, and anticausative structures, which are the output of anticausativization (see above).

Internal causation Internal causation, a notion introduced by Levin & Rappaport Hovav (1995), is the linguistic encoding of change that arises from an inherent property of the changing participant. In Bentley (2023) I argued that the propensity of specific types of participant to undergo specific types of change is the key facet of the meaning of the relevant stems ('blossom', 'germinate', 'rust', etc.).

Logical Structure This is Van Valin's (2005, 2023) formal representation of the meaning of a lexical entry, which elaborates Vendlerian and Dowtyan principles of lexical decomposition (e.g., the Logical Structure of 'walk' is *do*'(x, [walk' (x)])). The meaning of clauses and larger syntactic units is built compositionally from the Logical Structure of the predicator(s) contained in them.

Passive In the article the term passive mainly refers to an argument realization strategy whereby the highest-ranking argument in Logical Structure (see above) is SE-suppressed (see above) and the lower argument is realized as the controller of verb agreement. A relevant Italian example is *Si sono costruite quelle strade* 'Those roads were built/ One built those roads'. Unlike the SE anticausative, the SE passive is not lexically constrained. In a different passive structure, which is not the focus of this article, the verb bears passive morphology, and the higher argument is demoted in a by-phrase rather than being suppressed.

Reflexive In the article the term reflexive names a structure whereby the higher of two coreferential arguments is suppressed and the suppression is manifested by the reflexive clitic *SE* (e.g., *Maria si pettina* 'Many combs herself'). Alternatively, the lower of the two coreferential arguments is expressed by the tonic pronoun *sé* (*stess-*) (e.g., *Maria pettina sé* (*stessa*) 'Many combs herself').

Resultative I call resultative a lexical entry which lexicalizes a result state, i.e., a state that is not an inherent property but rather the outcome of change. An example is the lexical entry of *sbocciare* 'blossom', which is resultative because it describes a change of 'becoming blossomed'. A result state can also be a facet of the compositional meaning of a construction, in which case the construction is resultative.

Appendix B Verb frequencies

Table 5: Proportion of occurrence of each verb in each grammatical domain. Each verb was sampled 500 times. Column name abbreviations: Tr: Transitive; Pas: Passive; +SE: +SE Non-passive Intransitive; -SE (rel): -SE Non-passive Intransitive (relevant); -SE (irr): -SE Non-passive Intransitive (irrelevant); PC: Periphrastic Causative; PA: Participial Adjective

Verb (IT)	Verb (E)	Tr	Pas	+SE	-SE (rel)	-SE (irr)	PC	PA	Other
sparpagliare	scatter	0.180	0.022	0.168	0.000	0.006	0.002	0.610	0.012
sbriciolare	crumble	0.270	0.068	0.292	0.008	0.006	0.008	0.318	0.030
aprire	open	0.420	0.068	0.224	0.050	0.010	0.010	0.204	0.014
chiudere	close	0.472	0.080	0.152	0.058	0.102	0.006	0.118	0.012
rompere	break	0.530	0.038	0.188	0.010	0.104	0.006	0.088	0.036
cuocere	cook	0.266	0.172	0.018	0.048	0.168	0.134	0.150	0.044
bruciare	burn	0.476	0.126	0.058	0.218	0.014	0.010	0.052	0.046
congelare	freeze	0.312	0.130	0.046	0.032	0.028	0.002	0.412	0.038
sgonfiare	deflate	0.288	0.034	0.544	0.036	0.022	0.030	0.038	0.008
riscaldare	heat	0.422	0.152	0.114	0.012	0.034	0.014	0.230	0.022
asciugare	dry	0.380	0.058	0.198	0.098	0.062	0.170	0.012	0.022
arrugginire	rust	0.014	0.006	0.074	0.128	0.000	0.018	0.742	0.018
migliorare	improve	0.732	0.046	0.016	0.170	0.002	0.002	0.024	0.008
aumentare	increase	0.492	0.040	0.000	0.358	0.000	0.020	0.062	0.028
marcire	rot	0.008	0.008	0.000	0.628	0.000	0.196	0.054	0.106
sbocciare	blossom	0.006	0.000	0.000	0.700	0.000	0.080	0.146	0.068

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Chapter 11

Transitivity in Séliš-Ql'ispé

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Séliš-Ql'ispé, an Interior Salishan language spoken in northwestern Montana, has a verbal system that seems at first glance to distinguish transitive constructions from intransitive ones in a quite straightforward way: transitive verbs have a transitive suffix and a characteristic set of subject and object markers, while intransitive verbs have no transitive suffix or object markers and have a completely different set of subject markers. In addition, the two constructions differ systematically in their marking of adjunct (or argument) noun phrases. Initial appearances are deceiving, however. It turns out that morphologically intransitive verbs can take object noun phrases, and that certain transitive constructions, most notably monotransitive continuatives, lack part of the transitive morphology. The goal of this paper is to explore the morphosyntactic means by which different degrees of transitivity are signalled in Séliš-Ql'ispé, and to propose an analysis that pulls apparently disparate facts together in a unified way.

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1 Introduction

Not surprisingly, Salishan languages show both similarities and differences in their morphosyntactic patterns relating to transitivity. Many or most of these patterns have been described, of course, but as far as we know, comprehensive discussions of the patterns we focus on in this paper are still rather rare for a Southern Interior Salishan language.¹ There is some overlap between several of these analyses and ours, but none of them makes the same claims we do. Since Séliš-Ql'ispé transitivity differs from that of other Southern Interior languages in certain respects, a description of this system should be of interest to Salishanists.² More generally, the Séliš-Ql'ispé system is of potential interest to theoreticians concerned with types and degrees of transitivity, because of the wide variety of constructions – some of them rather unusual – in which transitivity plays a role. Our account is strictly synchronic and specific to Séliš-Ql'ispé; we have not carried out any systematic study of the diachronic sources of the current structures, or any systematic comparison with partially cognate structures in other Salishan languages.

The bulk of this paper consists of a description of nine relevant constructions (Sections 2.1–2.9): ordinary transitives; ditransitives; unsuffixed intransitives; intransitives with the ANTIPASSIVE suffix $-(\acute{e})m$ (often called “middle” in the Salishan literature); transitives with the BACKGROUNDED AGENT suffix $-(\acute{e})m$ (often analyzed as “passive” and/or “indefinite agent” in the literature); TRANSITIVE CONTINUATIVES in $-m$; DERIVED TRANSITIVES in $-mí(n)$; intransitives with lexical suffixes; and transitives detransitivized by the reflexive suffix $-cút$. These nine constructions do not exhaust the list of relevant patterns; we have not yet

¹See e.g. Mattina (1982), Kinkade (1981), Carlson (1980), Doak (1997) for descriptions of the morphology of transitive verbs in Colville-Okanagan, Moses-Columbia, Spokane, and Coeur d'Alene, respectively, and Kroeber (1999) for insightful comments on parts of the transitivity systems in various Salishan languages. Relevant analyses of parts of transitivity systems are found in e.g. L. Thomason (1994), Mattina (1994), Mattina (2004), Dilts (2006), Gerdtts & Hukari (1998), and Sobolak (2020).

²The language called Séliš-Ql'ispé today is primarily a merging of Bitterroot Salish (formerly known as Flathead), spoken by people whose homeland was the Bitterroot valley south of Missoula, MT, with Ql'ispé (formerly known as Upper Pend d'Oreille), as spoken by people whose homeland was the Jocko River area north of Missoula. Both tribes now live on the Flathead Reservation north of Missoula. Séliš-Ql'ispé belongs to a dialect complex that also includes Spokane and Kalispel; these dialects comprise a single language, but there is no language name that covers all three. The data in this paper comes from Thomason's field notes, from materials compiled by the Flathead Culture Committee (now renamed as the Séliš-Ql'ispé Culture Committee), and from the thousand-page 19th-century Jesuit dictionary of the language (Mengarini et al. 1877–1879).

explored all the constructions that have some connection with transitivity. We omit a few detransitivizing constructions, notably the reciprocal, because they behave basically like reflexive forms with respect to transitivity. We also omit discussion of the so-called “intransitive reflexives”. A more significant omission is the lack of any specific consideration of interactions between control and transitivity (see e.g. Thompson 1985; we have as yet too little information on control features in Séliš-Ql'ispé to comment on them here). Another major transitivity-related topic that is largely omitted from our account is the patterning of the various constructions in discourse. We will mention interactions between discourse and transitivity occasionally, but we have not yet studied enough textual material to draw many systematic conclusions in this domain.

After presenting the data, we will discuss ways in which the various constructions reflect differing degrees of transitivity, and we will offer preliminary suggestions for an overall treatment of these differences (Section 3). We adopt, with modifications, the common view of transitivity in which the prototypical transitive construction involves a completed transfer of action from a definite agent to a definite patient (see e.g. Hopper & Thompson 1980). Some modification of this view is necessary for Séliš-Ql'ispé because here the two main variables that correlate with transitivity alternations turn out to be ASPECT and FOCUS ON THE AGENT vs. FOCUS ON THE PATIENT. Definiteness per se is not as important a variable in Séliš-Ql'ispé as it is said to be in some other Salishan languages, though it does play a role in determining the use of two non-prototypical constructions, the antipassive and the backgrounded agent, and it plays a minor role in the marking of patient noun phrases in ordinary transitive sentences. As we will show in the following descriptions, the ordinary transitive represents the prototypical transitive type in Séliš-Ql'ispé, while other transitive-related forms deviate from the prototypical model in various ways. Although we will not explore them in any detail in this paper, the Séliš-Ql'ispé facts have interesting implications for theories of transitivity and for the concept of the morpheme.

Our primary goal is to understand the interactions between the morphology and the sentence-level syntax of the relevant constructions. A secondary goal, one that we can only sketch in this paper, is to establish the circumstances under which the different constructions are used. One significant departure from most previous analyses of these phenomena in Salishan languages is our proposal that three of the constructions contain a suffix $-(\acute{e})m$ which has the effect of reducing transitivity in a stem to which it is added – either reducing transitivity in (paradoxically) a morphologically intransitive bivalent stem (antipassive) or reducing transitivity in a morphologically transitive bivalent stem (backgrounded agent, continuative aspect). (See Section 2.4 for a brief explanation of valency

in Séliš-Ql'ispé.) That is, we will argue that, for Séliš-Ql'ispé, it is reasonable to treat all these occurrences of $-(\acute{e})m$ in transitive-related constructions in a unified way. The construction in which Séliš-Ql'ispé seems to differ most sharply from other Salishan languages is the transitive continuative; here our account diverges from previous analyses of this language, notably those of Kroeber (1999) and Vogt (1940), in that we analyze these forms as transitives, not intransitives (Section 2.7).

2 Nine relevant construction types

In its basic morphological patterns, Séliš-Ql'ispé appears to make a straightforward distinction between transitive and intransitive predicates.³ First- and second-person subjects of intransitive verbs are proclitic particles that appear at the left edge of the verb complex, and third-person intransitive verbs have no overt subject marking; by contrast, the morphological transitive apparatus appears at the right edge of the verb complex in the order $-TR-o-s$ – that is, first a transitive suffix, then an object marker, and finally the transitive subject.

There are three exceptions to this basic transitive pattern. First, the sole 1SG object marker is a proclitic. Second, all 1PL forms have a proclitic component qe/q^wo , which in transitive constructions occurs in conjunction with a 1PL suffix in the usual subject or object suffix position. And third, transitive continuative predicates have completely different sets of subject and object markers; these will be discussed in Section 2.6. Third-person objects, like third-person intransitive subjects, have no overt marking.⁴ Except for the 1PL proclitic, non-tr.cont predicates in the basic system are divided cleanly into transitive and intransitive forms according to their pronominal markers.

2.1 Ordinary transitive verbs

Ordinary transitive verbs, illustrated in examples (1–6), are aspectually noncontinuative. They consist of a transitive stem to which a transitive (+ control) suffix,

³In this paper we will use the terms “verb” and “predicate” interchangeably, and we will also talk about “nouns” and “noun phrases”. We use this terminology for convenience; we do not mean to take a firm position on the question of whether Séliš-Ql'ispé and other Salishan languages have a clear lexical distinction between nouns and verbs (see e.g. Kinkade 1983 and van Eijk & Hess 1986 for discussion of this issue).

⁴This pattern naturally leads some specialists to posit split ergativity in various Salishan languages. We do not follow their lead, but we will not address the question in this paper.

either *-nt* or *-st*, is added.⁵ All of the transitive stems in examples (1–6) are bare roots, with the exception of (6). Example (6) consists of a root *púk^w* ‘spill, pour round objects’ preceded by two prefixes and followed by a lexical suffix =*ús* ‘fire, face’.

Note, crucially, the marking of full-word agents and patients in (2) and (3): patients are marked optionally by the subordinator *lu*, and agents are marked obligatorily by the oblique particle *t*.⁶ Kroeber (1999: 52–53) observes that Colville-Okanagan, Kalispel [including *Séliš-Ql'ispé*], and Coeur d’Alene are unique in Salish in making this distinction between the case marking of transitive agent noun phrases (NPs) and that of patient NPs, and that this distinction is obligatory in Kalispel only. We have found no exceptions to the case marking of full-word agents of transitive verbs. We do have example sentences in which an indefinite patient NP is marked by *t* rather than by *lu*, but since most patients of transitive verbs, whether definite or indefinite, are marked instead by optional *lu*, we treat the *t*-marked patients as nondistinctive variants (and see Section 3 for some discussion of the implications of the *t* marking of indefinite patients). The important point about the case marking of NPs in simple transitive constructions is that the patient NP is most intimately linked to the verb, as shown by its lack of obligatory case marking; the agent NP, by contrast, must be set off from the verb complex by the oblique particle.⁷

⁵These two suffixes differ functionally in some Salishan languages, such that the former is non-causative and the latter causative. Semantically causative verbs usually have *-st* in *Séliš-Ql'ispé*, but some verbs with this suffix are not causative, and in fact we have not found a systematic functional difference between the two suffixes in the basic transitive system (although the data in Mengarini et al. (1877–1879) indicates that *-st* is used consistently for habitual actions). In accordance with the Salishan literature more generally, as in e.g. Mattina & Montler (1990: 23–24), we consider these two suffixes to be transitivizers.

⁶We will not consider in this paper the question of the status of full words other than the main predicate (typically the first word) in the *Séliš-Ql'ispé* sentence. In particular, we do not address the issue of adjunct vs. argument status for noun phrases that are translated in English as agents and patients. It is clear that some noninitial full words are adjuncts, and some of these adjuncts are regularly marked by optional *lu*. Moreover, the oblique marker *t* is attached to words other than agent noun phrases (NPs), e.g. time adverbials. These facts do not necessarily mean that the NPs under consideration here are *not* arguments of the verb; still, their syntactic behavior does resemble the behavior of full words that are certainly not arguments. In any case, the status of the “agent” and “patient” NPs is not crucial for our present purposes. For convenience, and without prejudice, we will refer to them simply as agents (or subjects) and patients (or objects).

⁷The grammatical terminology used in this paper is loosely based on that of Carlson (1972), with modifications as needed. Like Mattina (e.g. Mattina 1987) and other Salishanists, we distinguish grammatical suffixes from lexical suffixes by using different boundary symbols, a hyphen preceding a grammatical suffix and an equals sign preceding a lexical suffix – and similarly for the few lexical prefixes, e.g. *pu?*= ‘spouse’ in example (20).

- (1) Púlstx^w.
 púls-st-0-éx^w
 kill-TR-3.OBJ-2SG.TR.SBJ
 ‘you killed him.’
- (2) Čłpntés łu nłámqe t Čoní.
 čłp-nt-0-és łu nłámqe t Čoní
 hunt-TR-3.OBJ-3.TR.SBJ 2ndary black.bear OBL Johnny
 ‘Johnny hunted a/the black bear.’
- (3) K^wełntén łu nłámqe.
 k^weł-nt-0-én łu nłámqe
 bite-TR-3.OBJ-1SG.TR.SBJ 2ndary black.bear
 ‘I bit the black bear.’
- (4) Q^wo wíčtx^w.
 q^wo wíč-st-éx^w
 1SG.OBJ see-TR-2SG.TR.SBJ
 ‘You saw me.’
- (5) Wíčtmn.
 wíč-st-úm-én
 see-TR-2SG.OBJ-1SG.TR.SBJ
 ‘I saw you.’
- (6) Ełčpq^wósntx^w.
 eł-č-púk^w=ús-nt-0-éx^w
 back/again-LOC:on-pour.round.objects=fire-TR-3.OBJ-2SG.TR.SBJ
 ‘You pour(ed) them on the fire again.’

2.2 Ditransitives

Examples (7–10) illustrate the second relevant construction type, noncontinuative ditransitive verbs. These differ from simple transitives in that they have a RELATIONAL (+ control) suffix, either *-łt* or *-šít*, in place of a transitive (+ control) suffix *-nt* or *-st*. The two relational suffixes differ semantically – *-šít* is a benefactive suffix, as in (7–9) (assuming that the recipient wanted a cat!), while *-łt* has a neutral or negative connotation, as in (10) (see Carlson 1980 for discussion) – but they are often used interchangeably. Examples (7–10) are all formed to bare roots, *x^wíc* ‘give’ and *máw* ‘break, destroy’.

It is rare for all three NPs to appear together in a ditransitive construction, but when they do appear, as in (7), *tu* optionally marks the recipient of the action and *t* obligatorily marks the patient, the “direct” object.⁸ The agent NP is obligatorily case-marked as an oblique, either by the simple oblique marker *t*, as in (7), or by the preposition *tl'* ‘from’. The general pattern resembles that of the monotransitives: one NP, in this case the recipient, is closely tied to the verb and has no obligatory overt case marking; the other NPs are obligatorily set off by oblique markers. Predictably, when the recipient is expressed by a pronominal (as in 8–10), the usual object pronominal form is used. There is, moreover, some variation in the case marking of the patient NP in ditransitive constructions when the recipient is a pronoun rather than a full-word NP: in this case the patient NP sometimes appears with zero case marking, as in (10), ‘He wrecked my car’ (but this does not seem to be possible with the verb ‘give’). The general rule still holds – at most one full-word NP is nonoblique, i.e. lacking overt case marking – but the zero-marked position may be filled by a full-word patient NP when there is no full-word recipient NP. Note that the verb codes directly for only two pronominal arguments; the third, usually the recipient of the action, is indicated only by the relational suffix.

There may be some dialect difference between *Séliš-Ql'ispé* and Spokane in the case marking of patient NPs in ditransitive constructions: according to Carlson (1980: 24), in Spokane the marking described here is valid only for ditransitives with the relational suffix *-ší*; for ditransitives with relational *-ł*, Spokane marks the recipient NP with a preposition and the patient (“direct object”) with optional *tu*. In *Séliš-Ql'ispé*, the normal case marking is the same with both relational suffixes.

- (7) X^wíc'šts *tu* Malí t pús tl' Čoní.
 x^wíc'-šít-0-és *tu* Malí t pús tl' Čoní
 give-REL.TR-3.OBJ-3.TR.SBJ 2ndary Mary OBL cat from Johnny
 ‘Johnny gave Mary a cat.’

⁸It is in a sense misleading to specify *tu* as marking one object in a transitive construction, because this particle also occurs sometimes before the oblique marker *t*, as well as before certain subordinate clauses and other adjuncts. But the particle is especially frequent with an object NP, and in any case the point is that the main object of a verb is normally the only NP in a transitive construction that may be preceded by this particle alone, whether the main object is the so-called direct object of a monotransitive verb or the so-called indirect object of a ditransitive verb.

- (13) ʔocqéʔ (ʔu) Čoní.
 0 ʔocqé ʔ(ʔu) Čoní
 3.INTR.SBJ go.out 2ndary Johnny
 ‘Johnny went out.’

2.4 Intransitives with ANTIPASSIVE *-(é)m*

So far the constructions we have discussed are morphosyntactically and semantically straightforward: the morphology and syntax of the transitive constructions reflect prototypical transitive semantics, with completed transfer of action from a definite agent to a (usually) definite patient, and the plain intransitives lack any such transfer. (The semantic patterns are not, of course, completely transparent throughout the language; as in all languages, the general semantic categories leak.) With the antipassive construction, illustrated in examples (14–17), we see more complicated relations between morphosyntax and semantics. The form we call antipassive (a term used by, among others, Kroeber (1999: 31), Darnell (1990), Gerdtz (1993), and, with reservations, Thompson & Thompson (1992: 102)) is often called “middle” in the Salishan literature, and Newman (1980: 158) posits a Proto-Salish suffix **-m* ‘middle’. If this suffix has a genuinely middle function in some other Salishan languages, with action that reflects back on and/or affects the verb’s subject, then *Séliš-Ql'ispé* has diverged from the rest of the family in this respect. The primary grammatical function of the suffix in *Séliš-Ql'ispé* is to force an active interpretation of an intransitive bivalent verb. Perhaps its most notable function in discourse is to highlight a switch from one agent to another (see the discussion in Section 3 below). The antipassive also serves as the usual citation form for bivalent verbs. For example, if one asks an elder what the word for ‘scrape’ is, the answer will most likely be an antipassive *ʔáx^wm* ‘he scrapes something’. It might perhaps be *ʔáx^wis* (= *ʔáx^w-nt-és*) ‘he scrapes it’, but it will never be a suffixless *esʔáx^w* ‘it is scraped’.

At this point we need to introduce the topic of valency in *Séliš-Ql'ispé* because of its crucial interactions with transitivity, especially in this construction. All roots in this language are intransitive, but they fall into three valency classes. MONOVALENT verbs, e.g. *x^wúy* ‘go’, have one lexically specified argument – an actor, an experiencer, or some other semantic category, but not a patient; and BIVALENT verbs, e.g. *wíč* ‘see, find’, have two lexically specified arguments, an agent and a patient. Monovalent verbs are agent-oriented; bivalent verbs are patient-oriented. The third and smallest root class, AMBI-VALENT, comprises agent-oriented verbs with two lexically specified arguments, agent and patient; an example is *ʔíln* ‘eat’. Ambi-valent verbs do not differ significantly from bivalent

verbs with respect to transitivity interactions, so we will ignore them in the rest of this discussion. The largest root class by far, at least for action verbs, is the bivalent class.

A monovalent verb that occurs alone or with just an aspect affix has an active (or at least a non-passive) reading, e.g. *čn x^wúy* ‘I go, I went’ and *čn x^wéy-t* ‘I’m lazy, I have no energy’. By contrast, a bivalent verb that occurs with just an aspect affix has a stative passive reading, e.g. *čn es-wič* ‘I am seen’. This is the most salient diagnostic for identifying a root as monovalent or bivalent; see Thomason et al. (1994) for other diagnostics and further discussion, including reasons for not classifying bivalent roots as unaccusatives as some other Salishanists have done (e.g. Gerds 1991).

The Séliš-Ql’ispé antipassive fits the standard definition of antipassive by promoting the subject of a bivalent verb to agent and, in effect, backgrounding or eliminating the lexically specified patient that is part of a bivalent verb’s argument structure. The stem to which the antipassive suffix is added may be either a simple root, as in (14–16), or a root with one or more affixes, as in (17).

It is crucial to our analysis that stems with the antipassive suffix – unlike intransitive stems with no *-(é)m* – have two arguments in their syntactic frame. That is, the antipassive suffix adds a second syntactic argument, in spite of the fact that it is morphologically intransitive. In addition to the syntactic behavior of antipassives (see below), further evidence for this interpretation lies in the fact that an antipassive added directly to a monovalent stem (that is, with no intervening derived transitive suffix *-mí(n)*) produces a bivalent causative stem, with a second agent. So, for instance, *k^w x^wúym* means ‘you cause someone to go’. Compare the corresponding morphological transitive, also with a causative reading and without a DER.TRANS suffix, *x^wúy-nt-x^w* ‘you cause her to go’. Compare also the same verb root in a non-causative transitive bivalent construction with the DER.TRANS suffix, e.g. *č-x^wúy-m-nt-x^w* ‘you visited him’ (with the locative prefix *č-* ‘to’).

Bivalent verbs appear most frequently in discourse in straightforward transitive constructions, as in examples (1–6) above. Antipassives, by contrast, are bivalent but morphologically intransitive. Accordingly, the subject pronominals for antipassives are the usual intransitive proclitic particles, and full-word subject NPs are marked by optional *tu*, as in example (16) (for which the free translation is ‘I skinned it and my wife sliced it’, ‘it’ being deer meat). But since, unlike monovalent intransitives, these are semantically transitive constructions, they also have notional objects – usually indefinite but sometimes definite, as in example (16). When the object is overtly expressed, as in (14) and (15), it is marked

obligatorily by the oblique proclitic *t*. Antipassives thus have the opposite marking from ordinary transitive constructions with two arguments: in antipassives a subject NP is marked by optional *lu* and an object NP by obligatory *t*, while in transitive constructions a subject NP is marked by obligatory *t* and an object NP by optional *lu*.⁹ Note that in (17) the oblique marker precedes an instrument NP, not an object NP; this common type of adjunct phrase underlines our point that the oblique marker indicates a phrase that is less closely linked to the verb, and thus arguably less important, than the “main” NP.

- (14) Čn k^weʔém t nłámqe.
 čn k^weʔ-(é)m t nłámqe
 1SG.INTR.SBJ bite-ANTIP OBL black.bear
 ‘I bit a black bear.’
- (15) Ha k^w wíčm t smxé?
 ha k^w wíč-(é)m t smxé
 Q 2SG.INTR.SBJ see-ANTIP OBL grizzly.bear
 ‘Did you see a grizzly bear?’
- (16) X^wcntén u t’élm lu innóx^wnX^w.
 x^wíc-nt-0-én u t’él-(é)m lu
 skin-TR-3.OBJ-1SG.TRANS.SUBJ and slice-ANTIP 2ndary
 in-nóX^w-nX^w
 1SG.POSS-wife-REDUP
 ‘I skinned it and my wife sliced it.’
- (17) Mk^w čx^wéycpm t anłn’í.
 m k^w č-x^wéyc-p-(é)m t an-l-n’íc’
 FUT 2SG.INTR.SBJ LOC:to-cut.off.hair-INCH-ANTIP OBL 2SG.POSS-DIM-cut
 ‘You’ll cut off the hair with your knife.’

Although the antipassive is clearly an intransitive construction, its two-argument syntactic frame and its ability to include a syntactic patient as well as an agent places it on the transitivity gradient: it is less transitive than a prototypical transitive construction (as in Section 2.1) semantically because it typically has an indefinite patient, and in any case its agent is the main focus. As we will argue in Section 3, this transitivity-reducing function unites the antipassive suffix with the backgrounded agent suffix (Section 2.5) and the transitive continuative suffix (Section 2.6).

⁹Formally marked antipassives are not the only verbs that participate in this pattern; ambi-valent stems also do so. An example is the ambi-valent verb *čłíp* ‘hunt’, as in intransitive *čn čłíp t nłámqe* vs. transitive *čłpntén lu nłámqe*, both meaning ‘I hunt(ed) black bear’.

2.5 Backgrounded agent constructions

The next construction in our list is the one typically characterized in the Salishan literature as a passive or indefinite-agent construction. In many or most other Salishan languages this characterization is accurate (see e.g. Kroeber 1999: 25–28 for discussion and for a characterization of this construction as Agent Demotion), but the cognate construction in Séliš-Ql'ispé is clearly active and transitive, and quite often the agent is definite (though indefinite agents are much more common with this construction). Morphosyntactically, the construction differs from ordinary transitives only in that the backgrounded agent suffix $-(\acute{e})m$ (or its allomorph $-t$; see below) replaces the usual transitive subject suffix. That is, the suffix $-(\acute{e})m$ is the subject suffix, and it is always preceded by a transitive suffix – $-nt$, $-st$, or relational (ditransitive) $-tt$ or $-šit$.¹⁰ The case marking of subject and object NPs, as in (18), (19), (21), and (22), is identical to that of any other transitive sentence, with the object optionally marked by lu and the subject obligatorily marked by t . (Example (18) means ‘One-Night told Qeyqeyši’, not vice versa. Zero marking of the patient in (21) is permitted because the recipient, the “indirect object”, is pronominal.)

No overt object suffixes occur between the transitive suffix and the backgrounded agent suffix allomorph $-(\acute{e})m$. This means that only the 1SG object proclitic k^wu and a zero-marked third-person object can occur with this allomorph. However, these forms are functionally identical to and in complementary distribution with backgrounded agent forms with the suffix allomorph $-t$, which does permit a preceding overt object marker; examples are in (22) and (23). That is, the forms with $-t$ occur always and only with 1PL and 2nd-person objects. We therefore treat this $-t$ as an allomorph of the backgrounded agent suffix, an analysis also found elsewhere in the literature (see e.g. Kroeber 1999: 25–28, with reference to Interior Salish generally, and Thompson & Thompson 1992: 63, with specific reference to Thompson; Kroeber considers the construction to be a true passive, while Thompson & Thompson treat it as an indefinite-agent construction).

¹⁰But see Section 2.6 below: there is some evidence that the transitive continuative suffix $-m$ is sometimes followed by an unstressed backgrounded agent suffix $-(\acute{e})m$, and that the two contiguous m suffixes coalesce phonologically into a single [m]. This hypothesized coalescence distinguishes this pair of m suffixes from the suffix sequence unstressed $-m(in)$ ‘derived transitive’ + m ‘transitive continuative’, in which both m ’s are always pronounced, either as a long [m:] or (more often) as [məm].

- (18) Cúntm Qeyqeyší t Nk^wusk^wk^wʔé, ...
 cún-nt-0-ém Qeyqeyší t nk^wúʔ-s-k^w-k^wʔé(c)
 say-TR-3.OBJ-BACKGRND.AG Qeyqeyší OBL one-NOM-REDUP-night
 ‘One-Night told Qeyqeyši, ...’
- (19) K^wu k^weʔntém t smxé.
 k^wu k^weʔ-nt-ém t smxé
 1SG.OBJ bite-TR-BACKGRND.AG OBL grizzly.bear
 ‘The grizzly bear bit me.’
- (20) Espuʔpúlstm.
 es-puʔ=púls-st-0-ém
 ASPECT-spouse=kill-TR-3.OBJ-BACKGRND.AG
 ‘Her husband got killed.’ (= ‘Someone killed her spouse’)
- (21) K^wu púlłtm isk^wisk^ws.
 k^wu púls-łt-0-ém in-sk^wisk^ws
 1SG.OBJ kill-REL.TR-3.OBJ-BACKGRND.AG 1SG.POSS-chicken
 ‘My chickens got killed.’ (= ‘Someone killed me my chickens.’)
- (22) Qe nčnčičłłt t sčq’iq^wé.
 qe n-čic(n)=ičn-łul-l-t t s-č-q’i-q^wáy
 1PL LOC:in-arrive=back-TR- OBL NOM-LOC:to-REDUP:PL-black
 1PL.OBJ-BACKGRND.AG
 ‘The blackfeet caught up with us.’¹¹
- (23) N’em łc’ncít.
 n’em łc’-nt-sí-t
 FUT whip-TR-2SG.OBJ-BACKGRND.AG
 ‘You’ll be whipped.’

Although this construction is an ordinary active transitive in *Séliš-Ql'ispé*, it does have one prototypical functional characteristic of passives (see e.g. Shibatani 1985): as our label suggests, it indicates backgrounding of the agent. This is not a new observation; Thompson & Thompson (1992: 58), for instance, interpret the Thompson cognate construction similarly, remarking that the indefinite-subject forms (as they analyze them) “serve to shift focus from the transitive subject to the object”. At least one discourse function of the *Séliš-Ql'ispé* construction appears to be identical to that of Moses-Columbia, as described in Kinkade

¹¹The transitive suffix *-łul* in this sentence is an allomorph of the standard transitive suffixes, occurring always and only with a 1PL or 2PL object.

(1989). Kinkade argues that the construction serves to track participants, being used to indicate a less prominent agent throughout a discourse. The Qeyqeyši story from which example (18) is taken illustrates this feature very neatly. The overall discourse environment is story-telling about a prominent tribal member named Qeyqeyši, specifically about his wild younger days when he and his friend One-Night repeatedly got into trouble. The particular story in which (18) occurs follows one in which Qeyqeyši himself is the major character; but in this later story, One-Night is the main instigator of the prank. In spite of One-Night's greater prominence in this context, however, transitive verbs referring to his actions consistently have the backgrounded agent suffix throughout this rather lengthy story. The reason surely is that Qeyqeyši himself is the primary character in the overall discourse environment, so that One-Night's agent role is consistently downplayed by means of the backgrounded agent construction. Although this story sequence is an especially clear example of the participant-tracking function described by Kinkade, the same phenomenon recurs in Séliš-Ql'ispé texts. The point that needs to be underlined here is that there is nothing indefinite about One-Night. It is of course true that indefinite agents are typically less prominent than other participants in discourse, e.g. when the patient is 1st or 2nd person (19, 22) or when no particular agent is specified (20, 21); but the common factor in these (and other) examples is backgrounding of the agent, not indefiniteness.

The backgrounded agent construction, like the antipassive and the transitive continuative construction, falls below a prototypical transitive construction on a gradient scale of transitivity, thanks to its typically (though not universally) indefinite agent. We will return to this topic in Section 3.

2.6 Transitive continuatives

The transitive continuative construction is the most interesting of all the Séliš-Ql'ispé transitivity-related constructions, thanks to the complications it presents. We will describe and illustrate the construction before discussing the complications.

The transitive continuative suffix *-m* does not co-occur with the transitive apparatus in monotransitive forms (e.g. examples 24–28). Instead, it occurs after a bivalent stem – i.e. after a bivalent root (24, 28) or a bivalent stem produced by the derived transitive suffix *-mí(n)* (25–27) – and it is never preceded (or followed) by an object suffix or an agent suffix. A transitive continuative verb is always preceded by a prefix that varies between the shapes *es-* and *s-*, and this variation is problematic. Treating this prefix as basically *es-* would mean that the prefix is an 'actual' aspect marker and would make transitive continuatives parallel to

the regular intransitive continuative monovalent form *es-STEM-mí*, as in e.g. *es-lap-mí* ‘s/he’s traveling by boat’¹² – an appealing symmetry. But analyzing the basic form of the prefix as the nominalizer *s-* is also appealing because, as we will see, the subjects of transitive continuative verbs are expressed by possessive affixes, which (elsewhere) can only be added to nominal stems. If the basic form of the prefix is *s-*, however, it is difficult to account for the variation phonologically: the *s-* variant occurs most often after a particle or full word ending in a vowel, and a regular rule; deletes a word-initial vowel in this context but there is no phonological rule that inserts a prothetic vowel *e* before a word-initial *s*. As others have pointed out (notably Kroeber 1999), there has been some conflation of these two prefixes in *Séliš-Ql'ispé*, and this partial conflation might have contributed to the relatively recent development of the construction in its current form. We provisionally analyze the basic form of the prefix as *es-* and consider it to have properties of both the ‘actual’ aspect prefix and the nominalizer.

The agent of a transitive continuative verb is expressed by a possessive affix – a prefix (1SG, 2SG), a preposed particle (1PL), or a suffix added after the TR.CONT suffix *-m* (2PL, 3). The patient is expressed in two different ways: either it is a normal object marker (1SG) or it is an intransitive subject particle (2SG, 2PL). In *Séliš-Ql'ispé*, 1PL and third-person patients provide no evidence for the “basic” marking of notional patients in this construction, because third-person objects and third-person intransitive subjects are all zero-marked, and the preposed part of the 1PL object is invariant *qe* and thus identical to the 1PL intransitive subject particle.

In ditransitive continuative constructions the transitive suffix does appear, specifically a relational transitive suffix *-t* or *-šít*, which immediately precedes the TR.CONT suffix (example 29). Otherwise the ditransitive forms are morphologically identical to the monotransitive forms.

Syntactically, the transitive continuative is identical to an ordinary transitive construction: subject NPs are obligatorily marked by oblique *t* (26), (28) and object NPs are optionally marked by *tu* (24), (27–29). (In (29), the fact that the recipient of the action is a pronominal is what allows the patient NP to receive optional *tu* marking.)

- (24) *Iesλ'eʔém ululím.*
 in-es-λ'eʔ-m ululím
 1SG.POSS-ACTUAL/NOM-look.for-TR.CONT money
 ‘I’m looking for money.’

¹²In spite of their shared /m/ segments, the intransitive continuative suffix and the transitive continuative suffix are not morphologically related either synchronically or diachronically. The parallelism consists of the aspect prefix combined with a continuative suffix.

- (25) K^wu esáy' mtmms.
 k^wu es-ŷáy'm-t-mí(n)-m-s
 1SG.OBJ ACTUAL/NOM-angry-STATV-DER.TR-TR.CONT-3.POSS
 'He's mad at me.'
- (26) P esáy' mtmms t Čoní.
 p es-ŷáy'm-t-mí(n)-m-s t Čoní
 2PL.INTR.SBJ ACTUAL/NOM-angry-STATV- OBL Johnny
 DER.TR-TR.CONT-3.POSS
 'Johnny's mad at you guys.'
- (27) Iesŷáy' mtmm łu Čoní.
 in-es-ŷáy'm-t-mí(n)-m łu Čoní
 1SG.POSS-ACTUAL/NOM-angry-STATV-DER.TR-TR.CONT 2ndary Johnny
 'I'm mad at Johnny.'
- (28) Esx^wépms łu síc'm t isq^wsé?
 es-x^wép-m-s łu síc'm t in-s-q^wsé?
 ACTUAL/NOM-spread- 2ndary blanket OBL 1SG.POSS-NOM-son
 TR.CONT-3.POSS
 'My son is spreading the blanket.'
- (29) K^w iesčéx^w łu tm łu asíc'm.
 k^w in-es-č'éx^w-t-m łu
 2SG.INTR.SBJ 1SG.POSS-ACTUAL/NOM-dry-REL.TR-TR.CONT 2ndary
 an-síc'm
 2SG.POSS-blanket
 'I'm drying your blanket for you.'

As mentioned above, our analysis of this continuative construction as transitive differs from previous analyses, in particular those of Kroeber (1991) and Vogt (1940), who treat the construction as intransitive. In later work Kroeber distinguishes the history of the construction (definitely nominalized and intransitive) from its synchronic status in *Séliš-Ql'ispé*, which may indeed be transitive (Kroeber 1999: 357). According to Kroeber, a transitive verb “is one that contains a Transitive or Ditransitive suffix, or at least inflects with Object pronominals. All other predicates are intransitive” (Kroeber 1991: 29). This definition straightforwardly excludes monotransitive continuative constructions from the transitive category; however, as we will try to show, the definition is too restrictive, in part because it does not take relevant syntactic patterns into account.

The construction has two properties that suggest intransitivity. First, and most obviously, it lacks the transitive suffix in the monotransitive form; and second,

the use of the 2SG and 2PL intransitive subject proclitics for second-person notional patients makes the construction look intransitive. A form like k^w *iswičm* (k^w *in-es-wič-m*, lit. 2SG.INTR.SBJ 1SG.POSS-ACTUAL/NOM-see-TR.CONT) would be glossed by Vogt and Kroeber as ‘you are my seeing’, whereas for us the translation is literally as well as freely ‘I am seeing you’.

The construction has two properties that are compatible with either a transitive or an intransitive analysis: the ambiguity in the marking of 1PL and third-person notional patients, already mentioned above, and the optional *tu* case marking on the notional object NP, as in (24) and (27). The sentences in (24) and (27) could be glossed either as ordinary transitives, ‘I’m looking for money’ and ‘I’m mad at Johnny’, respectively, or literally in the Vogt/Kroeber style, ‘money is my looking-for’ and ‘Johnny is my being mad at’.

However, the construction has four properties that make it look transitive. First, the 1SG object proclitic appears where the notional object is ‘me’. Second, a full-word agent NP is obligatorily marked by oblique *t*, as expected in a transitive but emphatically not in an intransitive sentence; this marking in turn shows that the apparently ambiguous optional *tu* marking on the other possible full-word NP must indicate the object, not an intransitive subject, because notional full-word subject NPs are invariably marked by *t* in this construction. Third, as noted above, the transitive suffix appears obligatorily in two-goal transitive continuative forms (e.g. (29)). And fourth, given the crosslinguistic links between possessive and agentive marking, the expression of the notional subject by possessive pronominals suggests that they are, indeed, agents (compare, for instance, English *I wrecked his car* and *my wrecking of his car*). This property is suggestive, but it cannot be considered diagnostic for the analysis of any particular language. A possibly relevant fifth property is the fact that the transitive continuative suffix *-m* occurs immediately after the derived transitive suffix *-mí(n)*, which otherwise precedes only a transitive suffix. (However, this property might perhaps be dismissed on the ground that the co-occurrence of these two suffixes could mean simply that what we’re calling the transitive continuative suffix has a detransitivizing effect, an analysis that would fit with the Vogt/Kroeber interpretation.)

The two intransitive-like properties, the absence of a transitive suffix in mono-transitive continuative forms and the use of 2nd-person intransitive subject proclitics, are balanced by two of the transitive-like properties, the presence of a transitive suffix in ditransitive continuative forms and the use of the 1SG object proclitic. The crosslinguistic tendency toward linking of transitive agents and possessives does not provide solid evidence for our analysis. That leaves us with one property which, in our view, argues strongly for a transitive analysis, namely, the case marking of full-word subject and object NPs. As we have seen

in Sections 2.1–2.5 (and will see below in Sections 2.7–2.9), this case marking is consistent throughout the language in identifying subject NPs and object NPs in both transitive and intransitive constructions. If the transitive continuative construction is not to be viewed as transitive, there is an inconsistency in case marking NPs that has no explanation.

By contrast, we do have an explanation for at least one of the two intransitive-like properties of this construction – the use of 2SG and 2PL intransitive proclitic pronominals to indicate the notional patient. Since, in monotransitive continuative forms, there is no transitive suffix, there is nothing to attach an object suffix to. In fact, the TR.CONT suffix replaces the entire transitive apparatus, so there is also no agent suffix, which must follow an object suffix in a normal transitive form. Obviously, then, patients must be expressed by some other means. This presents no problem for the 1SG object, which is a proclitic already, or for a third-person object, which has no overt marking, or for a 1PL object, which in ordinary transitive forms has both proclitic and suffixed components (so that the proclitic can take over the entire function). But how are 2nd-person objects to be expressed, given that the usual suffix position is not available? There are three other sets of person markers: transitive subject suffixes, possessive affixes, and intransitive subject proclitics. The transitive subject suffixes are unavailable for the morphological reason just given, even aside from the poor notional fit. The possessive affixes are unavailable because that set is already in use for the subject of the verb. This leaves only the intransitive subject proclitics, if a 2nd-person marker is to be used at all; and so that is what we find. Notice, moreover, that an analogous explanation will not account for the use of the 1SG object proclitic if the construction is viewed as intransitive: since both the 1SG object and the 1SG intransitive subject are proclitics, both are available – in contrast to the second person, where only the intransitive subject particles can be pressed into service as object markers in this construction.¹³

The other intransitive-like property of the transitive continuative, the lack of a transitive suffix in monotransitive forms, is what it appears to be: a signal that the

¹³Tony Mattina (p.c. 1992) has suggested a different analysis of the transitive continuative forms, as a ‘genitive’ construction. He points out that in *Séliš-Ql’ispé*, as in *Colville-Okanagan*, there are constructions like (*Séliš-Ql’ispé*) *k^w inxménč* ‘I like you’ and *k^wu anxménč* ‘you like me’, with pronominal marking identical to that of the transitive continuative forms – possessive affix for notional agent, 2SG proclitic intransitive subject vs. 1SG object for notional object – but with no actual/nominalizer prefix and no *-m* suffix. Vogt (1940: 32) also comments on links between transitive continuative verbs and possessed nouns. Exploring these connections is beyond the scope of the present paper, but they obviously must be considered in a complete analysis of the phenomena. We do not believe, however, that they will require a change in our analysis of the transitive continuative construction.

- (33) Wicínmłts ask^wísk^ws.
 wíʔ=cín-mí(n)-łt-0-és
 finish=mouth-DER.TR-REL.TR-3.OBJ-3.TR.SBJ
 an-s-k^wís-k^ws
 2SG.POSS-NOM-REDUP-chicken
 ‘He ate up your chickens.’
- (34) K^w yaʔmím.
 k^w yaʔ-mí(n)-ém
 2SG.INTR.SBJ gather-DER.TR-ANTIP
 ‘You gathered rocks.’

This suffix has been analyzed in various ways in the literature. It is not clear to us which, if any, of these interpretations are incompatible with ours; the apparent divergence may be due in large part to nonsubstantive terminological differences. In addition, of course, the suffix may function in less transparent ways in other Salishan languages. We will mention a few representative analyses here, but will not attempt to sort out the differences in any detail. Vogt appears to analyze the suffix as a transitivizer (Vogt 1940: 59–60), though his analysis of it is complicated (and made somewhat unclear) by his treatment of Kalispel transitive continuative forms as intransitives (see our example (25) above for a typical co-occurrence of the derived transitive and transitive continuative suffixes). Kinkade treats the cognate Moses-Columbia suffix as an intransitive suffix, specifically the middle suffix *-m*; the resulting stem is then transitivized, in his analysis, by the addition of the causative suffix (Kinkade 1981: 195). Gerdts’ (1993) analysis of the analogous construction in Halkomelem looks very similar to Kinkade’s, except that his middle category is her antipassive (see e.g. her example (45)). The Kinkade/Gerdts approach does not at first glance seem well suited to the Séliš-Ql’ispé facts. The Séliš-Ql’ispé transitive suffixes, including the so-called causative *-st* as well as *-nt* and the relational ditransitive suffixes, are normally added to stems that are already bivalent; bivalent roots are lexically specified, while lexically monovalent roots, together with stems that are detransitivized by lexical or other detransitivizing suffixes, normally appear with the transitive apparatus only after the derived transitive suffix *-mí(n)* has been added. (There is also no obvious preference for *-st* over *-nt* after this suffix in Séliš-Ql’ispé.)

This generalization requires a caveat, however, because there is evidence in Séliš-Ql’ispé that the transitive suffixes can indeed add a syntactic argument directly to a monovalent verb stem. As we have seen, the derived transitive suffix followed by a transitive suffix increases the valency of the stem by adding

a patient to its argument structure. But a transitive suffix added directly to a monovalent stem also increases the valency of the stem, in this case by adding a second agent – that is, it produces a causative stem, as we saw in Section 2.4. As a reminder of that discussion, compare examples (35–37):

- (35) *čn x^wúy*
čn x^wúy
 1SG.INTR.SBJ go
 'I go'
- (36) *čx^wúymn*
č-x^wúy-mí(n)-nt-én
 LOC:to-go-DER.TR-TR-1SG.TR.SBJ
 'I visit her' (lit. 'I go to her')
- (37) *x^wúyn*
x^wúy-nt-én
 go-TR-1SG.TR.SBJ
 'I make him go'

Example (35) is a plain intransitive monovalent verb. In (36) a derived transitive suffix, and thus a patient, has been added to the verb's argument structure, producing a bivalent stem; and the further addition of the transitive suffix *-nt* forms a verb with two syntactic arguments, an agent and a patient. Example (37) contrasts with (36) formally in that (37) lacks the derived transitive suffix; instead, the addition of the transitive suffix forms a causative verb by adding a second syntactic agent/actor to the verb. Moreover, as we also saw in Section 2.4, a form *čn xwúym* 'I make someone go' is also possible: in this case the antipassive suffix *-ém* also adds a second agent to the verb's argument structure and thus produces a bivalent verb. Both this antipassive construction and transitive constructions like (37) are rare in *Séliš-Ql'ispé* discourse, unlike derived transitive constructions, which are common.

Note, finally, that the derived transitive suffix may appear either after a lexical suffix, thus increasing the valency of a monovalent stem (e.g. example 33), or before a lexical suffix, in which case the potentially transitive stem formed by this suffix loses its syntactic patient (e.g. example (39) below).

2.8 Transitive-prone stems detransitized by lexical suffixes

The remaining two constructions that we want to illustrate are two types of verbs in which stems that are usually followed by transitive suffixes or the antipassive

are detransitivized. This section concerns the effect of certain lexical suffixes, as in examples (38–40). These suffixes may be added to bivalent roots, as in (38), or to stems that have had their valency increased by the addition of the derived transitive suffix, as in (39) and (40). In other words, a lexical suffix is added to the stem instead of a more usual transitive suffix. There are no morphosyntactic problems here: the derived stems take intransitive subject particles, as expected in an intransitive construction; a subject NP is marked by optional *lu* (40); and an object NP is marked by obligatory *t* (38–39).

- (38) K^w plsqé t šmén’
 k^w púls=sqé t šmén’
 2SG.INTR.SBJ kill=person OBL enemy
 ‘You killed an enemy.’ (lit. ‘You person-killed an enemy.’)
- (39) Ntx^wmsqá t xλ’cís.
 n-tóx^w-mí(n)=sqá t xλ’cín-s
 LOC:in-straight-DER.TR=domestic.animal OBL horse-3.POSS
 ‘He turned his horse around.’
- (40) Čx^wimsqé lu Malí.
 č-x^wúy-mí(n)=sqé lu Malí
 LOC:to-go-DER.TR=person 2ndary Mary
 ‘Mary visited someone.’

These constructions resemble antipassives syntactically in that the lexical suffix does not co-occur directly with transitive suffixes; instead, it is added either to a lexically bivalent root or to a derived bivalent stem. But where the antipassive is formed by a semantically empty suffix *-ém*, the constructions of interest here are formed by a lexical suffix with (often) concrete semantic content. A more significant difference between the two construction types is that a stem modified by a lexical suffix may become transitive-ready again by the addition of the derived transitive suffix, as in example (33) above. As we have seen, this is not possible with an antipassive.

A common proposal in the Salishan literature is that verbs like those in (38–40) contain an incorporated noun – that is, that the lexical suffixes are in fact incorporated noun stems. Such an analysis would of course account for their monovalent status, and a few of the 100+ lexical suffixes in *Séliš-Ql’ispé* have full-word nominal counterparts; the lexical suffix *-sqé*, for instance, is obviously related to the noun *sqélix^w* ‘person, Indian’. In order not to expand the present paper beyond reasonable bounds, we will not consider the implications of this interpretation here, in spite of its (indirect?) relevance to the general topic of transitivity.

2.9 Transitive-prone stems detransitivized by the reflexive *-cút*

The final construction we will consider is the reflexive in *-cút*, which – like reflexives in many other languages – detransitivizes the potentially transitive stem to which it is added. As with verbs detransitivized by lexical suffixes, these reflexives are straightforward intransitives: the pronominal subject is the usual intransitive subject proclitic (41, 43), and a full-noun subject NP is marked optionally by *lu* (42). For obvious semantic reasons, the reflexive takes no overt object NP. The reflexive construction differs strikingly from two of the four transitivity-reducing constructions we saw above: unlike the antipassive and lexical-suffix constructions, instead of replacing the usual transitive apparatus, the reflexive suffix is added to it, immediately after the transitive suffix.¹⁴ In other words, the reflexive suffix replaces the (object and) transitive subject suffix(es). Like lexical-suffix constructions, but unlike the antipassive, a reflexive may be retransitivized by the addition of the derived transitive suffix, as in (44).

- (41) Čn ct'ipmncú tl' esšít'.
 čn c-t'yí-p-mí(n)-nt-cút tl' esšít'
 1SG.INTR.SBJ LOC:hither-fall-INCH-DER.TR-TR-REFL from tree
 'I came down from the tree.'
- (42) Qsncú lu Čoní.
 qs-nt-cút lu Čoní
 scratch-TR-REFL 2ndary Johnny
 'Johnny scratched himself.'
- (43) Čn esplscúti.
 čn es-púls-st-cút-mí
 1SG.INTR.SBJ ACTUAL-kill-TR-REFL-INTR.CONT
 'I am killing myself.'
- (44) K^wu čta^xwlmncútmntm.
 k^wu č-ta^xwl-mí(n)-nt-cút-mí(n)-nt-ém
 1SG.OBJ LOC:to-start-DER.TR-TR-REFL-DER.TR-TR-BACKGRND.AG
 'He came up to me.'

This completes our survey of nine *Séliš-Ql'ispé* constructions that are relevant to an analysis of the language's transitivity system. The next step is to try to pull the various constructions together into a less fragmented system.

¹⁴The backgrounded agent suffix *-ém* also follows a transitive suffix, as does the transitive continuative suffix *-m* if the verb is ditransitive.

3 An analysis of transitivity in Séliš-Ql'ispé

In this section we will propose an analysis in which several of the transitivity-related constructions illustrated in Section 2 fit together in a coherent overall picture. We should begin by noting that plain intransitives – those without an antipassive suffix or another suffix that derives an intransitive verb – fall outside the transitive system entirely; they are included only to show what a basic intransitive construction is like, with its subject proclitics and its full-word subject NP marked only by optional *lu*.

As we said in our introduction, two main variables turn out to correlate interestingly with transitivity alternations in Séliš-Ql'ispé. First, there is a systematic morphosyntactic distinction between semantically transitive constructions with a FOCUS ON THE AGENT and those with a FOCUS ON THE PATIENT; and second, ASPECT plays a role in conditioning transitivity alternations. On our analysis, the ordinary (noncontinuative) transitive construction carries no particular emphasis on agent or patient, and no special marking of aspect: it is the neutral transitive construction, and the closest to a prototypical transitive construction that involves a completed transfer of action from a definite agent to a definite patient. The object NP is most closely linked to the verb, as indicated by its lack of obligatory case marking; in a ditransitive form, only one object NP, usually the recipient (the “indirect object”), may lack case marking. A full-word subject NP in a transitive construction is marked obligatorily by oblique *t*. This neat picture is complicated by the influence of definiteness, a feature that often affects transitivity in other languages (including elsewhere in Salish): an indefinite patient NP may be marked with oblique *t*. This alternative marking, though it is not at all consistent in Séliš-Ql'ispé, indicates in effect a reduction in the transitive force of the verb – a deviation from the prototypical transitive.

The next three constructions are all characterized by a suffix $-(\acute{e})m$. We propose to treat all three of these suffixes as a single morpheme *-em* with one general function and with specific interpretations linked to the various morphological environments in which it occurs: the antipassive $-(\acute{e})m$ occurs in absolute final position, without a preceding transitive suffix and without an actual aspect/nominalizer prefix plus possessive agent; the backgrounded agent $-\acute{e}m$ occurs in absolute final position after a transitive suffix and without a nominalizing prefix plus possessive agent; and the transitive continuative *-m* occurs word-finally except for a possessive agent suffix, and it always co-occurs with an aspect/nominalizer prefix plus a possessive agent. In other words, the three specific functions (designated by our three labels) of these three $-(\acute{e})m$ suffixes are predictable from their morphological context within a particular verb form; the three allomorphs of our proposed *-em* morpheme are in complementary distribution.

The primary function of the proposed *-em* morpheme is to signal a reduction of transitivity – a deviation from the prototypical transitive as represented by the neutral *Séliš-Ql'ispé* transitive construction. The *-(é)m* suffixes reflect two different kinds of deviation from the prototypical transitive: they indicate either a focus on one of the two main participants in the action – i.e. the agent or the patient (or perhaps, in a ditransitive verb, the recipient) – or a change in aspect that affects the transitive force of the verb. The transitive continuative is the sole member of the aspect-changing category. In the focus category, the participant that is highlighted is predictable from the morphological context in which the suffix occurs.

The antipassive *-(é)m* emphasizes the agent – so much so that it removes the patient argument from the verb's morphology. The result is that the sole argument in the verb itself is the agent, though the stem remains bivalent. The resulting intransitive construction is partly analogous to transitive stems that are detransitivized by a lexical suffix; these too highlight the agent and have no pronominal patient marking in the verb. The reflexive in *-cút* also fits here functionally and syntactically, its formation differing from the other two agent-focusing constructions only in that it retains the transitive suffix. Example (16) illustrates one use of the agent-highlighting antipassive construction. This sentence, which means 'I skinned it and my wife sliced it', has an antipassive (*t'élm* 'sliced') preceded by an ordinary transitive verb. With the second verb comes a change of agent, a switch that is highlighted by the use of the antipassive. Note that Vogt's characterization of what we call the antipassive as occurring with an indefinite object (1940: 31) would not capture this usage, since the 'it' in question refers to the same deer throughout the sentence; the difference is the switch in agents. Vogt was partly right, because antipassive constructions very often do include indefinite patients, but definiteness is not (in our view) the primary factor.

In the backgrounded agent construction, the *-ém* focuses on the patient. This is evident, for instance, throughout the particular *Qeyqeyši* tale from which example (18) is taken: as described above, *Qeyqeyši* is the main character in all the stories about him, even this one, where his friend One-Night is the instigator of the prank and the agent of most of the transitive verbs. *Qeyqeyši*'s more prominent overall status is highlighted by the use of the backgrounded agent construction throughout for all verbs in which One-Night is the agent.

As mentioned above, the transitive continuative construction does not participate in the argument-focusing functions of the other two manifestations of the proposed *-em* morpheme. Instead, its role is to signal an aspectual deviation from prototypical transitivity: this construction reduces transitivity by indicating that the action is not completely transferred from an agent to a patient. The

reduced transitivity of this construction is reflected morphologically in its one clear intransitive-like feature, the lack of a transitive suffix in monotransitive continuative forms.

All three $-(\acute{e})m$ formations, then, can be viewed as deviating from a prototypical ordinary transitive to a form that is lower on the transitivity gradient – either with unbalanced emphasis on one participant or with a deviation from the prototypical completive aspect. It is interesting to note that only the patient-highlighting formation, the backgrounded agent construction, remains straightforwardly transitive morphologically. By contrast, the agent-highlighting antipassive is morphosyntactically intransitive, and the transitive continuative construction, though transitive, is morphologically peculiar for a transitive verb.

We were initially tempted to combine all four constructions with m suffixes into a single morpheme – the three just discussed and also the derived transitive construction in $-m\acute{i}(n)$ (see Thomason & Everett 1993). The derived transitive construction might also be viewed as highlighting the patient, since it adds a patient to the verb's argument structure. But because it increases the valency of a verb root or derived stem, it is difficult to argue that it reduces transitivity; moreover, it is incompatible with the other three m constructions phonologically. All four suffixes surface frequently, perhaps most frequently, simply as $-m$, which is their predictable form unless they are stressed. But the stressed allomorph $-\acute{e}m$ of our proposed $-em$ morpheme cannot be reconciled with the stressed allomorph of the derived transitive.

The idea of combining two or more of these $-(\acute{e})m$ suffixes into one morpheme is of course not new, although our particular interpretation and our grouping of all three into a single morpheme are, as far as we know, unique. For instance, some authors connect the antipassive and the backgrounded agent suffixes; examples are Kuipers (1967) (Squamish), Darnell (1990) (Squamish, with an analysis that, like ours, involves de-emphasis of one argument in each case), and Gerdtz (1989: 185) (Halkomelem). Other authors, e.g. Vogt (1940: 32) (Kalispel), Newman (1980: 158–159), and Kroeber (1991: 294), group the antipassive and the transitive continuative suffixes together. Still others, e.g. Kinkade (1981: 105) (Moses-Columbia), consider the antipassive and the derived transitive suffix to be the same.

The remaining two constructions discussed above, the effect of certain lexical suffixes on transitivity and the detransitivization of stems by the reflexive suffix $-c\acute{u}t$, are obviously morphologically distinct from our $-em$ morpheme, but they share the function of reducing the transitivity of stems to which they are added. These two constructions therefore contribute to the overall picture of gradient transitivity in Séliš-Ql'ispé.

Our analysis ends here: this is as far as we have proceeded in our effort to work through the complex Séliš-Ql'ispé facts related to transitivity. We should close by emphasizing again that our analysis is necessarily incomplete. Aside from remaining gaps in our understanding of the constructions we have already examined, there are still other constructions that must be studied before we can aim at a complete analysis of the system. But we hope to have shown, at least, that there are interesting interrelationships among transitivity-related constructions that seem at first glance to be quite disparate.

Abbreviations

Besides the abbreviations from the Leipzig Glossing Conventions, this chapter uses the following abbreviations:

2ndary	‘secondary in importance’, a complement or subordinate to the main predicate
ACTUAL	actual aspect (as in continuative forms and certain stative forms)
BACKGRND.AG	backgrounded agent
CONT	continuative aspect
DER.TR	derived transitive (a transitivity suffix)
DIM	diminutive
INCH	inchoative
REL	relational (indicating that there is a recipient or other “indirect object”)
STATV	stative

Acknowledgments

We are both also grateful to Tony Woodbury and † Ken Hale for early discussions of some of the data in this paper and for offering valuable comments about the analysis; we have made use of some of their suggestions in the overall analysis in Section 3. The two authors of this paper have not worked together on this or other projects since shortly after the decade when we were colleagues at the University of Pittsburgh, but the first author views the current version of this paper as arising from the wonderfully stimulating intellectual connection we forged during that decade. She wishes to express her gratitude to the second author for that connection and for more than thirty-five years of friendship. She is one of the greatest admirers of his stellar record of sustained fieldwork and

linguistic documentation combined with valuable theoretical contributions, and of his linguistic insights, his intellectual courage, his remarkable ability to meet vituperation with courteous reasoned discussion, and his terrific sense of humor.

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Chapter 12

Contextualizing typologically remarkable sound patterns in Pirahã: A quantitative approach

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The sound system of Pirahã includes several remarkable phenomena. The present work seeks to illuminate some of those phenomena via the typological contextualization of a few of the most unusual phonetic and phonological features of the language. The study relies on previous research as well as new analyses of transcribed word lists in the UCLA Phonetics Lab Archive, alongside analyses of crosslinguistic databases of phoneme inventories and word lists. Three phenomena are focused upon: i) The small phoneme inventory in the language, contextualized against the distribution of phoneme inventories worldwide. ii) The vowel formant space of adult Pirahã speakers. iii) The unusually high reliance on vowels and glottal consonants, and the concomitant rarity of oral consonants and consonant clusters in this Amazonian isolate. This latter suite of interrelated features is uncovered via contrasts of patterns in Pirahã word lists with those of over four thousand language varieties worldwide. The language's high degree of reliance on vowels and glottal consonants is perhaps the most remarkable feature of its sound system, given that it is a statistical outlier in this respect. I suggest that this unusual feature may contribute to the challenges outsiders face when trying to learn the language.

1 Background

Pirahã is an Amazonian isolate with a number of typologically unusual characteristics. Daniel Everett, my father, brought attention to this language through a



series of studies published over the last few decades, based on his extensive fieldwork (Everett 1982, Everett & Everett 1984, Everett 1986, 2001). These studies include evidence for the language's lack of number words, which has been verified experimentally by other scholars (Frank et al. 2008, Everett & Madora 2012). The language also exhibits rarities in other lexical domains, including its terms for colors and kin relationships (Everett 2005). It displays uncommon morphological and syntactic characteristics as well (Thomason & Everett 2001, Everett 2012). Most famously, there is an absence of evidence for syntactic recursion in the language (Everett 2005). A study of a corpus of transcribed clauses supports this absence, to the extent that it yielded no evidence that Pirahã grammar allows recursive clauses (Futrell et al. 2016). While such lexical and morphosyntactic characteristics are certainly rare crosslinguistically, some of them are apparently found in other languages. Absence of recursion has been claimed for several languages, for instance, and other languages lack, or once lacked, precise number words (Pullum 2020, Everett 2017b).

The phonetic and phonological characteristics of Pirahã are also unusual in some regards. For instance, the language has one of the world's smallest phoneme inventories (Everett 2009). It also exhibits unusual socio-phonetic variation across genders: Women can produce a voiceless alveolar fricative instead of a voiceless glottal fricative, though this sociolinguistic variation may be more pronounced in some villages (Keren Madora, personal communication). Another intriguing feature is the presence of onset-sensitive stress, which was not attested crosslinguistically prior to the publication of Everett & Everett (1984). Also, the language has one very unusual allophone, a flap that requires tongue contact at both the alveolar ridge and the lower lip, and in so doing requires tongue protrusion from the mouth (Everett 1982).

Many of Pirahã's remarkable characteristics owe themselves at least partially to its status as a language isolate, the last survivor of the Mura family. Another factor involved in promoting these unusual characteristics is the culture of the people, which proscribes the adoption of most aspects of other cultures, including number words (Everett 2005). In the next two sections I offer some discussion of a few of the typologically remarkable phonetic and phonological features of the language, though this is not meant to be an exhaustive review of those features, especially since many of these features have been documented extensively in the literature – largely through the work of my father. In Section 2 we will examine the language's small phoneme inventory, contextualizing it against patterns evident in worldwide surveys of phoneme inventories. In Section 3 we will examine some of the language's phonotactic features, demonstrating with a novel approach that the language is quite unusual in terms of its reliance on

vowels and glottal consonants. I suggest that the latter feature likely contributes to the well-known difficulty of non-Pirahã acquiring the language, which has been observed for the last several decades. In Section 4 I offer some concluding remarks.

2 The phoneme inventory

The Pirahã phoneme inventory is famously quite small, with eight consonants and three vowels, though the figure of eight consonants is open to some debate given the socio-phonetic variation mentioned above for the alveolar and glottal fricative. However, the phoneme inventory is relatively normal in terms of its phoneme types. The four voiceless stops in the language are /p/, /t/, /k/, and /ʔ/. The first three of these voiceless stops are among the most common phonemes, in terms of both crosslinguistic frequency and frequency within word lists, across about 7000 word lists and over 3000 phoneme inventories (Everett 2018b, 2021). The two voiced stops are /b/, which has an [m] allophone, and /g/, which has an [n] allophone. The language is somewhat unusual in that it is missing a voiced alveolar plosive phoneme but has a voiced velar plosive. The reverse pattern is much more common typologically (Everett 2018a). As noted above, there is also a glottal fricative with an alveolar fricative variant in the language (Everett 1986). All of these consonants are quite common crosslinguistically. Similarly, while Pirahã only has three vowel phonemes, these are peripheral vowels with very distinct formant characteristics, vowels we might expect in a three-vowel system: /a/, /i/ and /o/.

To get a sense of how common these consonants and vowels are crosslinguistically, we can look at the PHOIBLE database of 3,183 phoneme inventories (Moran & McCloy 2019). There we see that /k/ is found in over 90% of the world's languages, and is the most common voiceless stop typologically. /p/ is found in about 86% of the world's languages, while /t/ is found in 68% of them. /b/ and /g/ are found in 63% and 57% of the world's languages, respectively. /h/ is found in 56% of PHOIBLE inventories. Turning to the vowels, /i/ and /a/ are found in 92% and 86% of the world's phoneme inventories, respectively, while /o/ is found in 60%. The only phoneme in Pirahã that is not found in well over half of the world's languages is the glottal stop, and even that sound is fairly common as a phoneme, as it is found in 37% of the phoneme inventories in PHOIBLE.

While the language has one of the world's smallest phoneme inventories, then, most its phonemes are quite common typologically. Also, it is worth noting that many of the world's phoneme inventories are not much larger than that of Pi-

rahã. Framed differently, if we plot a density distribution based on the number of phonemes in the world's phoneme inventories, the values representing phoneme inventory size will be compressed along the leftmost portion of the x-axis. The same can be said if we plot the number of consonant phonemes or vowel phonemes. This is evident in Figure 1, which includes three density distributions of phoneme-inventory sizes, based on the UCLA phonological segments inventory database (USPID, Maddieson & Precoda 1989). That database contains 451 phoneme inventories from around the world.

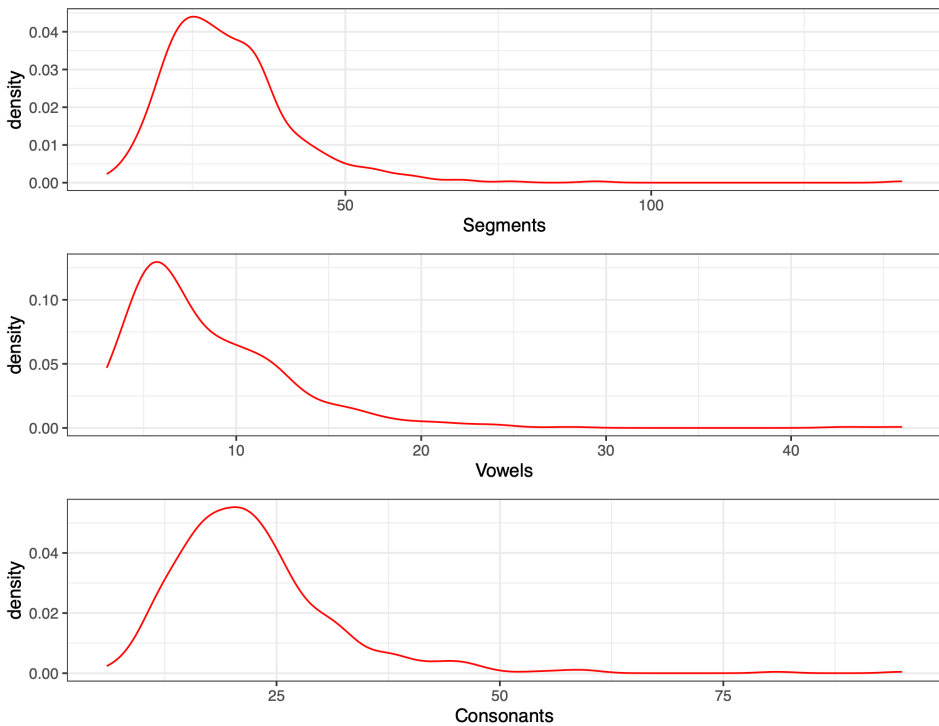


Figure 1: Density distributions of the number of phonemic segments, vowels, and consonants across the world's phoneme inventories. This is based on 451 languages in the UPSID database (Maddieson & Precoda 1989).

As is evident in Figure 1, the world's phoneme inventories range dramatically in size, but the vast majority have fewer than fifty total phonemic segments. The typological outliers are those languages at the far-right end of the distribution of phoneme, vowel and consonant inventories. Languages like Danish, with dozens of vowel phonemes, and Xóõ, with over ninety consonants according to UPSID

(estimates vary), are such outliers. If we take a common approach to defining outliers, an upper-limit outlier would include any language with a phoneme inventory that is above the third quartile by greater than 1.5 times the IQR (interquartile range). Under this approach, a phoneme inventory in UPSID would need to exceed 55 phonemes to be an outlier. There are seven phoneme inventories in the data set that do exceed this figure. Conversely, a lower-limit outlier would include any language with a phoneme inventory that is below the first quartile by 1.5 times the IQR. A phoneme inventory in UPSID would need to have fewer than five phonemes to be an outlier in this respect. Of course, no languages exhibit such a small phoneme inventory. The truth is that many languages have small sets of vowel and/or consonant phonemes, so there are no outliers on this end of the phoneme-inventory spectrum. Framed differently, many languages have one-to-a-few more phonemes than Pirahã. This pattern is apparent not just in these data but in large studies on phonological typology, for instance Gordon (2016). Nevertheless, Pirahã is certainly unusual in that it is small both in terms of its consonant inventory and vowel inventory – setting aside that it does use tones, in contrast to some other languages with three vowel phonemes. In the UPSID data, 23 of 451 languages have inventories with three vowels and many of these are not tonal. In contrast, only five of the 451 languages in that database have eight or fewer consonant phonemes.

Pirahã also has straightforward syllable structure. If we examine the 150 words representing Pirahã in the UCLA Phonetics Lab Archive, we see that all of the words contain syllables of either the CV or CVV type, and only these two types of syllable are represented. Both of these syllable types are common worldwide. In Maddieson’s survey of the syllable structure types in 486 languages, 61 languages are categorized as having simple syllable types (Maddieson 2013). Pirahã would fall into this category, which is not a particularly rare one. While Pirahã has simple syllable structure along with a small phoneme inventory that consists of a straightforward set of common vowels and consonants, these points do not imply that the language is simple, overall, in terms of its phonological or phonetic characteristics. To the contrary, there is arguably unusual complexity in the language’s sound system, at least from the perspective of learners of the language, a point I quantify below. This complexity stems from the fact that the language is tonal and relies heavily on vowels, yielding words that are distinguished inordinately by vocalic characteristics without many intervening oral consonants. This is one factor contributing to the fact that the acquisition of the language is notoriously difficult, particularly for speakers of English and Portuguese who lack familiarity with tones and are unable to easily distinguish the distinct tones of adjacent vowels. This point, to which I will return in the conclusion, is based

on my own experience with the language, having seen many outsiders struggle to distinguish or reproduce Pirahã words. It is also based on the simple fact that, to date, few outside speakers have learned Pirahã well, arguably only two in fact: Dan Everett and Keren Madora.

Previous acoustic studies of Pirahã vowels have described the formant space in the language. Keren Madora, in a description of stress correlates in the language, describes the mean formant space for the /a/ and /i/ vowels across twelve adult speakers (Everett 1998). These are evident in Figure 2. De Carvalho (2010) describes the formant space of the three vowels for two male adult speakers and one female. Carvalho also relied on the Pirahã data in the UCLA Phonetics Lab Archive. These data were collected by Peter Ladefoged, with the assistance of Dan Everett and Keren Madora, in 1995. The mean formant values for these three speakers are also depicted in the formant space in Figure 1. Note that the /a/ vowel is articulated at a wide range of points along the F1 dimension, suggesting some freedom in tongue height for this vowel. In contrast, the /i/ and /o/ vowels appear to occur in a more constricted portion of the vowel space. An important caveat is that, since these formant values are not normalized, they do not necessarily reflect meaningful inter-speaker variation. Formant values are affected by vocal-tract length, for instance, which varies across speakers. It is not particularly surprising that the lone female speaker, of the three examined in de Carvalho (2010), has the highest F2 value for /i/ and the highest F1 value for /a/. These points are characteristic of females, given their typically higher fundamental frequencies (owing to smaller vocal folds), as well as their typically smaller oral and pharyngeal cavities. The /i/ vowel in the language varies somewhat along the F2 dimension, across speakers, but it also varies across contexts. In many words it is pronounced as a near-front high vowel, for instance in the first-person pronoun /ti/. Given that the voiceless alveolar plosive is produced as a voiceless postalveolar affricate before the high front vowel, this pronoun is actually pronounced most commonly with the [ɪ] vowel, judging from my own experiences in Pirahã villages.

In short, the Pirahã vowel inventory is small and occupies expected regions of the vowel space. The Pirahã consonant inventory is also small and consists of phonemes that are common cross-linguistically. While the consonant and vowel inventories are small, this small size does not imply straightforward simplicity in its sound system. As noted above, previous work has documented some unusual allophonic variation and stress patterns (Everett & Everett 1984, Everett 1986). Finally, while the Pirahã phoneme inventory is atypical in terms of its size, it is not a statistical outlier in this regard since no languages are technically outliers at the low end of the inventory-size spectrum. Next we turn to some characteristics

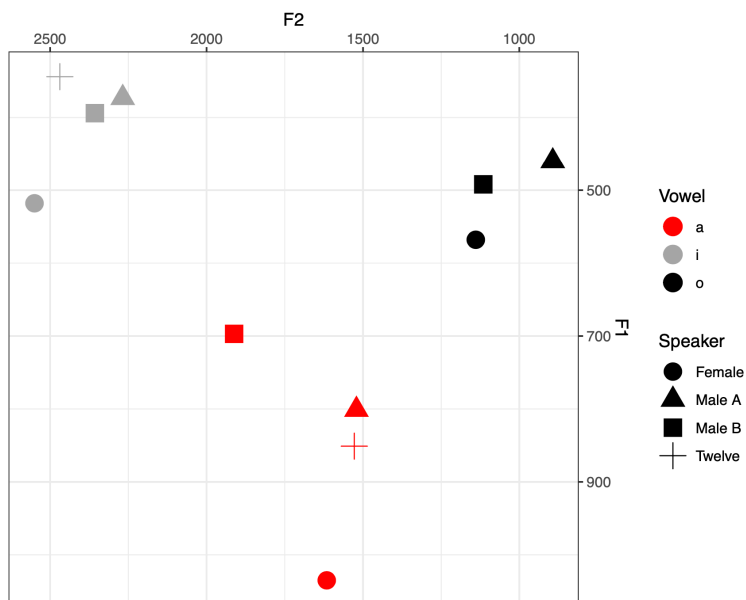


Figure 2: Vowel spaces based on values in Everett (1998) and de Carvalho (2010). The values in Everett (1998) were based on twelve adult speakers. Their mean F1 and F2 values are depicted here.

of the sound system that are unambiguously outliers from a typological perspective, characteristics that hint at the unique challenges that those acquiring Pirahã must overcome.

3 How the Pirahã sound system is a global outlier

While phoneme inventories tell us something about the sounds that are meaningfully contrastive in a language, they tell us nothing about the commonality of those sounds or about the way those sounds are typically distributed within words in a language. The frequency and distribution of sounds within a language can offer a bit more detail regarding the role that individual phonemes play in a language. Recent research examining the intra-linguistic distribution and frequency of sounds has uncovered a variety of findings related to, for instance, the functional load and informativity of sounds (see, for instance, Wedel et al. 2013 and Priva 2017.) Other work has examined the frequency of sounds to demonstrate that, across the world's languages, the frequency of consonants within a language generally follows a power-law distribution not dissimilar from that evident in the frequency of word types in a corpus (Everett 2018b).

If we examine the one hundred and fifty words in the UCLA Phonetics Lab Archive, we can see that such patterns also hold in Pirahã. Some of the phonemes in the language are particularly frequent in the words. To describe such patterns quantitatively, I imported the 150 words into R as strings of IPA characters. While this is obviously a small data set, it is worth noting that these 150 words contain many basic semantic concepts that one would expect to be common in actual speech. Using R (Venables et al. 2009), I obtained the relative frequency of Pirahã phonemes across these 150 words, which contain a total of 982 phoneme tokens. In these words for basic semantic concepts, /i/ is the most common phoneme, with 267 tokens (27.2%). The second-most common phoneme is /a/, with 223 tokens (22.7%). The remaining sounds, in descending order of frequency, are /o/ (12.1%), /ʔ/ (7.9%), /g/ (6.9%), /b/ (5.8%), [s] (4.9%), [h] (4.1%), /p/ (4.0%), /t/ (2.2%), and /k/ (2.1%). Note that I separated [s] and [h]. The motivation for this separation will be evident below. A couple of observations are worth making, based on this ordering of sounds according to token frequency. First, while the language does have a common set of oral voiceless stops, namely /p/, /t/, and /k/, these are not common in the words. In fact, the latter two phonemes appear to be the least common in the language. At the other end of the spectrum, the three vowels are quite common, with /i/ and /a/ combined representing about half of all the language's phoneme tokens in the data considered. This point bears stressing: Roughly half the sounds of Pirahã, judging from the words in the UCLA Phonetics Lab Archive, are variants of the high-front and low-central vowels.

We can use the data to get a sense of some of the common sound sequences in the language, particularly in word-initial and word-final positions. The three most-common word-initial sounds are /ʔ/ (31.3% of words), /b/ (12.7%), and /k/ (12%). Most of the occurrences of the latter phoneme are in word-initial position. A more striking pattern surfaces word-finally: 86% of the transcribed words end in some variant of the /i/ vowel. It is worth noting that most of the words in the data are nouns, so there may be some lexical bias here as nouns are more likely to end in /i/. (Keren Madora, personal communication.)

Word-medially, the most common sequence of two phonemes is /ai/, which surfaces 77 times in CVV syllables in these words. Taking these points together, we might state that a typical-sounding word in the language could begin with /ʔ/, end in /i/, and have an /ai/ sequence. The spectrogram of one such word, ʔáapahai 'bird arrow' is depicted in Figure 3.

In the highlighted features of the spectrogram in Figure 3, we can see the following: In A, the two /a/ vowels with distinct tones have very similar formant structures with respect to F1 and F2. This is evident in the dark bars within the rectangle. Yet the pitch is higher for the first /a/ vowel, which carries a high tone,

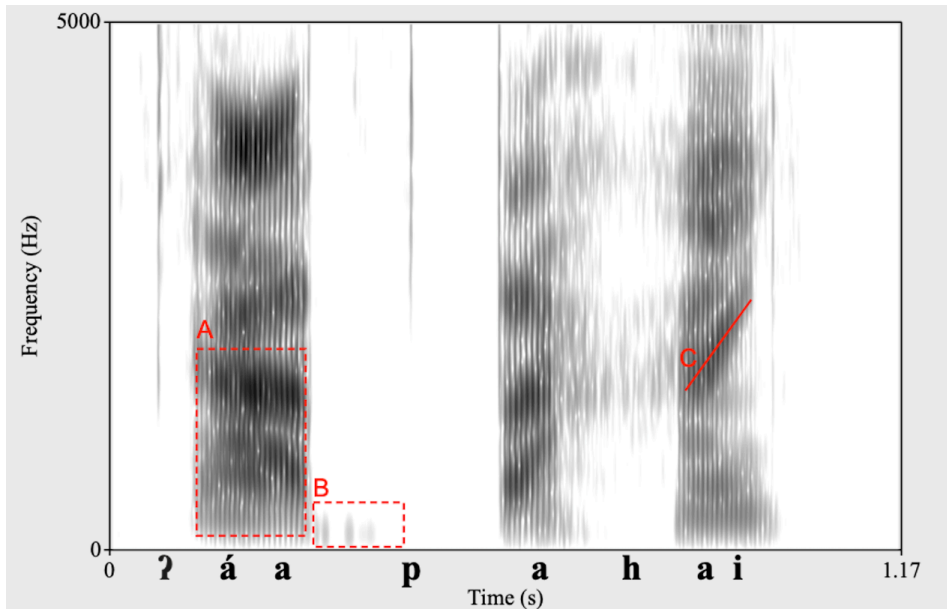


Figure 3: Spectrogram of the word ʔáapahai, ‘bird arrow’ a typical word in Pirahã. The word is typical in that it only contains one oral consonant, is largely comprised of vowels and glottal consonants, begins with the glottal stop, and ends in /i/. Spectrogram created via PRAAT (Boersma 2001).

as evidenced by the more compressed vertical striations that reflect vocal cord vibration. In B, we see that the /p/ consonant is relatively long and that the first half of it exhibits some voicing, though these characteristics may be partially an artifact of the deliberate pronunciation associated with word-elicitation tasks. Finally, C is a line highlighting the second formant in the /ai/ sequence, the most common sequence of two sounds in the language judging from these data. This rising F2 formant is found at the end of many of the words in the data set, as many end in /ai/.

Given that the three most frequent phonemes in the language are /i/, /a/, and /o/, the ratio of Pirahã phoneme tokens that are vowels is quite high. In fact, there is evidence that the language relies on vowels more than any other language, at least judging from the Automated Similarity Judgement Program (ASJP) data (Wichmann et al. 2016). Each language variety in the ASJP data is represented by a transcribed word list for 40–100 basic concepts. In Everett (2017a) I analyzed the transcriptions of the word lists in 4,012 language varieties, using the stringr packaged in R (Wickham 2019). (The code is available in the SI of that study.)

That analysis yielded a figure for each language variety, a figure representing the ratio of vowels as a proportion of all sounds in each word list. I referred to this ratio of vowels as a language’s “vowel index”. Unlike the phoneme inventory data evident in Figure 1, if a density distribution of all the “vowel indices” are plotted for the 4012 varieties, they approximate a Gaussian distribution (Everett 2017a).

The goal of Everett (2017a) was unrelated to Pirahã. Instead I aimed to test the hypothesis that very cold/dry air yields articulatory pressures against the usage of vowels in cold/dry regions. This hypothesis was based on extensive laryngology data suggesting that dry air increases jitter and perceived phonatory effort during speech. Since the publication of Everett (2017a) more lab-based research has offered evidence of this, including work demonstrating effects in non-lab settings (Alves et al. 2019). The results offered in Everett (2017a) are correlational and could be coincidental though the pattern seems to be generally robust to the confounds of language relatedness and language contact. Furthermore, some researchers believe this distribution is due to ecological adaptivity owing to acoustic rather than articulatory factors (Maddieson 2018). This line of inquiry is mentioned in the present context simply because it underscores an interesting feature of Pirahã, namely that it relies so heavily on vowels.

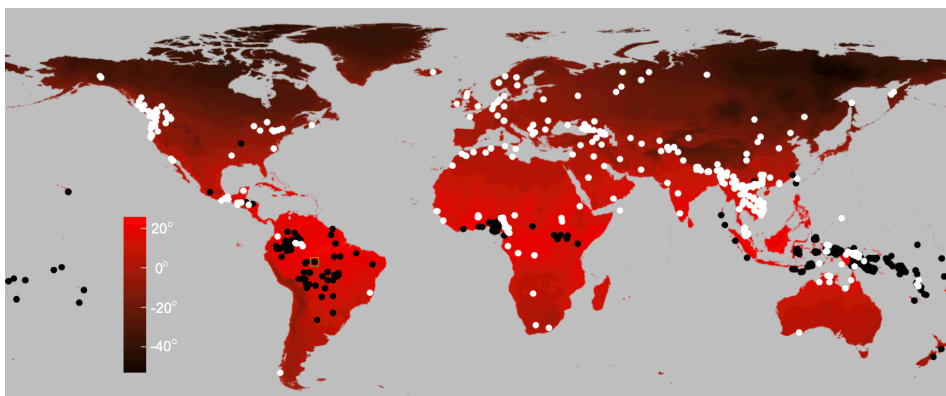


Figure 4: Locations of eight hundred language varieties from the ASJP database. The black dots represent the top 10% of the 4012 language varieties in Everett (2017a), according to number of vowel sounds in a variety’s word list, as a ratio of all transcribed sounds in that list. The white dots represent the bottom 10% of language varieties according to this metric. The map coloring is based on a raster layer created by the mean temperature of the coldest month, taken from the global bioclim data (Noce et al. 2020). Pirahã is highlighted with a square.

Languages with the highest and lowest vowel “indices”, i.e. ratios of vowels-to-all-sounds in the ASJP word lists, are depicted in Figure 4. In the figure, Pirahã is highlighted with a square, as it is one of the languages with the top 10% of vowel indices according to the ASJP data. As can be seen in Figure 4, Pirahã is typical in one sense: Nearly all of the languages with high vowel ratios occur in the tropics. In fact, only eight of the four hundred languages in the top 10%, according to “vowel index”, are found above the Tropic of Cancer or below the Tropic of Capricorn. In contrast, 212 of the four hundred languages in the bottom 10% occur outside the tropics. Setting aside the question of whether this is purely coincidental or due to some ecologically adaptive characteristics of languages, like those hypothesized by myself or Ian Maddieson, what is clear is that most languages that share this characteristic with Pirahã lie somewhere near the equator. It has long been known that many language families of the Pacific, Amazonia, and elsewhere rely heavily on simple syllable structures (and therefore rely heavily on vowels), yet the extent of the pattern evident in Figure 4 is surprisingly pronounced. Interestingly, Pirahã is an outlier even among Amazonian and South American languages in terms of its reliance on vowels. If we plot the vowel index data from Everett (2017a) by continent, for instance, we see that the language would be an outlier on any continent according to this parameter. This is apparent in Figure 5.

One might wonder how representative the ASJP data are, given that they only encode 40–100 concepts per language variety and given that the transcription system used in the database is coarse. However, in those cases in which ASJP data are cross-referenced with other data, the results are generally quite similar (see, e.g., Everett & Chen 2021). We can test the data against the UCLA Phonetics Lab Archive data, for example. In the case of Pirahã, 609 of the 982 transcribed phoneme tokens in the UCLA data are vowels. In other words, 62% of the sounds are vowels in that data set, in contrast to 64% in the ASJP data. Even if we adopt the figure of 62%, the language would remain an outlier in this respect. In the ASJP data, fewer than 1% of the languages have “vowel indices” above 0.60. Even more remarkably, the language relies much less on consonants made with the lip or tongue, when contrasted to the world’s languages judging from the ASJP data. As noted above, the glottal consonant phonemes in the language are quite common. Taking the vowel frequency and glottal consonant frequency together, one might conclude that the load carried by laryngeal articulations is exceedingly high in the language. In the ASJP data 74% of the transcribed Pirahã sounds are vowels or glottal consonants. In the 150 transcribed words in the UCLA Phonetics Lab Archive data, the exact same figure (74%) obtains. (There is some modest overlap between the words in these data sets.) In other words, three quarters of all

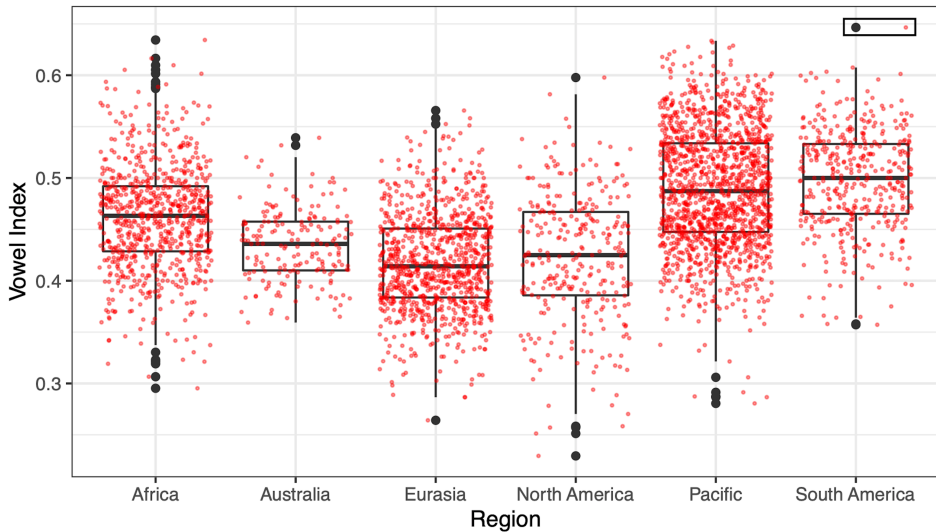


Figure 5: Contextualizing the high vowel reliance of Pirahã. “Vowel index” denotes the ratio of all transcribed sounds in an ASJP word list that are vowels. Each of 4012 language varieties is represented via a red dot. Dots are separated along the x-axis, within each column, via the jitter function in R. Pirahã is highlighted with a rectangle, which includes the black dot representing the outlier for the boxplot of South American languages in the data considered. All other regional outliers are also represented with black dots.

sounds in the basic Pirahã words in these data sets are not oral consonants. Using a function created via the `stringr` package in R (code available upon request), I calculated the ratio of sounds that are vowels or glottal consonants, across each of the same 4012 ASJP word lists in Everett (2017a). As evident in Figure 6, Pirahã is an even more pronounced outlier in this respect. Only eight of the 4012 language varieties have vowel-plus-glottal ratios greater than 70%, and none obtain a figure as high as the 74% in Pirahã.

4 Discussion and conclusion

Subsequent to the publication of my father’s 2005 paper in *Current Anthropology* (Everett 2005), a number of papers were published contesting his claims. Oddly to some, these papers were published by scholars who had either no first-hand familiarity with the language, or had only marginal experience with the language, and certainly had no attested fluency in the language. Arguably, part

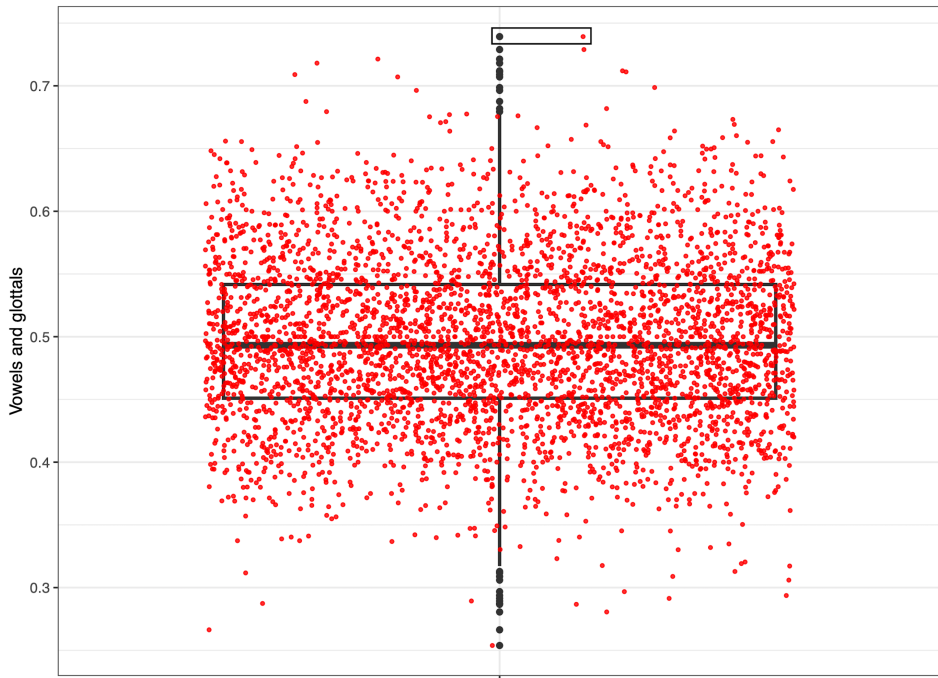


Figure 6: Ratio of sounds in ASJP word lists that are vowels or glottal consonants. Pirahã is highlighted via the rectangle. Dots are separated along the x-axis via the jitter function in R.

of the explanation for the lack of successful follow-up research on Pirahã grammar, despite the extensive attention the debate surrounding it received, is that the language is so difficult to learn. There are, as of yet, no truly bilingual Pirahã who could serve as language resources to outsiders who do not speak the language. In my own experience with the people since I first spent some of my childhood in Pirahã villages some decades ago, I have seen numerous missionaries and linguists journey to the Pirahã, with varied aims. Some of these visitors have produced manuscripts on a variety of topics. Despite such work I have never seen an outsider maintain an extensive conversation in Pirahã, besides Dan Everett and Keren Madora. To my knowledge, no outsider has been able to demonstrate any degree of fluency. This is not meant as a criticism to those who have tried, instead I think this point merely underscores how difficult it is to learn the language. It took many years of work as missionaries, living in the village much of that time, before my parents could speak the language. I was there for much of this time, and can personally attest to the frustrations they conveyed and obstacles they

overcame along the way in learning the language. Setting aside potential factors like appropriate training and aptitude, it seems unlikely that others could learn the language well without spending years on the effort. One would imagine that few interested parties would have the combination of time and funding that my parents dedicated to this task.

This begs the question as to why the language is so difficult to acquire, apparently even when contrasted to some other Amazonian languages that government officials, missionaries, and others have acquired with high degrees of fluency. It is not just that it is difficult to gain fluency with the grammar of the language, many outsiders struggle stringing together basic words into simple phrases. I suspect that part of the reason may be Pirahã's unusual phonetic and phonological characteristics that yield difficulty of both production and discrimination for outsiders. I have heard plenty of anecdotes from people visiting Pirahã villages suggesting that, for instance, the language sounds like "bees buzzing" and that it is hard to distinguish words given the heavy reliance on tones. Such stories hint at the key pattern outlined in Section 3 above: The language really is an outlier when it comes to its heavy reliance on vowels and glottal consonants or, framed differently, its limited reliance on oral consonants. Distinguishing a series of vowels, often with distinctive tones and relatively few intervening consonants, many of which are glottal, is an exceedingly difficult task for outsiders. Conversely, on the articulatory side, the language relies an inordinate amount on laryngeal gestures, including the creation of precise tone sequences without intervening oral consonants. This is an entirely unfamiliar enterprise to many. (Impressionistically, I also find it to be a difficult language to pronounce, despite my childhood experiences in Pirahã villages, even when contrasted to some other languages in the region.) Evidence now suggests that some languages are in fact more difficult to acquire, including by children, because of the unique characteristics of their sound systems. For instance, Danish, with so many vowel qualities, poses unique challenges for language acquisition for first and second-language learners (Trecca et al. 2021). While Danish occupies the other end of the vowel-phoneme spectrum as Pirahã, in that it has many vowel phonemes, Pirahã relies more heavily on vowels than any other language according to the data discussed above. Further, there are interesting intervocalic differences in such sequences due to tone variations, and such sequences of tone-varying vowels appear to contribute to the challenge of acquisition by outsiders.

Languages are profoundly diverse. An increasing number of scholars believe that this diversity is the chief explanandum that should occupy language researchers (Evans & Levinson 2009). My father's work on Pirahã underscored to many just how diverse languages could be. While debate will likely persist regarding some of his specific claims, perhaps especially because of the lack of

other linguists who actually speak the language, it is clear to many of us that Pirahã exhibits some typological rarities that pose difficulties of various sorts to universalist approaches to language. That may say less about the language, of course, and more about the inadequacy of such approaches in the face of seemingly limitless linguistic variation. (As noted by Piantadosi (2024), Chapter 15 of this volume, the influence of Chomskyan approaches to language appears to be crumbling, for reasons he elegantly lays out.)

In this paper I have outlined a few aspects of the sound system of Pirahã, suggesting that it is unique in some respects that are quantifiable. While the language is known to have a small phoneme inventory, I have suggested that this is arguably not the most remarkable feature of its sound system, partially because the global distribution of phoneme inventory sizes is compressed in the manner evident in Figure 1. No known languages are technically outliers in terms of having small phoneme inventories, though some like Pirahã and Rotokas have unusually small inventories. The phoneme inventory of Pirahã is small and consists of phonemes that are quite common crosslinguistically. This may give the impression of simplicity of the language's sound system, but I have suggested this would be inaccurate. Instead, I have argued that perhaps the most remarkable feature of the Pirahã sound system is its extreme reliance on vowels, and also its combined reliance on vowels and glottal consonants, judging from analyses of the ASJP data and the UCLA Phonetics Lab Archive data. It is a regional and global outlier in both of these respects. The language is characterized by strings of vowels with varying tones and limited intervening oral consonants, a fact that presents perceptually and articulatory unique characteristics that likely contributes to its difficulty of acquisition for outsiders. If someone spends time listening to and producing Pirahã, they are unlikely to be left with an impression that the sound patterns in the language are simple. Quite the contrary, in my experience they may be baffled by its sound patterns, potentially because of the language's status as a typological outlier in the sense that I have outlined here.

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Chapter 13

What is the simplest semantics imaginable?

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We claim that three binary relations, 0, 1, and 2, are both necessary and sufficient for formal semantics: 1 and 2 are the well-known “subject of” and “object of” relations, and 0 corresponds to the subsumption or “is a” relationship well known from knowledge representation. We describe how these can be used to compositionally assign a semantic representation built from primitives (morphemes, semantic atoms) and how the system can be related to the computational “word vector” semantics which is surprisingly effective even though it appears to employ no grammatical rules or constraints.

1 Introduction

There is no evidence that in English the vestigial system of object marking can be extended beyond personal pronouns, yet we have little doubt that English speakers can fully grasp transitive constructions involving inanimate objects. Since most linguists assume that coordination and subordination will be present in every language, Everett’s discovery of a language lacking syntactic facilities for these is seen as some grave error akin to a hypothetical discovery of a language lacking subjects and objects. But when viewed from the perspective of semantics, impoverished syntax is no more surprising than impoverished morphology, so the question should be: what is the absolute minimum we require for semantics?

In this paper, we start from the simplest imaginable cases, subject-predicate and modifier-head constructions, and gradually build up a system of semantic



representation both in the tradition of Knowledge Representation (KR; Brachman & Levesque 2004) and in the contemporary “thought vector” approach (LeCun et al. 2015, Kornai 2023). These are not competing but complementary views of the same subject matter, both true at the same time like the algebraic and the function-theoretic views of polynomials. While the KR view does not significantly depart from the common linguistic view that structures are to be represented by some kind of graphs (an idea common to transformational and dependency grammar formalisms), the vector approach is very unfamiliar: if the representations are n -tuples of numbers, what are the rules?

This question is especially vexing in light of the observation that the main vector operation, vector addition, plays only a marginal role in the computational system: it is used for solving analogical puzzles like *France is to Germany as Paris is to X* (Mikolov et al. 2013) and little besides. Using the KR side to explore the issue we find that three binary operations, 0, 1, and 2, are both necessary and sufficient for formal semantics. 1 and 2 are the well-known “subject of” and “object of” relations, and 0 corresponds to the subsumption relationship known as “is a” in KR and as hyponymy in lexicography. (The vector equivalents of these operations are somewhat more technical, and are not required for making our main point that the minimum is three – see Kornai (2023) for details.)

The sufficiency of these operations is not trivial – students of Relational Grammar and many similar systems will no doubt wonder about “3” and perhaps different kinds of linkers such as thematic (proto)roles or *kāra*kas. For indirect objects, the reader is referred to Kornai (2012), and for deep cases, thematic roles and *kāra*kas see Chapter 2.4 of Kornai (2023). The main line of attack in reduction to “1” and “2” is that “3” can itself be considered (together with other conceptual relations typically expressed by case markers and adpositions) to have their own subjects and (prepositional) objects. This will of course complicate the graphs (in ways that will be familiar from generative semantics) but ensure that we will never need *hyperedges* just *hypernodes*. The resulting system is rather similar to the Resource Description Framework¹ used in the Semantic Web² where binary relations are encoded in a (subject verb object) triple. Since such triples can be substituted for one another, for *give* we obtain an analysis “cause to have” so that *x gives y to z* becomes (x cause (z has y)). This method is immune to the standard criticisms (Fodor 1970) leveled against generative semantics-style meaning decomposition that were based on the pronominalization possibilities of the ‘to + inf’ natural language paraphrase, since the formulas explicitly contain this information. Kornai (2010) discusses how the other criticism, that such

¹https://en.wikipedia.org/wiki/Resource_Description_Framework

²https://en.wikipedia.org/wiki/Semantic_Web

a decomposition (cause to die \rightarrow cause not to have life functions \rightarrow cause not to metabolize, respond, ...) may never terminate, is actually irrelevant in an algebraic setup that enables circularity, and Kornai (2012) describes how higher arity verbs, such as *promise* can be handled in the same manner.

But the necessity of three different operations is even less trivial: after all, natural language semantics is often viewed as translation to First Order Predicate Calculus (FOPC; Blackburn & Bos 2015) and via combinators (Curry & Feys 1958) FOPC can be reduced to strings of a single symbol J with the appropriate parenthetization (Schönfinkel 1924, English transl. van Heijenoort 1967). We can take the no-frills approach further, since the parens can be eliminated in favor of Reverse Polish Notation (RPN), leaving us with binary strings. As the first symbol is always J , which we denote by '1', we can use '0' for the binary operator symbol of RPN, and we are guaranteed that each well-formed predicate formula corresponds to a unique integer written in base 2. Furthermore, the translation between the original formula and the binary number is computable mechanistically in either direction by a rather simple Turing machine. Taking this to the extreme, binary integers can be written in base 1, and again translation between the formats by a Turing machine is available in both directions, so that all we need is a single symbol which can be repeated as many times as we need. If we are happy with integers, base unspecified, Gödel numbering would work just as well.

This is not just a walk through some rarely visited pages of the mathematical logic bestiary. There are sophisticated attempts at using combinatory logic in semantics since the 1980s (Szabolcsi 1987, Steedman 1987, Jacobson 1999, Baldrige 2002), with important links to mild context sensitivity/polynomial parsability (Joshi et al. 1991). Clearly, neither FOPC nor higher order intensional calculi such as those employed in Montague Grammar have a privileged status as the One True Formalism (OTF) for semantics, and the search for OTF is not a trivial one. Our argument will rely on a stricter understanding of compositionality than the one generally assumed: while the mapping from Gödel numbers (or binary strings) back to logic formulas is unique, and Turing-computable, not every such mapping is compositional in the accepted sense of taking some string X , decomposing it *by simple means* as AB , and computing the meaning of the whole from the meanings of the parts A and B .

In Section 2 we set expectations by discussing some important desiderata for OTF. We also introduce some less commonly taught desiderata students of linguistic semantics may not even have heard of, such as *smooth transition from morphology to syntax* and *embeddability*, and argue that these are actually part

of the same cluster of desiderata. Our own proposal, the 4lang system (see <https://github.com/kornai/4lang/tree/master/V2>), is discussed in Section 3, where we return to the issue whether there is, or should be, a minimal system among the proposals meeting the desiderata.

2 What do we expect of semantics?

Let us begin with some standard desiderata:

- D1 Comes with reasonable model theory
- D2 Reasonably simple (compositional) mapping from natural language to OTF
- D3 Mapping in the reverse direction into passable natural language so that OTF can serve as a translation pivot
- D4 Usable for disambiguation
- D5 Usable for characterizing synonymy
- D6 Extends smoothly to verbal description of non-verbal material (music, scientific models, functional description of algorithms, ...)

D1 is taken very seriously by proponents of logical semantics, who treat all other approaches (by natural language paraphrase, by diagrams, and by KR in general) as *markerese* since Lewis (1970). To satisfy this, OTF must contain three well-defined parts: a language of formulas L , a collection of models \mathcal{M} , and an interpretation relation $i : L \rightarrow \mathcal{M}$ between the two (Tarski 1956). By well-defined we mean the existence of effective procedures to decide whether something is a (well-formed) formula and to decide whether something amounts to a model. The mapping itself needs to be not just effective (Turing-computable), but computable in a particularly simple manner we will discuss at D8 below.

For linguistic semantics to follow the same architecture one would expect L to contain all well-formed (grammatical) strings, and only these, and would use \mathcal{M} , the collection of models, to capture the world that is being talked about, with i mapping elements of the language onto their meanings. In reality, Montague Grammar (MG; Montague 1970, 1973) represents a considerable departure from this architecture. On the left side, we do not find L , natural language, but D , *disambiguated language*, a theoretical construct that contains not just the well-formed expressions of language but also their constituents and derivation histories (see discussion of D4 below).

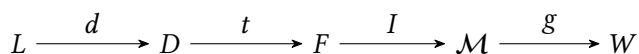


Figure 1: Information objects associated with MG

On the right side, we do not find real-world objects or even formal objects (models), but formulas F of a particular logic calculus. The full picture of MG is composed of the first two or three arrows in Figure 1, with the primary attention focused on the translation homomorphism t . The models \mathcal{M} are reasonably standard set-theoretical constructs (except for an internal time parameter that temporal semantics often relies on), and the grounding g in the real world is completely left out – Montague was no doubt familiar with Quine’s and others’ criticism of direct reference.

The disambiguation mapping d is an elegant technical device that helps a great deal in simplifying subsequent stages of the mapping. Unfortunately, scholars in the MG tradition have spent little effort on building grammatical models of natural language that could serve as a starting point for disambiguation in the sense Montague urged, and the use of d in semantics is more a promissory note than an actual algorithmic method. In this key respect, MG actually fails D4.

D2 is also taken very seriously, so much so that important ranges of phenomena where it obviously fails, such as noun-noun compounding, are simply declared out of scope for semantics. Fodor (1998) is typical in treating all word meanings as atomic, i.e. ignoring all productive morphological phenomena. This of course requires the memorization of all word meanings and brings back the psychological problem (Partee 1979, 2013) of accounting for infinite data sets in a finite brain.

Clearly, expressions like *ropeladder* ‘ladder made of rope,’ *testtube* ‘tube used for testing,’ and *manslaughter* ‘slaughter undergone by man’ (Kiparsky 1982) are not entirely compositional. Equally clearly, the meaning of novel compounds is largely predictable, as are the meanings conferred by productive derivational processes. The Lexicalist Hypothesis (Chomsky 1970) segregating morphology from syntax is clearly untenable (Bruening 2018), and in its place we offer our own desideratum:

D7 Compositional (syntactic) and non-compositional (morphological) processes must be part of the same continuum

In other words, there cannot be a different semantics for morphology and for syntax, especially as the border between the two is not uniform across languages.

It must be one and the same interpretation mechanism that takes you “from morpheme to utterance”. This is not to say that there is no *word* unit that syntax can refer to (the classical psycholinguistic evidence in favor of memorized units with lexicalized meanings cited in Müller (2018) is hardly controvertible), but simply to insist on deriving as much of this meaning by compositional means as possible. In Section 3 we offer a mechanism that deals with the non-compositional aspects by means of subdirect products, which contain the fully compositional direct products as a limiting case.

D3, while in principle compatible with many theories, is seriously underresearched. Using a natural language (typically English) as pivot (intermediary) between two languages is common both in manual and in machine translation. The use of a formal language is almost unheard of: the only proposal with actual translations is Universal Networking Language (Cardenosa et al. 2005), and the use of logic formulas is unattested. Given how common it is to consider semantics “the language of thought”, the single-minded focus on translation to, but never from, mentalese is rather surprising.

This oneness cannot be entirely attributed to the fact that systems of translation to logic formulas (including descendants of MG such as Dynamic Predicate Logic) have very little coverage to begin with. It appears the real issue is lack of transparency, a phenomenon well observable on the Schönfinkel-style reduction step of replacing the standard *S* and *K* combinators by a single combinator *J*. This *J* is defined by cases:

$$Jx = \begin{cases} K & \text{if } x = S \\ S & \text{otherwise} \end{cases}$$

Therefore, we have $JJ = S; J(JJ) = JS = K$ eliminating the original *S* and *K* entirely in favor of a single entity. Notice that the method would be just as applicable if we didn’t have 2 things to reduce but 52. We would only need to stretch the case-by-case definition accordingly (see Curry & Feys 1958: Chapter 1E4).

For a concrete example, consider the translation of the English reflexive pronoun *himself* which Szabolcsi (1987) argues to be the combinator *W*, defined as $Wxy = xyy$. In the standard *S, K* basis *W* is expressible as $((SS)(SK))$ so *W* is $((JJ)(J))((JJ)(J(JJ)))$. Continuing with the no-frills approach, the order of applications encoded in the parenthetization can be just as well encoded by RPN, using the operator symbol \circ . This will make the formula into $JJ \circ JJ \circ \circ JJ \circ JJJ \circ \circ \circ$ which, by transliterating *J* as 1 and \circ as 0 becomes the binary number

11011001101110000, better known to us as decimal 111472, which could be written in unary base as a string of 111472 1s (see Fokker (1989) on how to obtain one-combinator bases).

It is worth emphasizing that the tricks of converting to combinatory logic, using the Schönfinkel reduction, converting the parenthesized *J* strings to binary numbers (and finally converting the binaries to unaries) are not essential for this undertaking. As is well known to students of logic, every formula (e.g. the kinds of formulas used in Montague's intensional logic) can be converted to a number by Gödel numbering³, and a Turing-computable and invertible mapping of natural language meanings to numbers is not hard to define.

But when we see decimal 69720375229712477164533808935312303556800 what is it exactly that we see? Well, we see $2^6 \cdot 3^4 \cdot 5^2 \cdot 7^2 \cdot 11 \cdot 13 \cdot 17 \cdot 19 \cdot 23 \cdot 29 \cdot 31 \cdot 37 \cdot 41 \cdot 43 \cdot 47 \cdot 53 \cdot 59 \cdot 61 \cdot 67 \cdot 71 \cdot 73 \cdot 79 \cdot 83 \cdot 89 \cdot 97$ which would be the Gödel code for [6, 4, 2, 2, 1, ..., 1] (a total of 21 1s). The problem is not that the translation back from the Gödel code to the *n*-tuple is not computable, but rather that it is not at all transparent, requiring a relatively powerful Turing machine to compute. For a translation, we would want compositionality, D2, which in turn requires a transparent machine, one that finds the boundary in the expression to make the first split into substrings A and B, and can recursively repeat the process for A and B. The real problem is that there is no boundary in the decimal number that the Gödel coding yields. Even if there were compositional boundaries in the original, these are washed out in the encoding process. Therefore, we replace the original desiderata D2 and D3 with D8 (mnemonic: $8 = 2^3$):

D8 The form \leftrightarrow meaning mapping should be maximally transparently compositional in both directions

D4 and D5 are part of the Katz & Fodor (1963) criteria that for many years were (and in many ways remain) the standard statement regarding the adequacy of any semantic theory:

A semantic theory describes and explains the interpretative ability of speakers by accounting for their performance in determining the number and content of the readings of a sentence, by detecting semantic anomalies, by deciding on paraphrase relations between sentences, and by marking every other semantic property or relation that plays a role in this ability.

³<http://bit.ly/3S0zTcX>

Over the years, as emphasis gradually shifted from lexical to compositional semantics, it became clear that these criteria are exceedingly hard to meet: D4 required some one-to-many mapping from form to “disambiguated language”, a technical device that (somewhat akin to universal phonetic realization) was never worked out in sufficient detail.

D4, together with D5, which is generally conceived of as a many-to-one mapping from different forms to the same meaning, jointly amount to assuming a form-to-meaning relation that is not functional in either direction. But the branching factors are very different: ambiguity is everywhere, synonymy is rare, in fact it is often claimed that no two natural language expressions are perfectly synonymous. This, if true, is highly problematic for Boolean connectives, where the logic creates synonymy: if something is translated as $p \wedge q$ it is perforce translated as $q \wedge p$ which then translates back to a non-synonymous natural language expression. This in fact happens: *I went home and had dinner* is not synonymous to *I had dinner and went home*.

This particular problem instance can be eliminated by insisting that the logic translation must also include an update of the temporal index that tracks event time, but the overall problem is much harder, since now all natural language tautologies must mean the same thing \top , and all natural language falsities must mean the same thing \perp . For this reason in Section 3 we will considerably relax D4 and D5: whatever is OTF, translation from it should not be more difficult than translation to it, and a full capture of ambiguity and paraphrase is impossible.

D6 is very ambitious, and is not shared widely among linguists, except those with a more semiotic bent. Clearly, there is such a thing as “the language of music”. It even has a written form, scores. But it is not clear that when we say that “music speaks to us” we mean the sequence of notes as traditionally depicted in scores: everyday experience shows that mechanical rendering of a score often fails to elicit the kind of emotional response that is triggered, according to many artists, precisely by those minute departures from the score that are the essence of human interpretation.

The same can be said for scientific theories: it is hard not to be touched by a deep sense of awe when understanding the Maxwell equations. But the awe is not a constitutive factor of the equations, and it is not clear how it is communicated to us, it just is there: we see the truth, and we marvel. And it’s not the truth, in and of itself, that triggers this response: we also see the truth of $3 = 3$ but we don’t particularly marvel.

This is not to say that music or science are somehow ineffable, impossible to explain, but without some notion of what is it that needs explication it is very hard to make progress on their semantics. With D1, as commonly understood,

this is much easier, because one of the several functions of natural languages is the interpretative function, to tell us things about the world, and model theory is an attempt to explicate how things are (or at least how things can be) in the world. If we had a substantive theory of being awestruck, “feeling great respect for the importance, difficulty, or seriousness of someone or something” (LDOCE, Procter 1978), we could make some progress on the semantics of these non-linguistic domains by leveraging the lexical semantics of words like *awe*, a matter we shall return to in Section 3.

Until now we have discussed a set of desiderata that any semantic theory should meet, selecting D1, D7, and D8 as our central desiderata. D2 and D3 are subsumed under D8, while D4, D5, and D6 are seen as *good to have*s, criteria that must be subordinated to the central ones. That failure to meet these three is not generally considered fatal is best seen from the widespread acceptance of MG and similar theories.

Perhaps the most important takeaway so far concerns D8, compositionality. The point of our “logic bestiary” examples is that semantics requires more than any old Turing-computable algorithm, it requires a specific mechanism of *decomposing* expressions into constituent parts, and computing the results based on the parts. Decomposition itself must be a simple operation, ideally expressed by a low-power Turing machine such as a finite state transducer that detects the constituent boundary. The overall semantics is obtained by (i) successive decomposition steps that together yield a parse tree of the input, and (ii) rolling back these steps by merging constituents. Proposals for these two steps go back as far as Wells (1947) and Knuth (1968) respectively. Whether the parse tree is strictly binary or not, whether it can contain discontinuous (gapped, interleaved) constituents are questions of great technical importance, but compositionality can be achieved either way.

This leaves us with one central desideratum we have not touched upon so far, *learnability*. In theory, the interpretation mechanism can be given externally (e.g. as a lex/yacc parser), but in practice we would prefer the entire algorithm to be learnable, ideally from positive data alone. Whether this is just good to have, or a non-negotiable desideratum as urged by Chomsky (1965) is hard to say, but one thing is clear: so far, all successful learners are *supervised*, requiring labeled data. These include *self-supervised* techniques where the labels are generated by simple automated methods from initially unsupervised data (raw text). At the price of demanding orders of magnitude more data than encountered by human language learners during language acquisition, such self-supervision is used to great effect in Large Language Models.

The difference between the purely symbolic algorithms, such as lex/yacc parsers commonly developed for computer languages by their creators on the one hand, and the machine learned algorithms on the other, generally boils down to a difference between the use of symbolic debugging versus optimization. The learning algorithm closest to the former is “principles and parameters” learning as proposed in Chomsky & Lasnik (1993), which has many precursors in formal language theory (for a survey, see Angluin 1980).

Since Large Language Models (LLMs) are far more successful in acquiring syntax than any symbolic approach, the hopes of acquiring semantics by symbolic means are rather dim, especially as compositionality requires the acquisition of a system that creates the parse tree, i.e. the acquisition of at least rudimentary syntax capabilities. Therefore, making the system optimization-friendly appears as a central desideratum. Since optimization is performed by gradient descent, this requires a system, any system, that states the problem in a framework where gradient descent is feasible, i.e. a smooth system where derivatives can be computed. Whether derived from a learnability desideratum or seen as a practical necessity, we have

D9 The problem statement must be embedded in a differentiable setup

One of the key inventions that powered the LLM revolution was enabling gradient learning by means of a new semantic structure, *word vectors* (Schütze 1993, Collobert et al. 2011). This is by no means the only relevant invention: we already mentioned *self-supervision*; and we should mention at least *byte pair encoding* (Gage 1994); *sequence to sequence transformation* (Sutskever et al. 2014); and *attention* (Vaswani et al. 2017). By replacing the discrete tree structures used since Katz & Fodor (1963) for encoding the meaning of lexical items by vectors in n -dimensional space where partial derivatives can be taken, learning based on optimization became possible. It is worth emphasizing that the resulting continuity/differentiability fully applies to the terminal nodes in the representation of lexical meanings, which were conceptualized as discrete (typically, binary) features by Katz and Fodor, and rightly objected to as “atomization of meaning” by Bolinger (1965).

In the next Section we turn to the vector-based, and thus optimization-friendly language system, with special emphasis on meeting the desiderata by a minimal system from this class of models. In fact, the system is so skeletal that the vectors can be computed just by solving a system of equations, a goal that makes particular sense for “low density” languages where training data is in short supply.

3 Hypergraphs and their linearization

In what follows, we take the system of polytopes⁴ induced by word vectors as our starting point (Kornai 2023), and begin with the trivial observation that the *thought vectors* of LeCun et al. (2015), which are intended as semantic representations of the (already spoken part of) sentences and larger discourses, appear in the same space. This takes care of D7, which asks for a style of representation that is common to subword units (morphemes, or the bytetrain-like units used in the WordPiece algorithm of Wu et al. 2016), phrases, sentences, and even larger units. In this system, non-compositionality corresponds to *subdirect* products, and compositionality appears as a special case, *direct* products (Kornai 2010) – the difference is illustrated in Figure 2.

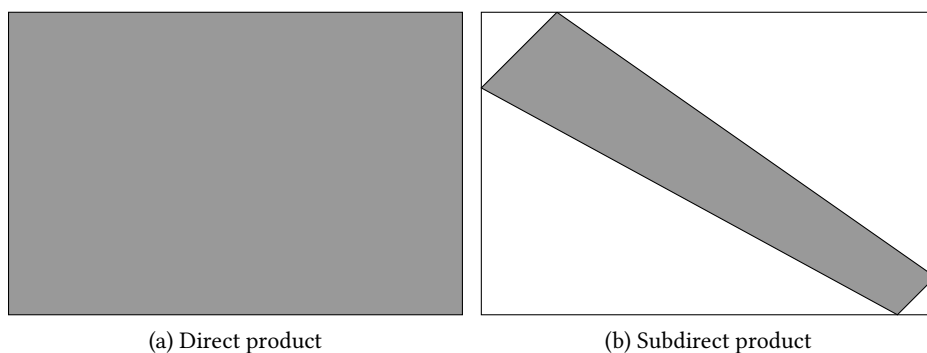


Figure 2: Direct and subdirect products of the same two intervals $[0, 12]$ and $[0, 8]$

The subdirect product⁵, standardly defined as a *subset of the direct product* satisfying projection requirements is not unique: there can be many subsets of the direct product that project onto both components. This means that the semantics itself is underdetermined, but this is only to be expected in cases like noun-noun compounding. Whatever portion of the semantics is rule-governed is captured, e.g. that in N-N compounding we have ‘ N_2 that is V-ed by N_1 ’ with the V indeterminate: ladder *made of* rope, slaughter *undergone by* man, tube *used for* test (Kiparsky 1982), the non-compositional part is admitted as such. This seems to be the right approach not just for morphology, but also for the grey zone of *constructions* between the purely morphological and the purely syntactic such as *NP of NP* studied in Berkeley Construction Grammar⁶ (Kornai 1988), taking care of our desideratum D7.

⁴<https://en.wikipedia.org/wiki/Polytope>

⁵https://en.wikipedia.org/wiki/Subdirect_product

⁶<http://bit.ly/4cCkLe5>

What are, then, the non-negotiable elements of vector semantics? One, perhaps the most important one, is the notion of containment, *IsA*, which we see as essential for the reconstruction of Aristotelian *genus*. Whatever definition we provide for *dachshund* or *labrador*, the first thing in the definiens will be *dog*. Given that we use polytopes (polyhedra-line n -dimensional regions) around the word vectors, *IsA* comes for free as the set-theoretical inclusion “ \subset ” relation. This works well for ordinary (intersective) adjectival modifiers as well: a *brown dog* is in the intersection of the *brown* and the *dog* polytopes. (For non-intersective adjectives like *former*, see Chapter 3.2 of Kornai 2023).

The method of assigning semantics to *Kim is a donkey* by leveraging set-theoretical containment cannot be directly generalized. Clearly, there is nothing in set theory that would directly work for *Kim has a donkey*, but the underlying idea of taking a relation, in this example the possessive relation *HAS*, and using that for assigning meaning, is solid. (*HAS* can be further subdivided into inalienable and ordinary possession, but we will not pursue this matter here.) There remains one technical difficulty: however the language signals the distinction, *John ate the fish* and *The fish ate John* should not be treated as synonymous. We use *SUBJECTOF* and *OBJECTOF* for the disambiguation. These are good candidates for universality, even in languages where the distinction is made in absolutive/ergative terms.

With this, we are done – we don’t need further disambiguators (deep cases, thematic roles or proto-roles, etc.) to get to ditransitive or even higher arity predicates, since these can be obtained by classic techniques of meaning decomposition that go back to generative semantics (Kornai 2012). (The *4lang* system writes =*agt* and =*pat*, but we could have written “1” and “2” as well – the only theoretical claim here is that there is no “3” required.) The representation structures we obtain are best depicted as hypernode graphs that can contain other such graphs as nodes (but not as edges). These should be familiar from the Resource Description Framework that is standard on the WorldWideWeb.

It is easy to check that the system presented here meets our desiderata *D1* and *D8* as well, so our work is done. Readers interested in how the system can be extended, without adding further operators, to issues of temporal and spatial semantics, indexicals, negation, quantification, probability, modality, gradience, implicature, and other issues generally considered relevant for semantics are advised to look at Kornai (2023). But one word of caution is in order: not having further operations is not the same as not having further primitives.

The *4lang* system actually treats a handful of binary relations *AT*, *BETWEEN*, *CAUSE*, *ER*, *FOLLOW*, *FOR*, *FROM*, *HAS*, *IN*, *INS*, *ISA*, *LACK*, *MARK*, *ON*, *PARTOF*, *UNDER* as primitives (and makes the claim that all others are derivable). These cor-

respond to matrices, rather than vectors. Remarkably, what traditional syntax treats as higher order operators, quantifiers in particular, will require only vectors, rather than full matrices: the central example is the generic quantifier *gen*, which simply corresponds to the n -dimensional vector $(1/n, 1/n, \dots, 1/n)$ (for details see Kornai (2023) Chapter 4.5). The bulk of the primitives are unaries (vectors) appearing in a system of mutually constraining definitions, and this includes most verbs that can have an optional object like *eat* as well.

With *eat* it is reasonably easy to see how one can define it in terms of the Longman Defining Vocabulary ‘to put food in your mouth and chew and swallow it’ and the process of turning this into a λ lang clause can be automated (Recski 2016) to yield $=\text{agt cause_} \{=\text{pat in mouth}\}$, *swallow*, $\langle =\text{pat}[\text{food}] \rangle$, $\langle \text{bite}/1001 \rangle$, $\langle \text{chew} \rangle$, $=\text{agt has mouth}$, which uses an even smaller defining vocabulary of 739 elements (including the 16 binaries).

Arguably *eat*, if not a universal semantic primitive, is at least very close to being one, and clearly it is a “simple” word (Kornai 2021) that comes very early in language acquisition. Our earlier example, *awe*, is clearly far from the simple/basic layer of the vocabulary, but the same method remains applicable: take the LDOCE definition, in this case ‘a feeling of great respect and liking for someone or something’, normalize the syntax, and reduce further until only the λ lang primitives remain. We begin with *for someone or something* and replace it by $=\text{pat}$. *great* and *liking* are defined, *great* as *big* and *like* as *feel* $\{=\text{pat}[\text{good}]$, *good for* $=\text{agt}\}$. For *respect*, we have to go back to LDOCE to obtain “admire”, for which we obtain ‘to look at something and think how beautiful or impressive it is’. The process goes on, but for *beautiful* we obtain “extremely attractive” and with *attract* we terminate at $=\text{agt cause_} \{=\text{pat want } \{=\text{pat near } =\text{agt}\}\}$.

This may appear tedious, but eventually all non- λ lang words are eliminated, since the system was constructed from the Longman Defining Vocabulary by systematic elimination (Ács et al. 2019) until a feedback vertex set⁷ is obtained. The price of the termination guarantee is that the resulting set is considerably larger than the system of Natural Semantic Metalanguage (NSM; Wierzbicka 1992, 1996, Goddard 2002), which in many ways served as an inspiration. But λ lang both has a formal syntax and guarantees that all words not defined in the core are definable by it via LDOCE, whereas NSM uses an informal (English) syntax, and has no guarantees that words outside the core are actually definable as NSM stanzas.

As for minimality, we make no claim that the set of λ lang primitives is truly minimal, just that by systematic reduction of the entire English vocabulary we arrived at a stage where we see no further reduction possibilities. This does not

⁷https://en.wikipedia.org/wiki/Feedback_vertex_set

mean that for other languages no further reductions would be possible, and it would be an interesting research program to (i) harden NSM syntax until it becomes machine-parsable and (ii) define the 4lang primitives in terms of the NSM primitives. Whether this is possible remains to be seen, but our system already provides an upper bound on the dimension of the vector space we use for modeling semantics.

4 Conclusions

Minimality requires thrift both in the number of operations and in the number of primitives manipulated by these. To maintain compositionality in both directions, the “bestiary-style” minimalism of (Gödel) numbering has to be sacrificed for more transparent operations. Of particular interest is the case when the objects manipulated are vectors and matrices in finite-dimensional Euclidean space, since these can be acquired gradually, by optimization techniques that change the vectors only a little bit as new learning data becomes available, rather than by huge and unpredictable discrete steps that require a complex system of inborn directives.

As for the primitives, our current system is likely overcomplete⁸, at least as far as the vectors (unaries) are concerned, though we seem to approach the limits of reducibility for the matrices (binary relations) used. Remarkably, it is not the verbs, transitive, ditransitive, or even higher arity, that require departure from unary relations, but the prepositions expressing spatial relations, AT, BETWEEN, FOLLOW, FROM, IN, ON, UNDER, for which we must assume a prepositional subject and a prepositional object, the comparative ER, the negative LACK, and a few conceptual relation markers, quite often expressed by cases, such as CAUSE, FOR, HAS, INS, and PARTOF. Pride of place goes to ISA, essential for taxonomic organization, and MARK, denoting the relation between the two parts of the Saussurian sign.

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⁸<https://en.wikipedia.org/wiki/Overcompleteness>

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Chapter 14

Investigating grammatical borrowing in Mosetén through historical sources

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Using historical sources (1804–1913), this paper investigates the influence of Spanish on the grammar of modern Mosetén, an indigenous language spoken in the foothills of the Bolivian Andes and the adjoining Amazon basin. Focusing on the categories of gender agreement and phrasal word order, I argue that modern Mosetén gender agreement follows Spanish patterns, while word order rules are in part affected by intensive language contact with Spanish. Speaker variation, as observed in modern Mosetén, appears to be present in the historical data already. Yet, changes in use-patterns and frequencies may be the reason for the extension of some grammatical categories, meaning modern Mosetén grammar is closer to Spanish than the language observed in the original historical sources.

1 Introduction

Mosetén is a Mosetenan language (cas, ISO 639-3) spoken in the tropical region of the lower foothills of the Bolivian Andes. The language family consists of just three closely related and mutually intelligible varieties. Mosetén of Covendo and Mosetén of Santa Ana, both spoken in the foothills of the La Paz Andes, are highly endangered and only have a few hundred speakers altogether. Chimane, spoken in the adjacent lowland Beni area, has a growing number of speakers, with various estimates of 5000–8000 speakers in total. Despite suggestions of relationships between Mosetenan and other South American languages, these have so far not been conclusively established and this small language family is still considered unrelated to other languages (cf. Sakel 2004). Despite the absence of



genetic relationships, the Mosestenan varieties did not exist in isolation and have once been in close contact with a range of languages in the region, leading to loanwords and other contact phenomena, which are likely the reason for some of the superficial similarities with other language families. Historically, Mosestenan would have been in contact with indigenous languages such as Quechua, Uru-Chipaya and Tacanan. Since the middle of the 1950s, contact has mainly been with Spanish. This is not surprising, as Spanish is used in most official, and increasingly also personal, domains among many indigenous groups of Bolivia. In the Mosestén situation, Spanish influence has increased significantly in conjunction with better accessibility to the area, and subsequent heavy migration of indigenous farmers from the highlands in search of better living conditions.

2 History and sociolinguistic profile of the language

What we know about the history of the Mosestenan languages is largely what we can deduce from synchronic sources, such as variation between the varieties and speaker differences across generations. For example, Chimane has experienced less heavy language contact with Spanish. Dialectal differences aside, it can serve as a guide to the structures that have undergone changes in Mosestén due to contact with Spanish.

When I started to work on Mosestén in the 1990s, most Mosestén speakers would predominantly use Spanish in their day-to-day interactions, with Mosestén restricted to a few informal domains. For my grammar of the language (Sakel 2004), I worked closely with older generations that used Mosestén as their primary language, preserving some of the grammatical structures that many young speakers had replaced with largely Spanish patterns. For example, older speakers would regularly use feminine pronouns when referring to groups of mixed-sex people (1). Younger speakers generally preferred the use of the masculine in the same situation (2) – modelled on the Spanish template (3):

- (1) Mö'-in
3F.PL
'they, e.g. father and mother' (older Mosestén of Covendo speakers).
- (2) Mi'-in
3M.PL
'they, e.g. father and mother' (younger Mosestén of Covendo speakers).
- (3) Ellos
3M.PL
'They, e.g. father and mother' (Spanish)

3 Language contact

The above changes to gender agreement can be subtle and difficult to identify as potential Spanish influence without an in-depth analysis of the patterns of the language. This is because Mosetén lexical elements are used to model Spanish patterns, without the direct loan of Spanish elements (Matras & Sakel 2007a,b). The other type of loan, matter borrowing of Spanish morphophonological elements, is also attested in modern Mosetén and is very common. These loans are often much more obvious, as they stand out as Spanish words. However, some of these loans have been adjusted phonologically, e.g. Spanish *hasta* ‘until’ is pronounced *ashta* in Mosetén. Matter loans can go hand in hand with borrowed patterns. For example, Mosetén has borrowed many Spanish function words, such as coordinators, subordinating conjunctions, markers of time and space, discourse markers and delimitation markers that are borrowed together with their respective syntactic patterns (Sakel 2007a,b):

- (4) Its-näjä’ yi-sin’ ke jam-ra’ karij-tya-kha’
 DEMM-FOC say-1.PL.OBJ that.E NEG-IRR work-APPL-1PL.INCL.SBJ
 ‘This one (now) told us that we all wouldn’t be working.’

(4) shows a sentence structure modelled on Spanish, using the conjunction *ke* (a direct Matter loan from Spanish *que* ‘that’) between the two clauses. In the language spoken by elders, complement clauses can be expressed in a range of different ways, the most typical way being the addition of a clitic *-dye*’ to the verb of the subordinate clause:

- (5) Yäe ködye-ye sob-a-k-dye’ öi-yä’ phen.
 1SG beg-1SG/2SG visit-V-ANTIP-NMLZ DEM.F-LOC woman
 ‘I beg you to visit this woman.’

4 Data on the language

Mosetén and Chimane are relatively well described, with grammars and further analyses of the varieties, most notably Gill (1999), who wrote various manuscripts on Chimane, and my own work on the grammar of the Mosetén and later Chimane, first published in the early 2000s (Sakel 2004).

Yet, as for many other indigenous South American languages, we have very little historical information for Mosetén. The first acknowledgments of the language came from missionary sources, e.g. the Mosetenes (then referred to as

Amo) were mentioned in 1588 (Métraux 1942), with various missionaries more or less successfully settling in the area and subsequently noting down some information on the language.

The first language data are presented by Andrés Herrero, a Franciscan missionary settling in the region in the early 1800s. Upon his return to Europe in 1834, he put together a prayer book on the language.

The Franciscan missionary Benigno Bibolotti stayed in the village of Covendo in 1857. His notes on the language were published and analysed by Rudolph Schuller, who published a basic grammatical description of the language based on Bibolotti's original data as an "introduction", alongside Bibolotti's manuscript (Bibolotti & Michelson 1917, Schuller 1917).

There are a number of other collections of information on the language, most notably a collection of data by a renowned Swedish adventurer, Erland von Nordenskiöld, who visited the region on an excursion in 1913, when he spent a short time in the Masetén area and got a speaker of the language, Tomas Huasna, to write down three short stories for him, which are preserved in Nordenskiöld's diary held at the archive of the Etnografiska Museum in Göteborg, Sweden. Various publications by Nordenskiöld reference these stories (Nordenskiöld 1924). These are the first native-speaker first-hand language data we have of the language.

During my fieldwork on Masetén in the 1990s and 2000s, I worked with Tomas Huasna's grandson, the late Juan Huasna, who remembered Tomas as a modern, forward-looking man who had spent a great deal of time working with the local missionaries. The missionaries had taught him to read and write in Spanish and, to a certain degree, Masetén, and he was helping with the translation of prayers and bible portions.

5 Lexical borrowing in the historical data

A number of lexical loans are attested in the historical data. Loan words in Herrero's text are mainly of a religious nature, e.g: *santo* 'holy', *reino* 'kingdom', *gracia* 'grace', *salve* 'hail', *virgen* 'virgin' and *testimonio* 'testimony'. Some of the borrowed elements are integrated into Masetén structures, for example turning them into Masetén verbs, which are obligatorily marked by verbal affixes: *misa-arai* (attend.mass.E-verb, 'to attend mass'), *confes-arai* (confess.E-verb, 'to confess'), *comulga-arai* (commune.E-verb, 'to commune'). Huasna uses the Spanish loan *semana* 'week' in his text, capturing a western concept of timekeeping. All loans are purely lexical, and there are no matter loans of Spanish subordinators together with their structures in the historical data, despite their frequency in modern Masetén.

6 Comparing the historical data with modern varieties of the language

It is impossible to rule out any grammatical borrowing in the historical data. Pattern borrowing is often linked with a degree of bilingualism. It is unclear how much Mosetén Bibolotti and Herrero acquired. They were keenly working on the language, so may have had a certain level of command. Huasna is likely to have been fairly proficient in Spanish, working with the missionaries on a daily basis and assisting in their tasks. Some grammatical interference may have happened due to Mosetén speakers imitating the missionaries' imperfect learning of their language.

While not ruling out the possibility of changes in the language due to contact with Spanish or other Romance languages such as Italian (Bibolotti was a native speaker of Italian), the old language data is most likely able to give us an insight into a much earlier stage in the language contact journey. Thus, the variation we see in modern varieties of Mosetén across speakers of different ages, environments, and levels of bilingualism with Spanish would be expected to be different in earlier stages of that contact journey, giving us an insight into possible changes due to language contact in modern Mosetén.

In Sakel (2007b) I identified two areas of grammar that had undergone pattern changes due to the influence of Spanish in modern Mosetén: 1. changes in the use of gender: the unmarked gender changing from feminine to masculine and 2. changes in the word order within the NP: modifiers (esp. adjective) – head noun.

I will test to what degree my assumptions based on the comparison of synchronic data in the language are backed up by the historical data, as well as adding some information from Chimane.

7 Gender agreement

As shown in (1) and (2) above, there is a difference in how older and younger speakers of modern Mosetén attribute gender to mixed-sex groups: younger speakers typically model the Spanish pattern of using the masculine gender, while some older speakers with less frequent exposure to Spanish would use the feminine gender in the same situation.

Herrero presents a number of examples of feminine forms used in situations with male protagonists (head nouns italicized, agreement markers in bold):

- (6) *Dios momo cogchi-cam eraise-te mi.*
 God.E only.F heart-LOC love-3M.OBJ 2SG
 ‘You love only God from your heart.’
- (7) *Dios Mumu, Dios Aba-mu, Dios Espiritu Santo.*
 God.E Father God.E son-his God.E holy.E spirit.E
 Chivin **munsi**, **yeret momo Dios** ato.
 three people.F one.M only.F God yet
 ‘The father, the son and the holy spirit – three people, but only one God.’
- (8) *Jesu Chisto tim-mo.*
 Jesus Christ name-POSS.F
 ‘His name is Jesus Christ.’
- (9) *YäeAchii-ti munsi uñan arai inca-Ø-in?*
 bad-POSS.M people.F where IRR go-M.SBJ-PL
 ‘Where do the bad people go?’

In (6) and (7) *Dios* ‘God’ appears with the form *momo* ‘only’ and *Jesu Chisto* ‘Jesus’ in (8) appears with the related possessive pronoun *mo*. In modern Mose-tén, these forms are feminine and have the masculine equivalents *mumu*’ and *mu*’ (*mimi*’ and *mi*’ in some varieties of Mose-tén). Are these representations of God and Jesus expressed as overtly feminine? God and Jesus are theoretical concepts, so the use of feminine as a generic gender may have been appropriate in this case, despite their depictions as male protagonists. Another possibility is that *momo* and *mo* are generic, underlying forms that could be used in both masculine and feminine environments. A third option is that Herrero made a mistake when noting these down. However, there appears some consistency in the use of feminine agreement in these cases, which makes it less likely to be a mistake. Note the use of a masculine cross-reference marker *-te* ‘3rd person masculine object’ in (6) and the masculine form of the numeral *yeret* ‘one’ in (7), which refers to God as a masculine entity. Thus, masculine gender agreement is used at the same time as the feminine forms. We see that masculine gender agreement is present elsewhere, for example in the cross-reference marking referring to a masculine subject (9). Likewise, Bibolotti has examples of the use of *momo* in environments where reference is to antecedents that are not exclusively feminine:

- (10) *Eñe-ra Cui tsuñ momo.*
 like-IRR self we only.F
 ‘just like ourselves’ (referring to the people)

- (11) Dojit-si Aua-mu
 God-POSS.F son-3M.POSS
 ‘God’s son’ (lit. ‘of God, his son’)

There are various examples of the type given in (10), where *momo* is used with masculine or mixed-sex antecedents. (11) shows an example of the use of feminine agreement with masculine antecedents. In this case, the possessive marker on the head *Dojit* ‘God’ is in the feminine form, while the possessed entity *Aua* ‘son’ appears with a masculine possessive marker. The texts written by Huasna are consistent with what we find in modern Mosetén, using masculine agreement forms of *momo*’/’*mumu*’ with masculine heads:

- (12) Pfai-tiiñ tac-mumu caca-tiiñ
 jump.on-VIO.M.SBJ throw.to.ground-just.M pick.up-VIO.M.SBJ
 ‘He (the jaguar) jumps on him violently, just throws him to the ground
 (and) picks him up.’

Indeed, we even find the use of masculine gender agreement when referring to various people of mixed (or unclear) gender, despite there being variation in modern Mosetén (cf. examples (1) and (2) above):

- (13) rre mu-che jicai-Ø-iñ
 all up.there.M go-M.SBJ-PL
 ‘they all went up there.’ (context: men, women, big children, small children)

In summary, the picture presented for gender agreement is somewhat complex. While Huasna appears to display a number of characteristics that are typical of modern speakers with heavy exposure to Spanish, Bibolotti and Herrero show examples of feminine forms used in masculine or mixed-sex environments. Provided these L2 speaker missionaries did not make a mistake, feminine forms are not just used as neutral gender forms in mixed-sex environments, but appear to also be with some masculine antecedents – sometimes together with masculine agreement forms referring back to the same antecedents. This type of “mixed” gender marking is not found in modern Mosetén. However, it exists – in part – in the closely related language Chimane, which has experienced considerably less contact with Spanish. In Chimane, *momo*’ ‘only, F’ is generally used in NPs with masculine antecedents (Gill 1999).

The likely explanation is that *momo*’ ‘only’ – a reduplicated form of the third person personal pronoun *mo*’ ‘she’ – was originally used more generally across

the language and could be applied to both masculine and feminine environments. Due to increasing pressure from Spanish, a masculine gender form *mimi'*/*mumu'* appeared in analogy with *momo'*, which is already present in the first-hand data presented by Huasna in 1913.

8 Word order

Another area of grammar that is often affected by language contact through Pattern changes without overt matter borrowing is word order. Looking at the phrasal word order in modern Mosestén, we find variation: both orders N-ADJ and ADJ-N are accepted (Sakel 2004: 103). There is a tendency related to the animacy of the head, namely inanimate heads are typically preceded by an adjective ADJ-N, while animate heads are typically followed by the modifier N-ADJ, though the reverse order ADJ-N is possible as well. In Spanish, when the adjective describes a quality of the head, the order is typically N-ADJ, as opposed to ADJ-N to express a level of emphasis or appreciation of the head. While there are not many examples of phrasal word order in Herrero, those that appear are ADJ-N (repeated from (9)):

- (14) **Achii-ti mumsi** uñan arai inca-in?
Bad.POSS people.F where IRR go.M.SBJ-PL
'Where do bad people go?'

In the historical data, Bibolotti's manuscript sets out instructions for other missionaries to understand the intricacies of the language. He translates Spanish phrases into Mosestén, at first giving a literal translation and then indicating the preferences of the speakers he worked with. In this way, ADJ-N word order is consistently "corrected" in Bibolotti's data (the relevant NPs are highlighted in bold):

- (15) Literal:
Chinca peaqui **peage achis**, vori Soyo cañ cui si cotchi!
that.who speak speech bad call demon? in own heart
'The one who speaks dirty words calls the devil in his heart.'
- (16) Corrected:
Chinca peaqui **achis peage**, vori Soyo cui si cotchi cañ!
that.who speak bad speech call demon? own heart in
'The one who speaks dirty words calls the devil in his heart.'

(17) Literal:

Ges **soñi achitchit**, ere coi ueñege.
 for man bad.bad all appear dream
 ‘To the corrupt man, everything seems like an illusion.’

(18) Corrected:

Achitchi-ges soñi, ere coi ueñege.
 bad.bad-for man all appear dream
 ‘To the corrupt man, everything seems like an illusion.’

Thus, in this case the order of *achis* ‘bad.F’ and *peacge* ‘word, story (F)’ is “corrected” from the literal translation N-ADJ to ADJ-N. Likewise, the animate head noun *soñi* ‘man’ and the adjective *achitchit* ‘very bad’ are presented as following a preferred ADJ-N order. The element *-ges* ‘for’ is a clitic in Mosetén, as opposed to a preposition in Spanish, as given in the literal translation. While the literal translations in Bibolotti’s times did not seem to be acceptable to the speakers he was working with – or at least those speakers had a clear preference for the ADJ-N word order – in modern Mosetén both phrasal word orders are often acceptable and commonly used. Finally, Huasna has no clear examples of noun phrase word orders. In two cases, adjectives appear with nouns in the order N-ADJ, but they are divided by commas. Thus, it is unclear whether this is the word order N-ADJ, or whether Huasna added the adjective after the noun as a form of afterthought:

(19) Oi **Pfeyacej-iñ, Poroma-si**

DEM.F story-PL old.POSS
 ‘these stories, the old ones’

(20) jique muñthi-iñ, pfeñ-iñ, **ñañathi-iñ, chi-dere-si-iñ,**

PST man-PL woman-PL boy-PL also-big-POSS.F-PL
chi-chubo-si-si-iñ,
 also-carried-POSS.F-POSS.F-PL

‘and the men, the women, the boys, the big ones and also the ones carried (by their mothers)’

In summary, we may be seeing a loosening of the word order in Mosetén. While Bibolotti indicates a clear speaker preference for ADJ-N order, modern Mosetén allows a range of phrasal word orders. This looks to be closer to the Spanish pattern, while not being a carbon copy. This does not rule out language contact, as it is commonly attested in contact situations that structures resulting from language contact are not exact copies of the source language, but may undergo partial changes (Heine & Kuteva 2005). Yet, both phrasal orders also exist

in Chimane, which means that the loosening of the word order may either not be entirely due to language contact or Chimane may have undergone the same contact.

9 Conclusion

It can be difficult to attest to language contact influence at the level of pattern borrowing in grammar. We may be able to get an insight into this when we look at synchronic data of speakers of different characteristics and varieties of a language or closely related languages with different language contact histories and levels of exposure to the contact language. Yet, historical data on a language can give us further insights into contact histories, being able to test a range of scenarios.

While historical data on a language can be helpful, we have to accept various insecurities: did the L2 speakers get it right? Did some of the L1 speakers already display considerable amounts of Spanish influence, e.g. Huasna, whose language seems much closer to some of the more progressive speakers of modern Mosestén?

The picture of Pattern borrowing becomes more complex by looking at the historical data, rather than supporting my original hypothesis of contact-induced changes modelled on synchronic language use. While overtly many modern Mosestén structures are modelled on Spanish patterns, often associated with lexical loans, the detailed analysis of gender agreement and noun phrase word order is only in part explicable as a result of language contact.

Abbreviations

Abbreviations follow the Leipzig glossing rules. Additional abbreviations:

- E Spanish loan
- VIO Marker for violence

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Chapter 15

Modern language models refute Chomsky's approach to language

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Modern machine learning has subverted and bypassed the theoretical framework of Chomsky's generative approach to linguistics, including its core claims to particular insights, principles, structures, and processes. I describe the sense in which modern language models implement genuine theories of language, and I highlight the links between these models and approaches to linguistics that are based on gradient computations and memorized constructions. I also describe why these models undermine strong claims for the innateness of language and respond to several critiques of large language models, including arguments that they can't answer "why" questions and skepticism that they are informative about real life acquisition. Most notably, large language models have attained remarkable success at discovering grammar without using any of the methods that some in linguistics insisted were necessary for a science of language to progress.

1 Introduction

After decades of privilege and prominence in linguistics, Noam Chomsky's approach to the science of language is experiencing a remarkable downfall. The story is, in part, a cautionary tale about what happens when an academic field isolates itself from what should be complementary endeavours. Chomsky's approach and methods are often argued to be problematic (e.g. Harris 1993, Pullum 1989, Behme 2012, Postal 2012, Behme 2014), but it is yet to be widely recognized just how the underlying ideas have been undermined by recent computational advances.



The state of the art in virtually all computational language tasks makes use of deep neural networks (LeCun et al. 2015), often neural networks called *transformers* (Vaswani et al. 2017, Brown et al. 2020, Bommasani et al. 2021). These are models of text that are trained on huge datasets of internet-based text to predict upcoming linguistic material (often at a sub-word but supra-character encoding). Several advances have allowed these models to become wildly successful. First, we have become able to train them on massive data sets. This is in part due to advances in computing (e.g. computing derivatives in arbitrary models) and in part the availability of huge collections of text from the internet. A typical language model might be trained on hundreds of billions of tokens, estimated to cost millions of dollars in energy alone. Second, the model architecture flexibly deals with nonlocal dependencies and allows the prediction of a word to potentially depend on material that is far away. The key consequence is that leading models are able to generate not only grammatical sentences, but entire discourses, scripts, explanations, poems, etc. This pursuit has been led in recent years by AI companies (Ahmed et al. 2023).

A nice example can be provided by the following, produced by ChatGPT when prompted with only the text in italics. All examples in this paper show the first response generated by ChatGPT to these queries by the author in early 2023. Good queries often focus on weird or unusual requests in order to ensure that the output is well outside of the training set.

Write a short story explaining how an ant could sink an aircraft carrier.

Once upon a time, in a far-off land, there lived a colony of ants. These ants were not like any ordinary ants, they were highly intelligent and had the ability to communicate with one another telepathically. They had been observing the humans and their ways for a long time, and they had become fascinated by the massive aircraft carriers that the humans built and sailed across the seas.

One day, the leader of the ant colony, a wise old ant named Alex, had an idea. He called all the ants together and said, “My friends, we have the ability to think and communicate like the humans do. We can also work together as a team to achieve great things. I propose that we build a ship of our own, a ship that can sink the human’s aircraft carrier.”

The other ants were shocked and amazed by Alex’s proposal, but they were also excited by the idea. They immediately set to work, gathering

twigs and sticks from the forest floor, and using their own tiny bodies to weave them together.

Months passed, and the ants' ship grew bigger and bigger. It was not as big as an aircraft carrier, but it was large enough to hold many ants and was heavily armed with miniature torpedoes and cannons.

Finally, the day arrived when the ant ship was ready. The ants climbed aboard and set sail towards the nearest aircraft carrier. As they approached the carrier, the humans were shocked to see such a small ship coming towards them. They laughed and jeered, thinking that the ants' ship was no threat to them.

But the ants were not to be underestimated. They had been studying the human's ship, and they knew its weak spots. As they neared the carrier, they launched their miniature torpedoes, which hit the carrier directly in its vulnerable areas. The carrier began to sink, and the humans panicked.

In the end, the ant ship emerged victorious, having successfully sunk the massive aircraft carrier. The ants returned home to their colony as heroes, and their story became a legend that was passed down through the generations.

And the human learned to never underestimate the power of collective intelligence and unity.

It is important to note that the story above is a fictional tale and it is impossible for an ant to sink an aircraft carrier, as in reality the size, strength and weapon capability of an aircraft carrier would far exceed the capability of a group of ants.

Ignore whatever theoretical persuasions you have for a moment and just recognize how *remarkable* that response is. A coherent and grammatical story unfolds from a simple prompt, even though the requested scenario strains plausibility and probability. The response tackles the core essence of the question to provide a compelling story about how an ant *might* sink an aircraft carrier. The answer is outside of the training set. In fact, after "Once upon a time, in a far-off land, there lived a colony of ants", a Google search returns no matching strings on the entire internet.

Anyone who has dealt in simpler language models like n -grams or context-free grammars will be impressed that this model is capable of incorporating not just longer dependencies within sentences, but across sentences. It also creates

complex structures, with e.g. multiple embedded clauses, prepositional phrases, conjunctions, etc; it uses pronouns, determiners, quantifiers, adjectives, etc. in realistic ways; this and similar models have few apparent problems with agreement or pronoun reference (Gulordava et al. 2018, Goldberg 2019). Any one of these abilities has been the topic of countless dissertations in linguistics. Beyond that, such models succeed on tasks that formal linguistic theories haven't yet dreamed of – like interpreting the semantics of the words in question and maintaining not just coherent sentences but entire coherent discourses. Such models exhibit over 90% accuracy in Winograd-style questions (Kocijan et al. 2020, 2022), which require interpreting language in world contexts (e.g. “The trophy doesn't fit in the brown suitcase because it is too large. What is too large?”). What is clear is that these models *use language in a way that is remarkably human* (Mahowald et al. 2024).

This kind of example harkens back to Turing (1950), who considered asking an artificial intelligence to write a sonnet on the topic of “The Forth Bridge”,¹ and suggested that success in solving such tasks would demonstrate many key aspects of human-like intelligence. Contemporary language models can do much more – they can write raps, explain things, imagine conversations, create a mash-up script of Seinfeld and Star Trek, etc. They are aware of even sub-word properties like rhymes:

Come up with a limerick about syntactic island constraints in linguistics.

On syntactic islands, we must be precise,
For constraints on movement can be quite nice.
But when the constraints are breached,
Our grammars are impeached,
And our linguistic analyses pay the price.

And they are aware of style:

Explain the fundamental theorem of arithmetic in the style of Donald Trump.

Well, let me tell you folks, the fundamental theorem of arithmetic is a really great theorem, it's tremendous, and it's all about prime numbers.

¹According to Wikipedia, a cantilevered rail bridge from 1890 in Scotland that was voted in 2016 to be Scotland's greatest human-made wonder, apparently beating out Ewan McGregor.

Believe me, I know a lot about prime numbers. They're very important, very powerful, very beautiful numbers.

Now, the fundamental theorem of arithmetic tells us that every whole number greater than 1 can be expressed as a unique product of prime numbers. That's right, every single number can be broken down into its prime factors, and there's only one way to do it. It's like a code, a secret code that only primes can unlock.

This is a really amazing theorem, folks, and it's incredibly useful. It's used in cryptography, number theory, and all sorts of other fields. It's a cornerstone of mathematics, and it's something that we should all be very proud of.

So, in conclusion, the fundamental theorem of arithmetic is a fantastic theorem, it's tremendous, and it's all about prime numbers. I love prime numbers, they're the best. And this theorem tells us that every number can be broken down into primes in a unique way. It's a great thing, believe me

Note that this specific example was not in the model's training set – there is no possibility that Trump understands prime numbers. However information about the fundamental theorem of arithmetic was, as was the speech style of Donald Trump. “Believe me, I know a lot about prime numbers” is probably not something Trump has actually said, but certainly something he *would* say.

Examples like these show why dismissals from cognitive scientists like Gary Marcus² that the models are just the same as “autocomplete” systems on your phone are far too shallow. The model is able to put together things in its training *in new ways* that maintain a considerable amount of linguistic and conceptual coherence. That requires more than merely guessing things it has seen before – it requires modeling the dynamics of language. If models only repeated what they had seen before, they would not be able to generate anything new, particularly complex sentence structures that are grammatical and coherent. It is somewhat difficult to convey how remarkable the models are currently. You just have to interact with them. They are imperfect, to be sure, but my qualitative experience interacting with them is like talking to a child, who happened to have memorized much of the internet.

²<https://garymarcus.substack.com/p/nonsense-on-stilts>

2 How this magic happens

These tools are not just impressive, they are philosophically important. The reason they are important is that they succeed by following a very particular approach: they are trained *only on text prediction*.³ This means that the models form probabilistic expectations about the next word in a text and they use the true next word as an error signal to update their latent parameters. This idea is one that dates back to at least Elman (1990), who showed how training a neural network on text prediction could lead it to discover key pieces of the underlying linguistic system.

Modern models are a resounding scientific victory for Elman’s idea. But while modern models inherit his general setup, advances have added a few critical differences. Probably the most important is that modern models include an *attentional mechanism* that allows the next word in sequence to be predicted from some previous far in the past. For example, in the ant story above, when it says that “The other ants were shocked and amazed by *Alex’s* ...”, it retrieves the name “Alex” from dozens of words prior. This likely is the key property that distinguishes large language models from the most popular earlier models. An n-gram model, for example, would estimate and use a conditional probability that depends on just the preceding few words (e.g. $n = 2, 3, 4, 5$); context-free grammars make independence assumptions that keep lexical items from influencing those far away. Not only do large language models allow such long-distance influences, but they allow them to take a relatively unconstrained form and so are able to induce functions which, apparently, do a stellar job at in-context word prediction.

A second key feature of these models is that they *integrate semantics and syntax*. The internal representations of words in these models are stored in a vector space, and the locations of these words include not just some aspects of meaning, but properties that determine how words can occur in sequence (e.g. syntax). There is a fairly uniform interface for how context and word meaning predicts upcoming material – syntax and semantics are not separated out into distinguished components in the model, nor into separate predictive mechanisms. Because of this, the network parameters these models find blend syntactic and semantic properties together, and both interact with each other and the attentional mechanism in nontrivial ways. This doesn’t mean that the model is incapable of distinguishing syntax and semantics, or e.g. mirroring syntactic structures regardless

³The underlying neural network weights are typically optimized in order to predict text, but note that many applications of these models also use human feedback to fine-tune parameters and try to tamp down the horrible things text on the internet leads models to say.

of semantics (see examples below), but it does mean that the two can be mutually informative. A related aspect of the models is that they have a huge memory capacity of billions to trillions of parameters. This allows them to memorize idiosyncrasies of language, and in this way they inherit from a tradition by linguists who have emphasized the importance of constructions (Goldberg 1995, Jackendoff 2013, Goldberg 2006, 2003, Tomasello 2000, McCauley & Christiansen 2019, Tomasello 2005, Edelman & Waterfall 2007) (see Weissweiler et al. 2023 for construction grammar analyses of large language models). Such models also inherit from the tradition of learning bottom-up, from data (e.g. Bod et al. 2003, Solan et al. 2005), and computational work which explicitly connects syntax and semantics (Steedman 2001, Siskind 1996, Ge & Mooney 2005, Kwiatkowski et al. 2012, Liang et al. 2009). The attentional mechanism seems to function in some ways like dependency grammar (Tesnière 1959, Hays 1964, De Marneffe et al. 2021).

A good mental picture to have in mind for how massively over-parameterized models like these work is that they have a rich potential space for inferring *hidden variables and relationships*. Hidden (or latent) variables have been one of the key aspects of language that computational and informal theories alike try to capture (Pereira 2000, Linzen & Baroni 2021). In the middle of a sentence, there is a hidden variable for the latent structure of the sentence; in speaking an ambiguous word, we have in mind a hidden variable for which meaning we intend; throughout a discourse we have in mind a larger story arc that only unfolds across multiple sentences. The formalisms of linguistics attempt to characterize these hidden variables too. But what large language models do is *infer* likely hidden structure because that structure permits them to better predict upcoming material. This makes them conceptually similar to embedding theorems in mathematics (Packard et al. 1980, Takens 1981, Ye & Sugihara 2016), which show that sometimes the full geometry of a dynamical system can be recovered from a low-dimensional projection of its states evolving in time. Linguistic corpora are a low-dimensional projection of both syntax and thought, so it is not implausible that a smart learning system could recover at least some aspects of these cognitive systems from watching text alone (Piantadosi & Hill 2022).

The structures present in large language models can be seen in detailed analyses, where as the model generates text after training, its internal states represent latent aspects of syntactic structure and semantic meaning (Manning et al. 2020, Futrell et al. 2019, Linzen & Baroni 2021, Pavlick 2022). The structure of the model's internal representation states and attentional patterns after training comes to capture tree structures with strong similarities to human-annotated parse trees (Manning et al. 2020), and the degree to which a model is tree-structured even predicts its generalization performance (Murty et al. 2023). The

models seem to perform well on constructions involving tracking the right latent state, like function words (Kim et al. 2019) and filler-gap dependencies (Wilcox et al. 2018). In fact, the internal processing structure of some models seems to spontaneously develop an intuitive pipeline of representing parts of speech, followed by parsing, semantic analysis, etc. (Tenney et al. 2019a, Liu et al. 2019).

All of this is possible because large language models develop representations of key structures and dependencies. It’s just that these representations are parameterized in a way which is unfamiliar to linguistics. As argued by Baroni (2022), this means that language models should be treated as bona fide linguistic theories. Specifically, a space of possible theories is parameterized by the models and compared to data to find which theory is best in a formal sense. To make one version of the idea concrete, imagine a physicist who wasn’t sure whether the law of gravitation force fell off with distance $1/r$ or distance squared $1/r^2$. To decide, the physicist might formulate a super-equation that captured both possibilities, for instance,

$$F(r, \alpha) = \alpha \cdot \frac{1}{r} + (1 - \alpha) \cdot \frac{1}{r^2}.$$

By seeing which parameter α best fits empirical data (e.g. measurements of forces F and distances r), they are *comparing* these two theories: $\alpha \approx 1$ means the former theory is right (since then $F = 1 \cdot (1/r) + 0$) and $\alpha \approx 0$ means the latter (since $F = 0 + 1 \cdot (1/r^2)$). When the data are stochastic, a good way to measure how well any particular α does is to see what probability it assigns to the data. We can make a principled choice between parameters – and thus theories – by choosing the one that makes the data most likely (*maximum likelihood principle*), although often including some prior information about plausible parameter values or penalties on complexity (e.g. Bayesian estimation). Such a physicist might then find that the best parameter for capturing data has $\alpha \approx 0$, supporting the second theory.⁴ In this case, inferring parameters *is* comparing theories: in computational modeling, there is no bright line between “just” fitting parameters and advancing theory.

Something very similar happens with many machine learning models; the main difference is that in these models, we don’t explicitly or intentionally “build in” the theories under comparison ($1/r$ and $1/r^2$). There exist natural bases from which you can parameterize essentially *any* computational theory.⁵ Parameter fitting in these models is effectively searching over a huge space of possible theories to see which one works best, in a well-defined, quantitative sense.

⁴For decades, other fields have used statistical learning models that take empirical data and infer laws (Koza 1994, Langley et al. 1983, Schmidt & Lipson 2009, Udrescu & Tegmark 2020).

⁵Up to the capacity of the network.

The bases required are actually pretty simple. Polynomials are one, but neural networks with sigmoid activations are another: fitting parameters in either of these can, in principle, realize countless possible relationships in the underlying domain. The challenge is that when these universal bases are used, it requires extra scientific work to see and understand what the parameters mean. Just to illustrate something roughly analogous, if we happened to write the above equation in a less transparent way,

$$F(r, \alpha) = \frac{\alpha \cdot (r - 1) + 1}{\log((e^r)^r)},$$

then it might take some work to figure out which α values correspond to $1/r$ and which to $1/r^2$. Squint just a little and you can imagine that instead of algebra, we had a mess of billions of weighted connections between sigmoids to untangle and interpret. It becomes clear that it could be hard to determine what is going on, even though *the theory is certainly in there*.

In fact, we don’t deeply understand *how* the representations these models create work (see Rogers et al. 2021). It is a nontrivial scientific program to discover how their internal states relate to each other and to successful prediction. Researchers have developed tools to “probe” internal states (e.g. Belinkov & Glass 2019, Tenney et al. 2019b, Kim et al. 2019, Linzen & Baroni 2021, Warstadt & Bowman 2022, Pavlick 2022) and determined some of the causal properties of these models. At the same time, this does not mean we are ignorant of all of the *principles* by which they operate. We can tell from the engineering outcomes that certain structures work better than others: the right attentional mechanism is important (Vaswani et al. 2017), prediction is important, semantic representations are important, etc. The status of this field is somewhat akin to the history of medicine, where people often worked out what kinds of treatments worked well (e.g. lemons treat scurvy) without yet understanding the mechanism.

One thing that is interesting is how modern language models integrate varied computational approaches to language, not by directly encoding them, but by allowing them to emerge (Manning et al. 2020, McClelland 2010) from the architectural principles that *are* built-in (Elman et al. 1996). For example, the models appear to have representations of hierarchy (Manning et al. 2020) and recursion, in the sense that they know about e.g. embedded sentences and relative clauses. They also almost certainly have analogs of constraints, popular in approaches like harmonic grammar (Smolensky & Legendre 2006, Prince & Smolensky 1997), model-theoretic grammar (Pullum 2007, 2013, Müller 2023), and HPSG (Müller et al. 2021, Dalrymple 2023). The models likely include both hard constraints

(like word order) and violable, probabilistic ones (Rumelhart & McClelland 1986). They certainly memorize some constructions (Goldberg 1995, Sag 2012, Jackendoff 2013, Goldberg 2006, 2003, Tomasello 2000, Edelman & Waterfall 2007) (see Chang et al. 2023). All of those become realized in the parameters in order to achieve the overarching goal of predicting text well.

3 The status of large language models as scientific theories

Many in language science see such models as at least relevant in some way to the future (Bommasani et al. 2021, Baroni 2022, Pater 2019). After all, they are the only models in existence that do a good job of capturing the basic dynamics of human language. However, in virtue of being neural networks, their – at least initial – state is wholly unlike the rules and principles that have dominated generative approaches to language.⁶ As described above, their parameters come to embody a theory of language, including representations of latent state through a sentence and a discourse. The exact same logic of tuning parameters to formalize and then compare theories is found in other sciences, like modeling hurricanes or pandemics: any set of assumptions will generate a distribution of predictions and the assumptions are adjusted to make the best predictions possible. In this way, a learning mechanism configures the model itself in the space of theories in order to satisfy the desired objective function. For hurricanes or pandemics, this is as rigorous as science gets; for sequences of words, everyone seems to lose their mind.

In discussing GPT-3 with Gary Marcus,⁷ for example, the most positive thing Chomsky could say was that it has an “ability to mimic some regularities in data”, followed quickly by “In fact, its only achievement is to use up a lot of California’s energy”.⁸ In another interview, he summarized that the models “have achieved zero” in terms of our understanding of language. Chomsky et al. (2023) characterized the models as useful “in some narrow domains” but hampered by “ineradicable defects” that made them “differ profoundly from how humans reason and use language”. As was quickly pointed out online, several of the examples they

⁶I will use “generative” to refer to the mainstream, Chomskyan approaches like Government and Binding and Minimalism. Note that some other approaches are also considered “generative”, and they may work from different, often more plausible, starting assumptions.

⁷<https://garymarcus.substack.com/p/noam-chomsky-and-gpt-3>

⁸One of the many limitations, concerns, and dangers (Bender et al. 2021, Bommasani et al. 2021) for these models is that they consume a lot of energy (Strubell et al. 2019). It’s estimated that these models take around 1000 MWh, compared to CA’s daily generation of about 750,000 MWh – so one model takes about 1/750th of one day of CA’s power.

brought up – like reasoning with counterfactuals or understanding sentences “John is too stubborn to talk to” – current models actually get correct. Chomsky et al. (2023) and others critique an imagined version of these models, while ignoring the fact that the real ones so aptly capture syntax, a success many have persistently claimed was impossible.

Part of why some generative linguists dismiss these models is that they are seen as too unconstrained, and thus not explanatory. Writing to Marcus about the models, Chomsky explains,

You can't go to a physics conference and say: I've got a great theory. It accounts for everything and is so simple it can be captured in two words: “Anything goes.”

All known and unknown laws of nature are accommodated, no failures. Of course, everything impossible is accommodated also.

This critique is a familiar rephrasing of his (and others') comments on language learning – essentially that one should not study an unconstrained system because it will not explain why languages have the particular form that they do.⁹

But it is too coarse a gloss to dismiss modern language models as “anything goes”. First, there is some ambiguity, in that as a model of language, the *trained* model is no longer “anything goes” – they implement one specific computation. But on the learning side, it's important to realize that there is no such thing as an “anything goes” model, in that any model will necessarily have certain tendencies and biases. Another way to say this is that not all “anything goes” models are equivalent. A three-layer neural network is well-known to be capable of approximating any computable function (Siegelmann & Sontag 1995). That's also an “anything goes” model. But the three-layer network will *not* work well on this kind of text prediction. Indeed, even some earlier neural network models, LSTMs, did not do as well (Futrell et al. 2019, Marvin & Linzen 2018, Hu et al. 2020); architectures generally vary in how well they capture computational classes of string patterns (e.g. Delétang et al. 2022).¹⁰

⁹You have to wonder how a physics conference would react to someone saying, following Lasnik (2002), “I've got a great theory. It accounts for everything: physical laws ‘might be a computationally perfect solution to the problem’ of how objects move.”

¹⁰Some also consider them not to be “scientific” theories because they are *engineered*. In an interview with Lex Fridman, Chomsky remarked, “Is [deep learning] engineering, or is it science? Engineering, in the sense of just trying to build something that's useful, or science, in the sense that it's trying to understand something about elements of the world ... We can ask that question, is it useful? Yeah, it's pretty useful. I use Google Translator. So, on engineering grounds it's kinda worth having, like a bulldozer. Does it tell you anything about human language? Zero, nothing.” In practice, there is often no clear line between engineering and science

We are granted scientific leverage by the fact that models that are equally powerful in principle perform differentially. In particular, we may view each model or set of modeling assumptions as a possible hypothesis about how the mind may work. Testing how well a model matches human-like behavior then provides a scientific test of that model's assumptions. This is how, for example, the field has discovered that attentional mechanisms are important for performing well. Similarly, "ablation" experiments allow researchers to alter one part of a network and use differing performance to pinpoint what principles support a specific behavior (see Warstadt & Bowman 2022).

Even when – like all scientific theories – we discover how they fail to match people in terms of mechanism or representation, they still are informative. Heeding George Box's advice that "all models are wrong, some are useful", we can think about the scientific strengths, contributions, and weaknesses of these models without needing to accept or dismiss them entirely. In fact, these models have already made a substantial scientific contribution by helping to delineate what is *possible*: Could it be possible to discover hierarchy without it being built in? Could word prediction provide enough of a learning signal to acquire most of grammar? Could a computational architecture achieve competence on WH-questions without movement, or use pronouns without innate binding principles? The answer to all of these questions is shown by recent language models to be "yes".

Beyond that, the models embody several core desiderata of good scientific theories. First, they are *precise and formal* enough accounts to be implemented in actual computational systems, unlike most parts of generative linguistics (Pulium 1989). Implementation permits us to see that these theories are internally consistent and logically coherent. In virtue of being implemented, such models are able to *make predictions*. Just to list a few examples, the patterns of connectivity and activation within large language models appear to capture dependency structures in words via attention (Manning et al. 2020). Their predictability measures can be compared to psychological measures (Hoover et al. 2023, Shain et al. 2022). Transformer models "predict nearly 100% of explainable variance in neural responses to sentences" (Schrimpf et al. 2021).

Unlike generative linguistics, these models show promise in being *integrated with what we know about other fields*, specifically cognition and neuroscience.

because scientists often need to invent new tools to even formulate theories: was Newton's calculus engineering instead or science? The machinery of transformational grammar? While the recent successes are due to engineering advances, researchers have been arguing for this form of model as cognitive theories for decades.

Many authors interested in human concepts have investigated the vector representations that the models form (Lake & Murphy 2021, Bhatia & Richie 2022). Surprisingly or not, the language model vectors appear to encode at least some aspects of semantics (Maas et al. 2011, Socher et al. 2013, Bowman et al. 2015, Grand et al. 2022, Bhatia & Richie 2022, Piantadosi & Hill 2022, Dasgupta et al. 2022, Petersen & Potts 2022, Pavlick 2022), building on earlier models that encoded semantics in neural networks (e.g. Rogers & McClelland 2004, Elman 2004, Mikolov et al. 2013). In fact, their semantic spaces can be aligned with the world with just a few labeled data points, at least in simple domains (Patel & Pavlick 2022). The representations that they learn can also be transferred to some degree across languages (Pires et al. 2019, Chi et al. 2020, Gonen et al. 2020, Papadimitriou & Jurafsky 2020, Papadimitriou et al. 2021, Hill et al. 2017), suggesting that they are inferring something deep about meaning. Following leading theories of concepts (Block 1986, 1998), the representations that language models learn may be meaningful in the sense of maintaining nontrivial *conceptual roles* (Piantadosi & Hill 2022), contrary to claims that meaning requires connections to the real world (Bender & Koller 2020). Building on the “parallel and distributed” tradition of cognitive modeling (McClelland et al. 1986), modern deep learning models are also likely able to be integrated with neuroscientific theories (Marblestone et al. 2016, Richards et al. 2019, Kanwisher et al. 2023, McClelland et al. 2020). In particular, they make predictions about neural data (e.g. Schrimpf et al. 2021, Caucheteux et al. 2022, Goldstein et al. 2022). Generative theories of syntax, by contrast, suffer from a “chronic lack of independent empirical support” and in particular have not been compellingly connected to neuroscience (Edelman 2019).¹¹

Moreover, these models *are empirically tested*, especially as a theory of grammar. Modern language models are state of the art in most natural language processing domains (Bommasani et al. 2021). Approaches from generative approaches to syntax are not competitive in any domain and arguably have avoided empirical tests of their core assumptions (Edelman & Christiansen 2003). Several authors have sought to quantitatively evaluate large language models on the syntactic constructions that motivate much of linguistic theory. Early results found some successes across a variety of different architectures. For example, Warstadt et al. (2019b) evaluated LSTM neural network models on a corpus of more than

¹¹“Considering how central the existence of a brain basis for syntax is to Chomskian (bio)linguistics, the scarcity of behavioral and brain evidence for syntactic structures is striking. ... In comparison to the basic phrase structure, evidence supporting the reality of more far-fetched theoretical constructs, such as movement, traces/copies, etc., remains elusive” (Edelman 2019).

ten thousand acceptability judgments published in the linguistics literature. They found that these models perform about in the mid 70% range, compared to human reliabilities in the upper 80% to 90% range. Warstadt et al. (2019a) look at BERT embeddings on negative polarity items and find “significant knowledge” of these structures but that success depends heavily on how the structures are tested. More recently, GPT models show high performance on filler-gap structures, including various forms of islands (Wilcox et al. 2018, 2022).

Gauthier et al.’s (2020) *SyntaxGym* is a standardized environment for testing models against a number of standardized test suites that captures linguistic constructions and phenomena like clefts, center-embedding, cataphors, negative polarity items, filler-gap dependencies, subordination, agreement, etc. This project builds on similarly exciting benchmarking efforts in neuroscience (Schrimpf et al. 2020). As of the writing of this article, the state of the art for language was a variant of GPT-2 that achieved nearly 90% on these constructions. *SyntaxGym* is an ingenious resource that finally allows quantitative comparison of theories. A general trend is that the more recent language models perform better, though it appears that proponents of (e.g.) minimalist grammars have not compared theories on *SyntaxGym*. It is not clear there has ever been, in the history of science, an implemented computational system that achieves such accuracy, but which is dismissed by a field which has failed to develop even remotely comparable alternatives.

In the spirit of considering large language models as scientific theories, it’s worth also highlighting their limitations. One is that while they succeed at language modeling, they are currently less successful in domains that require reasoning or thinking (Mahowald et al. 2024, Lake & Murphy 2021, Barrett et al. 2018, Collins et al. 2022). From an acquisition perspective, likely the most important limitation of current models is that they are trained on truly titanic datasets compared to children, by a factor of at least a few thousand (see Warstadt & Bowman 2022 for a comprehensive review of models in language acquisition). Moreover, these datasets are strings on the internet rather than child-directed speech. Work examining the scaling relationship between performance and data size shows that at least current versions of the models do achieve their spectacular performance only with very large network sizes and large amounts of data. (Kaplan et al. 2020) However, Zhang et al. (2021b) show that actually most of this learning is not about syntax. Models that are trained on 10–100 million words “reliably encode most syntactic and semantic features” of language, and the remainder of training seems to target other skills (like knowledge of the world). This in fact matches in spirit analyses showing that syntactic knowledge requires a small number of bits of information, especially when compared to semantics (Mollica

& Piantadosi 2019). Hosseini et al. (2022) present evidence that models trained on developmentally-plausible amounts of data already capture human neural responses to language in the brain.

Importantly, as Warstadt & Bowman (2022) outline, these models are in their early stages of development, so their successes are likely to be more informative about the path of children's language acquisition than the models' inevitable limitations. Current models provide a lower bound on what is possible, but even the known state-of-the-art doesn't characterize how well future models may do. Our methods for training on very small datasets will inevitably improve. One improvement might be to build in certain other kinds of architectural biases and principles; or it might be as simple as finding better optimization or regularization schemes. Or, we might need to consider learning models that have some of the cognitive limitations of human learners, as in Newport's (1990) "less is more" hypothesis. Such questions inspire the current "The BabyLM Challenge" (Warstadt et al. 2023), which aims to develop models capable of learning with a developmentally-plausible amount of data (see Geiping & Goldstein 2023 for training models with small amounts of compute resources; see also Eldan & Li 2023). It is an interesting scientific question whether low-resource, low-data learning is possible – I'll preregister a prediction of yes, with small architectural tweaks.

4 The refutation of key principles

The success of large language models is a failure for generative theories because it goes against virtually all of the principles these theories have espoused. In fact, *none* of the principles and innate biases that those who work in that tradition have long claimed necessary needed to be built into these models (e.g. binding principles, binary branching, island constraints, empty category principle, etc.). Moreover, these models were created without incorporating any of Chomsky's key methodological claims, like ensuring the models properly consider competence vs. performance, respect "minimality" or "perfection", and avoid relying on the statistical patterns of unanalyzed data.

The next sections focus on a few examples.

4.1 Syntax is integrated with semantics

Chomsky and others have long emphasized the study of syntax as a separate entity, not only from the rest of cognition but from the rest of language (see, e.g.

Chomsky 1957, Croft 1995, Newmeyer 1991, Adger 2018). Syntax in this approach is not supposed to be reducible to general statistics between words¹² – exactly the thing that large language models now provide.

Modern large language models integrate syntax and semantics in the underlying representations: encoding words as vectors in a high-dimensional space, without an effort to separate out e.g. part of speech categories from semantic representations, or even predict at any level of analysis other than the literal word. Part of making these models work well was in determining how to encode semantic properties into vectors, and in fact initializing word vectors via encodings of distribution semantics from e.g. Mikolov et al. (2013) and Radford et al. (2019). Thus, an assumption of the autonomy of syntax is not required to make models that predict syntactic material and may well hinder it.

We can have fun with the *colorless green ideas* sentence (see Gulordava et al. 2018) commonly offered as an example that syntax functions separately from semantics (and that transitional probability statistics are not enough). The claim that this is out of reach of statistical models (e.g. Chomsky 1957) has been shown to be wrong (Pereira 2000, Norvig 2012, Gulordava et al. 2018), but it is an interesting example nonetheless, in part because ChatGPT can not only learn relevant statistics, but it can create – in this case surely mostly regurgitate – an explanation of why this sentence is interesting:

Why is the sentence “colorless green ideas sleep furiously” interesting?

The sentence “colorless green ideas sleep furiously” is similarly interesting because it is a grammatically correct sentence, but it is semantically meaningless. The words used in the sentence are arranged in a way that follows standard syntax and grammar rules, but they do not form a coherent or understandable concept. This makes it an interesting linguistic oddity and a popular example of a nonsensical sentence.

We can test generalization with a follow-up:

¹²There is no current theory of linguistics that takes human syntactic capacities to be reducible to general probabilistic relations between elements of unanalysed data” (Adger 2018).

Generate ten other sentences like this

Purple fluffy clouds dream wildly

Blue glittery unicorns jump excitedly

Orange sparkly penguins waddle energetically

Red shimmering elephants trumpet melodiously

Yellow dazzling lions roar majestically

Green glittery monkeys swing energetically

Pink sparkly giraffes walk gracefully

Black shiny kangaroos hop playfully

White glittery bears cuddle affectionately

Brown shimmering rabbits hop merrily.

This model successfully gets the referent of “this” (a sentence). The model resolves the ambiguity in “like this” to refer to the *structure* of the sentence – the exact thing that statistical models weren’t even supposed to know! It generates some but not entirely low-frequency bigrams in the sentence. We can note a weakness in that it does not as readily generate wholly meaningless sentences (“black shiny kangaroos” is rare but not impossible), likely because meaningless language is rare in the training data. These results illustrate that even models which integrate syntax and semantics are able to make syntactic generalizations when appropriate. Syntax need not be autonomous in the underlying mechanism or model’s analysis in order to show this behavior.

4.2 Probability and information are central

A second point is that for these models, probability and information theory are central. Chomsky has long been dismissive of probability, saying “it must be recognized that the notion of ‘probability of a sentence’ is an entirely useless one, under any known interpretation of this term” (Chomsky 1969), a position he has maintained for decades (Norvig 2012).¹³ Often when those who work in Chomsky’s tradition talk about probability models, they refer to simple things like Shannon’s (1948) *n*-gram models that count up sequential word co-occurrences and were long used in natural language processing tasks (Chen & Goodman 1999, Manning & Schütze 1999). But by now, such models are decades out of date.

¹³Or in Chomsky (1957), “I think that we are forced to conclude that grammar is autonomous and independent of meaning, and that probabilistic models give no particular insight into some of the basic problems of syntactic structure.”

Newer models use probability to infer entire generating processes and structures, a common cognitive task and modeling domain (e.g. Tenenbaum et al. 2011, Ullman et al. 2012, Lake et al. 2015, Goodman et al. 2011, Lake et al. 2017, Rule et al. 2020, Kemp & Tenenbaum 2008, Yang & Piantadosi 2022); such models build on experimental work documenting statistical learning in human learners (e.g. Saffran et al. 1996a,b, Aslin et al. 1998, Newport & Aslin 2004, Aslin & Newport 2012). Probability is central for models because a probabilistic prediction essentially provides an error signal that can be used to adjust parameters that themselves encode structure and generating processes. An analogy is that one might imagine watching a driver and inferring the relevant structures and dynamics from observation – rules of the road (which side you drive on), conventions (behavior of multiple cars at stop signs), hard and soft constraints (don't turn too hard), etc. Even a simple domain like this faces many of the problems of underdetermination seen in language, but it is one where it is easy to imagine a skilled scientist or anthropologist discovering the key elements by analyzing a mass of data. Something similar goes on in machine learning, where a space of possible rules is implicitly encoded into the parameters of the model (see above).

It is worth noting that most models which deal in probabilities actually work with the log of probabilities, for reasons of numerical stability. Models that work on log probabilities are actually working in terms of description length (Shannon 1948, Cover 1999): finding parameters which make the data most likely (maximizing probability) is the same as finding parameters which give the data a short description (minimizing description length or complexity). Thus, the best parameters are equivalent to scientific theories that do a good job of *compressing* empirical data in the precise sense of description length. Far from “entirely useless”, probability is the measure that permits one to actually quantify things like complexity and minimality.

4.3 Representations are continuous and gradient

The fact that predictions are probabilistic is useful because it means that the underlying representations are continuous and gradient. Unlike work formalizing discrete rules and processes, typical of generative linguistics (e.g. Chomsky 1956, 1995, Collins & Stabler 2016, Chomsky 1957, Pinker & Prince 1988), modern language models do not use (at least explicit) rules and principles – they are based in a continuous calculus that allows multiple influences to have a gradient effect on upcoming linguistic items. The foundation for this approach was laid by early modelers like Rumelhart & McClelland (1986), who argued for the key features of today's architectures decades ago, including that “cognitive processes are seen

as graded, probabilistic, interactive, context-sensitive and domain-general" (McClelland & Patterson 2002).

Continuity is important because it permits the models to use gradient methods – essentially a trick of calculus – to compute what direction to change all the parameters in order to decrease error the fastest. Tools like TensorFlow and PyTorch that permit one to take derivatives of arbitrary models have been a critical methodological advance. This is not to say that these models end up with no discrete values – after all, they robustly generate subjects before verbs when trained on English. Similarly, the $F(r, \alpha)$ example might end up with a discrete answer like $\alpha \approx 0$. The key is that discreteness is a special case of continuous modeling, meaning that theories which work with continuous representations get the best of both worlds, fitting discrete patterns when appropriate and gradient ones otherwise. The success of gradient models over deterministic rules suggests that quite a lot of language is based on gradient computation. The success actually mirrors the prevalence of “relaxation” methods in numerical computing, where an optimization problem with hard constraints is often best solved via a nearby soft, continuous optimization problem. Thus, contrary to the intuition of many linguists, *even if* we wanted a hard, discrete grammar out at the end, the best way for a learner to get there might be via a continuous representation.

4.4 Learning succeeds in an unconstrained space

Perhaps most notably, modern language models succeed despite the fact that their underlying architecture for learning is relatively unconstrained. This is a clear victory for statistical learning theories of language (see Contreras Kallens et al. 2023). These models are capable of fitting a huge number of possible patterns, and while the principles of their architecture do constrain them to make some patterns easier than others, the resulting systems are incredibly flexible. Despite this lack of constraint, the model is able to figure out much of how language works. One should not lose sight of the role that “poverty of the stimulus” arguments have long played for generative linguists (e.g. Lasnik & Lidz 2016, Crain & Pietroski 2001, Legate & Yang 2002, Wexler & Culicover 1980, Laurence & Margolis 2001, Pearl 2022, Crain & Pietroski 2002). Poverty of the stimulus claims have been compellingly challenged both on empirical grounds about the nature of input, and through learning theories that acquire the relevant structures from input (e.g. Pullum & Scholz 2002, Clark & Lappin 2010, Perfors et al. 2011, Reali & Christiansen 2005, Solan et al. 2005). Large language models essentially lay this issue to rest because they come with none of the constraints that others have insisted are necessary, yet they capture almost all key phenomena

(e.g. Wilcox et al. 2022). It will be important to see, however, how well they can do on human-sized datasets, but their ability to generalize to sentences outside of their training set is auspicious for empiricism.

Recall that many of the learnability arguments were *supposed* to be mathematical and precise, going back to Gold (1967) (though see Johnson 2004, Chater & Vitányi 2007) and exemplified by work like Wexler & Culicover (1980). It's not that we don't know the right learning mechanism; it's supposed to be that it can be proven none exists. Even my own textbook from undergraduate syntax purports to show a "proof" that because infinite, productive systems cannot be learned, parts of syntax must be innate (Carnie 2021). Legate & Yang (2002) call the innateness of language "not really a hypothesis" but "an empirical conclusion" based on the strength of poverty of stimulus arguments. Proof of the impossibility of learning in an unrestricted space was supposed to be the power of this approach. It turned out to be wrong.

The notion that the core structures of language could be discovered without substantial constraints may sound impossible to anyone familiar with the generative syntax rhetoric. But learning without constraints is not only possible, it has been well-understood and even *predicted*. Formal analyses of learning and inference show that learners can infer the correct theory out of the space of possible computations (Solomonoff 1964, Hutter 2004, Legg & Hutter 2007). In language specifically, the correct generating system for grammars can similarly be discovered out of the space of all computations (the most unrestricted space possible), using only observations of positive evidence (Chater & Vitányi 2007).

In this view, large language models function somewhat like automated scientists or automated linguists, who also work over relatively unrestricted spaces, searching to find theories which do the best job of parsimoniously predicting observed data. It's worth thinking about the standard lines of questioning generative syntax has pursued – things like, why don't kids ever say "The dog is believed's owners to be hungry" or "The dog is believed is hungry" (see Lasnik & Lidz 2016). The answer provided by large language models is that these are not permitted under the best theory the model finds to explain what it does see. Innate constraints are not needed.

4.5 Representations are complex, not minimal

Next, there is an important sense in which large language models are not minimal representationally, but maximal. What I mean is that there is not a single core nugget of representation or structure (like *merge*) that leads these models to succeed. Nor are any biases against derivational complexity likely to play a key role,

since everything is a single big matrix calculation. This calculation moreover is not structurally minimal or “perfect” in the sense that minimalist linguistics means (e.g. Lasnik 2002). Instead, the attentional mechanisms of large language models condition on material that is arbitrarily far away, and perhaps not even structurally related since this is how they model discourses between sentences. A grammatical theory that matches people's almost limitless capacity for memorizing countless chunks of language changes the landscape of how we should think about derivation and complexity.

Deep learning has actually changed how people think about complexity in statistical learning too. It has long been observed that having too many parameters in a model would prevent the model from generalizing well: too many parameters allow a model to fit patterns in the noise, and this can lead it to extrapolate poorly. Deep learning turned this idea on its head by showing that some models will fit (memorize) random data sets (Zhang et al. 2021a), meaning they can fit all the patterns in the data (including noise) *and* still generalize well. The relationship between memorization and generalization is still not well-understood, but one of the core implications is that statistical learning models can work well, sometimes, even when they are over-parameterized.

While discussing statistical learning (before deep learning) with Peter Norvig, Chomsky noted that “we cannot seriously propose that a child learns the values of 10^9 parameters in a childhood lasting only 10^8 seconds.” One has to wonder if a similar argument applies to biological neurons: humans have 80 billion neurons, each with thousands of synapses. If childhood is only 10^8 seconds, how do all the connections get set? Well, also note that the 3 billion base pairs of the human genome certainly can't specify the precise connections either. Something must be wrong with the argument.

Two missteps are easy to spot. First, even if a model has billions of parameters, they will not generally be independent. This means that a single data point could set or move thousands or millions or billions of parameters. For example, observing a single sentence with SVO order might increase (perhaps millions of) parameters that put S before V, and decrease (perhaps millions of) parameters that put S after V. Steps of backpropagation don't change one parameter – they change potentially *all* of them based on the locally best direction (the gradient).

Second, these models, or learners, often don't need to pinpoint exactly one answer. A conjecture called the *lottery ticket hypothesis* holds that the behavior of a deep learning model tends to be determined by a relatively small number of its neurons (Frankle & Carbin 2018). Thus, the massive number of parameters is not because they all need to be set exactly to some value. Instead, having many degrees of freedom probably helps these models learn well by giving the models

directions they can move in to avoid getting stuck. It may be like how it is easier to solve a puzzle if you can pick the pieces up and put them down (move them in a third dimension) rather than just sliding them around the table. More degrees of freedom can *help* configure your theory to work well.

4.6 Hierarchical structure need not be innate

These models discover structure – including hierarchical structure – from their training (Hewitt & Manning 2019, Manning et al. 2020, Lin et al. 2019, Tenney et al. 2019b, Warstadt & Bowman 2020, Kim et al. 2020, Linzen & Baroni 2021, Mahowald et al. 2024). These models certainly could learn rules based on linear, rather than hierarchical, structure, but the data strongly leads them towards hierarchical, structural generalization. This capacity to discover hierarchical structure rather than building it in was long-emphasized by cognitive psychologists, for instance work inducing syntactic categories by clustering (Redington et al. 1998, Clark 2000), or Perfors et al. (2011), who showed that a Bayesian model comparison applied to child-directed speech could identify a context-free (hierarchical) grammar over alternatives. The most recent models build on earlier efforts to encode hierarchical processes into neural networks (e.g. Tabor et al. 1997, Christiansen & Chater 1999, Tabor 2000, Smolensky & Legendre 2006).

Chomsky’s familiar example of aux-inversion is meant to show that children must possess hierarchical syntax. As a simple experiment, we can also ask these models to form some questions. For example, the familiar aux-inversion:

Convert the following into a single question asking if the accordion is in the rain: “The accordion that is being repaired is out in the rain.”

Is the accordion that is being repaired out in the rain?

More rigorous experiments demonstrating aux-inversion success even in earlier language models can be found in Warstadt & Bowman (2020). It is philosophically interesting that this model knows which “is” to “move”, but I think it’s staggering that unlike pretty much every other model in the whole of science, we can interrogate these models simply by *asking* them to perform a certain behavior. It’s also interesting to ask it to generate a few more examples, without giving guidance on the nature of the question:

Form the following sentence into a question in ten different ways: "The accordion that is being repaired is out in the rain."

Is the accordion that is being repaired out in the rain?

Where is the accordion that is being repaired located in the rain?

In what condition is the accordion being repaired out in the rain?

Is the accordion being repaired, which is outside, getting wet in the rain?

Is the accordion being repaired, which is out in the rain, being protected from the water?

Is the accordion being repaired out in the rain, and if so, will it be damaged by the water?

How long has the accordion that is being repaired been out in the rain?

Is the accordion, which is being repaired, being kept in the rain outside?

What is happening to the accordion, which is being repaired, while it is out in the rain?

Is the accordion that is currently being repaired exposed to the rain outside?

It is remarkable too that the models maintain enough representation of its prior discourse to generate distinct questions. The responses also highlight integration of syntax and semantics, for instance knowing that such an accordion might be "protected from the water" or "damaged by the water".

The aux-inversion cases have been interesting only insofar as interrogatives are derived from declaratives. The question of how kids know which "is" to move is nonsensical outside of that assumption. These models, which are trained only on text prediction, provide an implemented account in which we don't need to think of interrogatives as derived from declaratives (it seems very unlikely this is what is happening inside the model). The models might thus lead us to consider connections between other constructions, such as those used in the above list, which are likely located nearby to the target question in the model's latent space of activations. For optimally predictive theories, constructions may be connected to each other but not in the way generative theories of syntax predict.

4.7 Language and thought dissociate

Human language, for Chomsky, is deeply interconnected to human thought (Everaert et al. 2015). Chomsky (2002) describes language as "a system for expressing

thought;” in fact, one which is used primarily for speaking to oneself. Interestingly, he does not draw on the literature on inner monologues, which show substantial variation between individuals, with some describing virtually no internal language use at all (e.g. Reed 1916, Heavey & Hurlburt 2008, Roebuck & Lupyan 2020). Chomsky’s view, though, is made perhaps more plausible by arguments that thought itself shares many properties of language, namely a compositional, language-like structure (Fodor 1975, Fodor & Pylyshyn 1988, Goodman et al. 2014, Piantadosi & Jacobs 2016, Quilty-Dunn et al. 2022). Chomsky frequently contrasts his inner thought view of language with the idea that language primarily is structured to support communication (e.g. Hockett 1959, Bates & MacWhinney 1982, Gibson et al. 2019), although it’s worth noting he sometimes draws the opposite predictions from what efficient communication would actually predict (e.g. Piantadosi et al. 2012). Mahowald et al. (2024) argue in a comprehensive review that large language models exhibit a compelling dissociation between linguistic ability and thinking. The models know so much syntax, and aspects of semantics, but it is not hard to trip them up with appropriate logical reasoning tasks. Thus, large language models provide a proof of principle that syntax can exist and likely be acquired separately from other more robust forms of thinking and reasoning. Virtually all of the structure we see in language can come from learning a good model of strings, not directly modeling the world.

Models therefore show a logically possible dissociation between language and thinking. But a considerable amount of neuropsychological evidence supports the idea that language and thought are actually separate in people as well. Fedorenko & Varley (2016) review a large patient literature showing that patients with aphasia are often able to succeed in tasks requiring reasoning, logic, theory of mind, mathematics, music, navigation, and more. Aphasic patient studies provide an *in vivo* dissociation between language and other rational thinking processes. They also review neuroimaging work by Fedorenko et al. (2011) and others showing that the brain regions involved in language tend to be *specific to language* when it is compared to other non-linguistic tasks. That is not what would be predicted under theories where language is inherently tied to thought.

This is not to say that there is no way language and thought are related – we are able to specify some kinds of reasoning problems, communicate solutions, and sometimes solve problems with language itself. A compelling proposal is that language may be a system for connecting other core domains of representation and reasoning (Spelke 2003).

5 Why this and not that

Chomsky maintains (in the same Marcus interview above) that large language models have achieved nothing because they fail to explain “Why this? Why not that?” The question of whether these models can explain why human language has the form that it does is an interesting one that likely depends on whether the language system evolved before language or concurrently with it. If language co-opted neural systems for general sequential prediction (e.g. Christiansen & Chater 2015), it's possible we had some architecture like these models before we had language, and therefore the form of language is explained by the pre-existing computational architecture. On the other hand, if the two co-evolved, language might not be explained by the processing mechanisms. With such uncertainty, there are “why” questions that a large language model may not answer. This does not mean the models lack scientific value. In the same way, Newton's laws don't answer why those are the laws as opposed to any other, and yet they still embody deep scientific insights. Anyone who has had a child ask “why” repeatedly will recognize that at some point, *everyone's* answers ground out in assumption.

However, it is worth highlighting in this context that Chomsky's own theories don't permit particularly deep “why” questions either. In large part, he simply states that the answer is genetics or simplicity or “perfection”, without providing any independent justification for these claims.¹⁴ For example, readers of Berwick & Chomsky (2016) – a book titled *Why only us: Language and evolution* – might have hoped to find a thorough and satisfying “why” explanation. Their answer boils down to people having *merge* (essentially chunking two elements into one, unordered). And when it comes down to explaining *why merge*, they fall down the stairs: they simply state that “merge” is the minimal computational operation, apparently because that's what they think and that's that. Forget the relativity of definitions of simplicity, articulated by Goodman (1965), where what is considered simple must ground out in some convention. Berwick & Chomsky do not even attempt to explain why they believe “merge” is simpler than other simple computational bases, like cellular automata or combinatorial logic or systems of colliding Newtonian particles – all of which are capable of universal computation (and thus encoding structures, including hierarchical ones). Or maybe more directly, what makes *merge* “simpler” or more “perfect” than, say, back-propagation? Or Elman et al.'s (1996) architectural biases? Berwick & Chomsky

¹⁴Indeed, as Johnson & Lappin (1997) argue, the assumptions lead to considerable complexity – e.g. “by building a global economy metric into syntax to identify an optimal derivation, the [Minimalist Program] introduces considerable conceptual and computational complexity into linguistic theory, and so produces a model of grammar that is far less economical than those that employ only constraints on representations ...”

don't consider these questions, even though the ability to scientifically go after such "why" questions is supposed to be the hallmark of the approach. One might equally just declare that a transformer architecture is the "minimal" computational system that can handle the dependencies and structures of natural language and be done with it.

We should not actually take it for granted that generative syntax has found any regularities across languages that need "why" explanations. Evans & Levinson (2009) have made a convincing empirical case that prior features hypothesized to be universal – and thus plausibly part of the innate endowment of language – actually are not found in *all* languages. Perhaps most damningly, not even all languages appear to be recursive (Everett 2005, Futrell et al. 2016, Pullum 2023), contradicting *the* key universality claim from Hauser et al. (2002). Dąbrowska (2015) highlights profound differences in adult grammars between languages and the lack of a coherent formulation from generative linguists. None of this is to say that we won't be able to find any universals; rather, the proposed ones aren't there, and the differences between languages may be more scientifically informative than their commonalities (Pullum & Scholz 2009). On the methodological side, statistical analyses show that in order to justifiably claim something as universal, one would need on the order of 500 *statistically independent* languages, which is likely beyond what is currently in existence (Piantadosi & Gibson 2014).

However, the question of why languages are the way they are does have plausible, testable hypotheses. These hypotheses are interdisciplinary (Reali & Christiansen 2009) and include variegated influences of communicative (Zipf 1965, Hockett 1959, Bates & MacWhinney 1982, Piantadosi et al. 2012, Gibson et al. 2013, 2019, Coupé et al. 2019, Hahn et al. 2020, Futrell & Hahn 2022), cultural (Everett 2005, Lupyan & Dale 2010, Dale & Lupyan 2012, Everett et al. 2015), ecological (Lupyan & Dale 2016), learning (Smith & Kirby 2012, Steinert-Threlkeld & Szymanik 2019, 2020), and cognitive factors (Gibson 2001, Futrell et al. 2015). Such pressures have led to efficient and useful properties, including minimization of dependency structures (Futrell et al. 2015), the presence of communicatively useful ambiguity (Piantadosi et al. 2012), and efficiency in lexical systems (Kemp & Regier 2012, Kemp et al. 2018, Zaslavsky et al. 2019, Steinert-Threlkeld 2019, Mollica et al. 2021, Mahowald et al. 2022, Denić et al. 2022). Recent advances in understanding cultural evolution also should shape our theories of linguistic nativism, highlighting that weak biases are often sufficient over the course of cultural transmission to lead to stable patterns across languages (Thompson et al. 2016, Kirby et al. 2014, Chater et al. 2009). All of these are factors that Chomsky and others have never really grappled with, much less successfully ruled out as alternative answers to the "whys" of language.

6 The refutation of method

Chomsky often describes his own approach as “Galilean”, meaning that he seeks the underlying principles in phenomena rather than analysis of large amounts of data. The term is both a misnomer (Behme 2014) and a not-so-subtle insult to colleagues who choose to work from different assumptions. Of course, Galileo cared about quantitative measurement of the world in order to formulate theories, developing tools of his own and even trying to measure the speed of light.¹⁵ Chomsky's view was clearly articulated in an interview with Yarden Katz in 2012 where, at the time, he was focused on explaining that Bayesian models were useless (Katz 2012):

... [S]uppose that somebody says he wants to eliminate the physics department and do it the right way. The “right” way is to take endless numbers of videotapes of what's happening outside the [window], and feed them into the biggest and fastest computer, gigabytes of data, and do complex statistical analysis – you know, Bayesian this and that – and you'll get some kind of prediction about what's gonna happen outside the window next. In fact, you get a much better prediction than the physics department will ever give. Well, if success is defined as getting a fair approximation to a mass of chaotic unanalyzed data, then it's way better to do it this way than to do it the way the physicists do, you know, no thought experiments about frictionless planes and so on and so forth. But you won't get the kind of understanding that the sciences have always been aimed at – what you'll get at is an approximation to what's happening.

It's worth pinpointing exactly where this kind of thinking has gone wrong because it is central to the field's confusion in thinking about large language models. Chomsky's view certainly does not address the above idea that parameter fitting in a statistical model often *is* theory building and comparison.

But another factor is missing, too. Over modern scientific history, many computational scientists have noticed phenomena of *emergence* (Goldstein 1999, McClelland 2010), where the behavior of a system seems somewhat different than might be expected from mere knowledge of its underlying rules. This idea has been examined specifically in language models (Wei et al. 2022, Manning et al. 2020), but the classic examples are older. The stock market is unpredictable even when individual traders might follow simple rules (“maximize profits”). Market booms and busts are the emergent result of millions of aggregate decisions. The high-level phenomena would be hard to intuit, even with full knowledge of traders'

¹⁵“Measure what can be measured, and make measurable what cannot be measured.”

strategies or local goals. The field of complex systems has documented emergent phenomena virtually everywhere, from social dynamics to neurons to quasicrystals to honeybee group decisions. The field to have most directly grappled with emergence is physics, where it is acknowledged that physical systems can be understood on multiple levels of organization, and that the same laws that apply one level (like molecular chemistry) may have consequences that are difficult to foresee on another (like protein folding) (Anderson 1972, Crutchfield 1994a,b).

Often, the only way to study such complex systems is through simulation. We often can't intuit the outcome of an underlying set of rules, but computational tools allow us to simulate and just *see* what happens. Critically, simulations *test the underlying assumptions and principles* in the model: if we simulate traders and don't see high-level statistics of the stock market, we are sure to have missed some key principles; if we model individual decision-making for honeybees but don't see emergent hive decisions about where to forage or when to swarm, we are sure to have missed principles. We don't get a direct test of principles because the systems are too complex. We only get to principles by seeing if the simulations recapitulate the same high-level properties of the system we're interested in. And in fact the *surprisingness* of large language models' behavior illustrates how we don't have good intuitions about language learning systems.

We can contrast understanding emergence through simulation with Chomsky's attempt to state principles and reason informally (see Pullum 1989) to their consequences. The result is pages and pages of stipulations and principles (see, e.g., Collins & Stabler 2016 or Chomsky 1995) that nobody could look at and conclude were justified through rigorous comparison to alternatives. After all they *weren't*: the failure of the method to compare vastly different sets of starting assumptions, including neural networks, is part of why modern large language models have taken everyone by surprise. The fact that after half a century of grammatical theories, there can be a novel approach which so completely blows generative grammar out of the water on every dimension is, itself, a refutation of the "Galilean method".

An effective research program into language would have considered, perhaps even developed, these kinds of models, and sought to compare principles like those of minimalism to the principles that govern neural networks. This turn of events highlights how much the dogma of being "Galilean" has counterproductively narrowed and restricted the space of theories under consideration – a salient irony given Chomsky's (appropriate) panning of Skinner for doing just that.¹⁶

¹⁶“What is so surprising is the particular limitations [Skinner] has imposed on the way in which the observables of behavior are to be studied.” (Chomsky 1959)

7 Enduring contributions of Chomsky's program

I have attempted to convey my own sense of excitement about large language models, as well as my own pessimism about several aspects of Chomsky's approach to linguistics. However, it is easy to see that beyond the critiques above, many of Chomsky's emphatic focuses will survive his specific theories. For example, one of Chomsky's lasting contributions to cognitive science will be his emphasis on the reality of cognitive structure, like Tolman, Newell & Simon, Miller, and others of the cognitive revolution (Nadel & Piattelli-Palmarini 2003, Boden 2006). The search for the properties of human cognition that permit successful language acquisition is clearly central to understanding not just the functioning of the mind, but understanding humanity. It is a deep and important idea to try to characterize what computations are required for language, and to view them as genuinely mental computations. Chomsky's focus on children as creators of language, and on understanding the way in which their biases shape learning is fundamental to any scientific theory of cognition. Linguistic work in Chomsky's tradition has done a considerable amount to document and support less widely spoken languages, a struggle for current machine learning (Blasi et al. 2022). The overall search for "why" questions is undoubtedly core to the field, even if there are disagreements about what counts as an answer.

Some of the ideas of Chomsky's approach are likely to be found even in language models. For example, the idea that many languages are hierarchical is likely to be correct, embodied in some way in the connections and links of neural networks that perform well at word prediction. There may be a real sense in which other principles linguists have considered are present in some form in such models. If the models correctly perform on binding questions, they may have some computations similar to binding principles. But none of these principles needed to be innate. And in neural networks, they are realized in a form nobody to date has written – they are distributed through a large pattern of continuous, gradient connections. Moreover, the representation of something like binding is extraordinarily unlikely to have the form generative syntax predicts since the required underlying representational assumptions of that approach (e.g. binary branching, particular derivational structures, etc) are not met.

Another key contribution of Chomsky's research program has been to encourage discovery of interesting classes of sentences, often through others like Ross (1967). Regardless of the field's divergent views on the reality of WH-movement, for example, the question of *what* determines grammaticality and ungrammaticality for WH-sentences is an important one. Similarly, phenomena like "islands" do not go away because of large language models – they are targets to be ex-

plained (and models do a pretty good job according to analyses by Wilcox et al. 2022). Such phenomena are often difficult to separate from theory, as in the example above about whether declaratives and interrogatives are actually connected in the real grammar. Regardless of theory, researchers working in Chomsky's tradition have illuminated many places where human linguistic behavior is more complicated or intricate than one might otherwise expect.

As articulated by Pater (2019), the field should seek ways to integrate linguistics with modern machine learning, including neural networks. I have highlighted some researchers whose approach to language clearly resonates with the insights of modern language models. The current upheaval indicates that we should foster a pluralistic linguistics that approaches the problem of language with as few preconceptions as possible – perhaps even a fundamental reconceptualization of what language is for and what it is like (Edelman 2019). Maybe many of the “syntactic” phenomena that Chomskyan theories have concerned themselves with are really about something else, like pragmatics or memorized constructions. Maybe the universals of language – if there are any – come from aspects of use like communicative and cognitive pressures, or other cultural factors. Maybe linguistics could learn from the methods of cognitive science (Edelman 2007). Maybe theories of grammar should respect humans' unparalleled memory capacity for sequential material. Maybe we should have linguistics students learn information theory, probability, neural networks, machine learning, anthropology, numerical methods, model comparison, Kolmogorov complexity, cognitive psychology, language processing, multi-agent systems, etc. The most permanent legacy of Chomsky's approach could be as an admonishment about what happens when the study of language is separated from the rest of science.

8 Conclusion

One must be frank about the state of the art for models that capture syntax. It's not that large language models offer slightly higher performance than other approaches in linguistics; it's not that they better cover some corners of syntax. It's that there is *nothing comparable in all of linguistic theory* to the power of large language models in both syntax and semantics – much less discourse coherence, style, pragmatics, translation, meta-linguistic awareness, non-linguistic tasks, etc. They are game changers on all fronts. Optimists who view them as at least a plausible direction for acquisition see them as a way to build in and test architectural principles and biases, long emphasized by connectionist modelers like McClelland et al. (1986), Elman et al. (1996), Smolensky & Legendre (2006),

and others. Those who doubt they could function as models of acquisition should nonetheless see the success of gradient representations, architectural assumptions, and implicit or emergent principles as a theory of grammar. These models have opened the space of plausible linguistic theories, allowing us to test principles beyond the ones that have traditionally concerned linguists. They allow us to finally develop compelling theories of the interplay of structure and statistics. And they appear to solve many problems that generative syntacticians have worried about, but without using any of their theoretical tools and constructs. Large language models rewrite the philosophy of approaches to language.

It's worth ending with a reminder of how impressive these models are:

Explain how large language models undermine Chomsky's main claims about linguistics.

Noam Chomsky is a prominent linguist and philosopher who has made significant contributions to the field of linguistics and language theory. He has put forward the idea that language is an innate, biologically determined ability that is unique to humans, and that all humans possess a universal grammar, or a set of innate linguistic rules that allow us to understand and produce language.

However, the development of large language models like GPT-3 has challenged some of Chomsky's main claims about linguistics and the nature of language.

First, the fact that language models can be trained on large amounts of text data and can generate human-like language without any explicit instruction on grammar or syntax suggests that language may not be as biologically determined as Chomsky has claimed. Instead, it suggests that language may be learned and developed through exposure to language and interactions with others.

Second, the success of large language models in performing various language tasks such as translation, summarization, and question answering, has challenged Chomsky's idea that language is based on a set of innate rules. Instead, it suggests that language is a learned and adaptive system that can be modeled and improved through machine learning algorithms.

Finally, the ability of language models to generate coherent and coherent [sic] language on a wide range of topics, despite never having seen these topics before, suggests that language may not be as rule-based as

Chomsky has claimed. Instead, it may be more probabilistic and context-dependent, relying on patterns and associations learned from the text data it was trained on.

In conclusion, while Chomsky's contributions to the field of linguistics have been significant, the development of large language models has challenged some of his main claims and has opened up new avenues for exploring the nature of language and its relationship with machine learning and artificial intelligence.

Models that can do this are a gift. They make language the most exciting arena in all of cognitive science and AI. Such models are also a tool that will help linguistics to refine theories and compare leading ideas to strong alternatives.

It's worth remembering that Chomsky's approach to linguistics isn't so much about his particular beliefs – many linguists who work in his tradition disagree with him on specific points. And there is nothing wrong with being wrong: Chomsky's own theories elegantly articulated many deep, compelling ideas that have motivated linguistic and cognitive research. But modern language models highlight the weaknesses of his methodological prescriptions. One cannot seek principles without justifying them with rigorous experiments and comparisons; one can't reason about complex systems without implementations; one can't discover mechanisms without formally testing completely different approaches (like neural networks); one can't proclaim simplicity, optimality, or “perfection” without seriously connecting these terms to formal notions. These arguments worked on many linguists and consequently prevented that tradition from developing anything close to the wild successes of modern language models.

Frederick Jelinek's quip “Every time I fire a linguist, the performance of the speech recognizer goes up” (Jelinek 1988) was a joke among linguists and computer scientists for decades. I've even seen it celebrated by academic linguists who think it elevates their abstract enterprise over and above the dirty details of implementation and engineering. But, while generative syntacticians insulated themselves from implementation, empirical tests, and formal comparisons, engineering took over. And now, engineering has solved the very problems the field has fixated on – or is about to very soon. The unmatched success of an approach based on probability, internalization of constructions in corpora, gradient methods, and neural networks is, in the end, humiliating for a subfield of linguistics that has spent decades deriding these tools. But now we can do better.

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Postscript: A commentary on replies

The original draft of this paper, posted on LingBuzz, received a number of replies from authors, including those who primarily work in generative theories of linguistics. Many offered a critical appraisal of my claims and in the summer of 2023, I added this postscript summarizing their critiques and giving brief responses.

Rawski & Baumont (2023) wrote a single page response titled "Modern language models refute nothing". They say the argument I made is invalid in that I argued that models which correlate with behavior (and neural data) are good candidates for what people actually do, or at least promising directions. They say that my argument is not *deductively* valid since it "fallaciously affirms the consequent". This logical error is the one you make if you know that swimming causes you to get wet ($p \rightarrow q$), you see someone wet (q) and claim to know that they went swimming (p). They, of course, could be wet from showering or rain or something else, so you did not draw a valid conclusion. In our case, a model showing the same behavior as people (q) does not logically entail that it works in the same way (p), even though we know if it worked the same way it would show the same behavior ($p \rightarrow q$). Sure, great, I agree it's not a *deductive* argument. But very little in science is. Scientific theories are more often *inductive* and take the form I describe: we think theories of physics or biology are likely because they predict the experimental data, not because they are deduced as some logical consequence of the experimental data. This setup can be formalized in e.g. Bayesian inference, and is common in Bayesian data analysis, machine learning, and Bayesian philosophy of science (see, e.g., Jeffrey 1939, Godfrey-Smith 2009). From this point of view, Rawski & Baumont's (2023) attempt to cast science as deduction is as strange to me as saying, "Even though the equations of thermodynamics did a good job of predicting what happened when I made a steam engine, it's fallaciously affirming the consequent to therefore increase my belief that thermodynamics is a true theory of the physical world."

Rawski & Baumont (2023) also disliked my analogy to forces ($1/r$ compared to $1/r^2$) and brought up the historical example of how a heliocentric theory of

the solar system made empirically worse predictions than a geocentric one. They are correct that there are other considerations in scientific theories, such as simplicity and explanation. Simplicity is very often formalized with e.g. Bayesian methods (that are not popular in generative linguistics by any means) and simplicity is often a consideration in neural networks and machine learning, which include regularization or other architectural features. However, Rawski & Baumont (2023) seem to misunderstand the linkage between experiment and theory: they argue that “explanatory power, not predictive adequacy, is directly responsible” for essentially everything in physics. Not all physicists think a strong focus on non-empirical considerations has been good for physics (Hossenfelder 2018). But a key ingredient Rawski & Baumont (2023) leave out of the success story of modern physics is *data*: physics actually tests its theory against alternatives – in fact, pretty much any alternative anyone can think of. Physics even works to actively chart out the landscape of mathematically *possible* theories. Modern physics actually rigorously implements a “standard model” that generates quantitative predictions, which it constantly attempts to falsify with new experimental work. Physics spent \$9 billion on the Large Hadron Collider, and \$10 billion on the James Webb Space Telescope in order to gather *empirical data and compare quantified, implemented theories*. As I pointed out, generative syntax didn’t even enter SyntaxGym.

Milway (2023) wrote a brief response that first argues that the title of my paper is a “category error” since “one cannot refute an approach.” I’d prefer not to be prescriptive about lexical semantics. There are plenty of papers for example talking about a “refutation of behaviorism” (many attributing that to Chomsky!), “refutation of psychologism”, or a “refutation of Copernicanism,” etc. Refuting an approach means showing how its core assumptions probably don’t work, and therefore that the approach is unlikely to be productive. My paper detailed which core assumptions I thought would not work and why.

Milway (2023) also did not like the generated examples for “Colorless green ideas sleep furiously”; nor did Gary Marcus, who critiqued these particular responses in a public video but then backed out of a public discussion with me about the paper. Milway and Marcus were unimpressed because the model’s generated sentences (e.g. “Black shiny kangaroos hop playfully”) still had meaningful pairs of words (a kangaroo, allegedly, could be “shiny” but an idea certainly can’t be “green”). I, on the other hand, was impressed that the model knew enough to generate the same *syntactic* form, which was evidence that it knew some syntax, and in fact could separate it from semantics. It was strange to me that authors criticized these responses as showing the models do not exhibit

“human-like behavior” without even wondering whether people would produce meaningless bigrams from the same prompt.

Milway (2023), Kodner et al. (2023), and others emphasized that a problem with modern language models is that you do not know *why* they show the behavior that they do. Here is Milway:

[Piantadosi] presents ChatGPT data showing grammatical aux-inversion in English, but provides no explanation as to how it achieves this. Such an explanation though, is at the core of Chomsky's approach to language. If MLMs do not provide an explanation, then how can they supplant Chomsky's approach?

It is correct that we do not, at present, know how the models achieve this and my claim was never that our work was done. The argument I made was that (i) these models do it *somehow* and (ii) how they do it is almost certainly different from Chomsky's approach, and (iii) their approach works *really, really* well. However they do it, they can actually produce streams of grammatical sentences! And translate, and summarize, and elaborate, etc. Whatever principles allow models to do these things are the most promising principles for modern linguistics. I specifically argued that they were likely things like reliance on probability, memorized constructions, implementations of lexical semantics, etc.

I'll agree, though, that there is an interesting debate about the nature of science lurking here. The critics' position seems to be that in order for something to be a scientific theory, it must be *intuitively comprehensible* to us. I disagree because there are many phenomena in nature which probably will *never* admit a simple enough description for us to comprehend. We cannot just exclude these things from scientific inquiry. There probably is no simple theory of a stock market (why IBM takes on a particular value) or dynamics in complex systems (why an O₂ molecule hits a particular place on my eyeball). Certainly there are local, proximate causes (Tom Jones bid \$142 for IBM; the O₂ molecule was bumped by another), but when you start to trace these causes back into the complex system, you will quickly exceed our ability to understand the complex network of interactions. Language *might* just be like that. Other scientific theories certainly seem just outside of what our intuitive cognitive systems can handle, and yet we discover and use them. Quantum mechanics is a nice example: we find it highly counterintuitive “why” or “how” something acts both like a particle and a wave – impossible to intuit, but eminently scientific. Physics sometimes adopts a “shut up and calculate” mentality (Mermin 1989) to push back against the angst of failing to find good intuitive explanations for the theory. That doesn't stop the field from doing science.

But on this point, none of the authors responded to my comments on the shallowness of those explanations that are supposed to be so central to Chomsky's approach. There are *no* theories of how Principle C or A-chains or whatever are encoded in human brains, much less genomes. So while some bask in the glory of explanations based on those pieces, others who want one little step of further explanation will be left empty-handed. In my own view, that is too fragile a kind of explanation to use as the knock-down advantage of the approach. Worse, there are now quite a few examples where the explanations provided by Chomsky's approach probably aren't even in the right ballpark. For example, "island constraints" in syntax seem very well explained by other factors like frequency, memory, discourse, or pragmatics (Kluender 1992, Kluender & Kutas 1993, Kehler 2002, Chaves & Putnam 2020, Goldberg 2006, Liu et al. 2022b, Winckel 2021, Ambridge & Goldberg 2008, Abeillé et al. 2020, Liu et al. 2022b,a, Cuneo & Goldberg 2023). This stands in contrast to the elaborate machinery constructed by generative syntax. If the explanations provided are wrong, it makes it hard to argue they were a key, defining strength of the approach. Some of us would prefer rigorous comparison between theories with very different starting assumptions, rather than celebration of merely theory-internal chains of "explanation".

Several authors have also pointed out that modern language models can learn "impossible languages" (Milway 2023, Moro et al. 2023), although nobody seems to think it's important to first know how much data these architectures (or nearby ones) would require in order to learn "possible" versus "impossible" languages. There is historical irony to this move because, without missing a beat, the field switched from saying that innateness was true because learning was impossible ("poverty of stimulus"), to saying that learning can't be right because it works too well. But also, simultaneously somehow, we should doubt the models since they don't work on small amounts of data? A related tangle from the "abundance of the stimulus" has also been around in generative circles for a while (Babyonyshev et al. 2001), essentially saying that slow learning of frequent constructions is evidence for innateness too. Of course, nobody says what timing for learning would be evidence *against* innateness.

Even if the right model has constraints enforcing some typology, as generative syntax claims, that does not imply that the best scientific method will be to include these constraints from the start. If you want to figure out how to juggle knives – a constraint not to touch the blades – you might do well to start by juggling balls (no constraints) really well. Similarly, approaches to constrained optimization often care quite a bit about the unconstrained solution. The reason is that if you ignore constraints, you can often come up with approximately right solutions, which can then be refined. If you try to take constraints into account too early, you might have a harder time discovering the key pieces and dynamics,

and could create a worse overall solution. For language specifically, what needs to be built in innately to explain the typology will interact in rich and complex ways with what can be learned, and what other pressures (e.g. communicative, social) shape the form of language. If we see a pattern and assume it is innate from the start, we may never discover these other forces because we will, mistakenly, think innateness explained everything.

More troubling, the idea of “impossible languages” has never actually been empirically justified. Nobody knows what the space of possible languages is. There are no examples where universals have been shown to be due to something specific to language through rigorous comparison to non-linguistic domains. And many hypotheses turn out to be wrong. The example of recursion and Pirahã is probably the most striking case of a universal that wasn't. Pullum (2023) points to a number of languages which were argued not to have recursive embedding, even *before* Hauser et al. (2002)'s claims that recursion was distinctly human and universal. I pointed to compelling reviews that very little which has claimed to be universal actually is (Evans & Levinson 2009), and that the claims are not sufficiently justified statistically (Piantadosi & Gibson 2014).

Katzir (2023) does name a few specific universals, but the examples do not make a good case for innateness. One is about WH-questions, and I pointed above to non-syntactic theories of these. One is that there are no palindrome-like phonological sequences. Other hypotheses about the absence of palindromes – likely facts about how memory or sequential processing works – are plausible, and the field should care about comparing these alternatives. I am not aware of work on exactly the right comparison (mirror vs. not), but mirror-repetitions sequences are possible for kids and monkeys to learn, though monkeys show substantial difficulty especially compared to kids (Jiang et al. 2018). So if the claim is that those kinds of patterns are genetically impossible, that's probably not true; if the claim was that something linguistic, as opposed to more generally cognitive, made them hard, that's probably not the right explanation either. Katzir (2023) contrasts palindromes with hierarchical processes, as another claimed part of innate syntax, but there is evidence for hierarchical behavior in other primates (Ferrigno et al. 2020, Voloh et al. 2023) and even crows (Liao et al. 2022), making hierarchy unlikely to be about innate syntax. Katzir (2023) points to conservativity of quantifiers as another universal, but the child learning studies of these quantifiers provide mixed results (Spenader & De Villiers 2019), and many properties of quantifiers, including conservativity and monotonicity, can be derived from more general principles (Steinert-Threlkeld & Szymanik 2019, Carcassi et al. 2021, van de Pol et al. 2021, Steinert-Threlkeld 2021). The assertion that any of these phenomena are known to be innate linguistic universals is an Olympic triple jump over a contested set of hypotheses.

Several authors also point to places where some implementations of language models show non-human-like judgments of acceptability. It is useful to find examples that models mess up, but it really can't serve as a form of *comparison* between theories. There are, also, plenty of tasks where language models do *better* than anything using theories based on generative grammar, like translation, summarizing, and interpretation. Single sentences can't serve as a comparison of theories until we are specific on (i) what the important set of phenomena are, and (ii) how specific implementations of e.g. Chomsky's minimalism do on the corresponding sentences. Part of my complaint was that generative linguists don't typically have implementations and don't quantitatively compare theories, so sentences they fail on are just anecdotes.

Moreover, failures on examples at this point may not mean much since the approach of modern neural networks is so new. It very well could be that closely related architectures will succeed on these sentences. Kodner et al. (2023) tries to say that the architectural changes we build into the next round of neural networks *are* Chomsky's universal grammar ("But what are these biases, principles, and limitations [we can build into neural networks] if not some form of the Universal Grammar?"). But I think that's deeply mistaken. There are no doubt *some* principles required for language. The question is whether they are language-specific (or syntax-specific), innate, and whether they have the form that Chomsky and similar theorists have said, as opposed to lower-level principles that work through emergence. I am not sure that anyone could claim with a straight face that Chomsky's theory of Universal Grammar was right all along because one neural network (e.g. transformers) works better than another (e.g. LSTMs).

Several authors also pointed to differences in the amount of data between current models and children's input. It is certainly true that modern language models are currently trained on more data than children receive. But Chomskyan authors write about this as though the models are known to *need* that much data for syntax specifically. That seems very unlikely to be true. No authors who highlighted this point about data even engaged the hypothesis that most of this data is probably for semantics rather than syntax. If it's hard for large language models to learn semantics from text, that wouldn't tell us much about how real language acquisition works because children independently receive all kinds of other information sources about meaning. AI companies use as much data as they can find because it seems to help models on the rich kinds of tasks that they want to solve, often involving reasoning and complex discourse or conversation. But if we were only concerned with capturing syntax, it may be that much less data is required. A nice picture of learning some sequential regularities from hundreds

of thousands of words was published this past summer in the NY Times,¹⁷ but the BabyLM challenge should tell us a lot more. Until the space of architectures is well-explored, the question of how far into syntax these kinds of models can get is entirely open.

Kodner et al. (2023) also disputed my claim that language models work over an “unconstrained space”. A better term might have been “relatively unconstrained” because there are constraints and priors in any learning system. The point is that those priors do not seem to be very strong or limiting (which is precisely why others complained about them learning “impossible” languages). They also need not be stated symbolically, as they are in Chomskyan theories. Kodner et al. (2023) erroneously connect the lack of constraints to the amount of data, claiming that “every” algorithm which learns over a broad (computable) class of languages “requires infeasible amounts of time and data.” This is a strange critique because my lab recently showed in Yang & Piantadosi (2022) that tiny amounts of data could suffice for learning key computations needed in grammar out of a huge space of possible algorithms – and Kodner led a reply to the paper, although he incorrectly summarized the basic framework our model uses (Piantadosi & Yang 2022). It is not true that large hypothesis spaces always require huge amounts of data, in part because data points can be very informative: each single bit of information in the data can cut the space of likely hypotheses in half.

Overall, I was disappointed in some aspects of the responses: nobody said much about how this technology *should* affect our linguistic theories. As a result, most of the points that these authors raised cut orthogonally to the key claims in my paper. Few responded to what I see as the primary failings of Chomskyan linguistics, including aspects like avoidance of probability and constructions, informality, failure to compare to different starting points, incorrect learnability claims, shallowness of “why” answers, failures of general predictions about typology, lack of clearly articulated and justified universals, lack of quantitative comparison to alternative models even on syntactic phenomena of interest, etc. These are issues that linguists who work in that tradition can fix.

I am optimistic, though, about the future of linguistics because there are other approaches to the field. In particular, I agree with Kodner et al. (2023)'s title, that “linguistics will thrive in the 21st century”. There are now an incredible set of ideas to be integrated into a new, interdisciplinary science of language. It is especially exciting to see language at the center of AI, which it well deserves. But language wasn't put at the center of the world by the theories of generative syntax. The kind of linguistics that will thrive is one that is interdisciplinary,

¹⁷Let us show you how GPT works – Using Jane Austen by Aatish Bhatia.

embraces new ideas, rethinks old assumptions, and integrates what we discover about the power of learning. There is no future for a corner of linguistics that slams the door and keeps repeating to itself that it, and it alone, has language figured out.

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Steven T. Piantadosi

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Name index

- Aarne, Antti, 199
Abeillé, Anne, 388
Acedo-Matellán, Victor, 227, 228
Ács, Judit, 335
Adger, David, 368
Ahmed, Nur, 354
Aikhenvald, Alexandra Y., 43
Alexiadou, Artemis, 226, 228, 229,
247, 252
Alves, Maxine, 314
Amaral, Luiz, 42, 59
Ambridge, Ben, 388
Anderson, Philip W., 380
Angluin, Dana, 332
Archangeli, Diana B., 122
Arvaniti, Amalia, 90, 91
Askedal, J. O., 144
Aslin, Richard N., 370
Astuti, Rita, 118
Atkinson, Quentin D., 203
Austin, John L., 91
Austin, Peter, 25

Babyonyshev, Maria, 388
Bach, Emmon, xv
Bailey, Laura R., xiii
Baldrige, Jason, 325
Bárány, András, xiii
Barbosa, Plinio, 90, 93
Bargmann, Sascha, xvi
Barker, Chris, 155
Barnes, Jonathan, 91

Baroni, Marco, 359–362, 374
Barrett, David, 366
Bartlett, Tom, 32, 37
Bates, Elizabeth, 376, 378
Bateson, Patrick, 112
Bauer, Laurie, xiii, 203
Baumont, J., 385, 386
Beavers, John, 226, 227
Behaghel, Otto, 184
Behme, Christina, 353, 379
Belinkov, Yonatan, 361
Bender, Emily M., 362, 365
Bentley, Delia, 227–229, 231, 235,
239, 241, 242, 250, 253, 259,
261, 263, 264, 267
Berent, Iris, 111, 113–117, 119, 121–125
Bering, Jesse M., 117
Bertinetto, Pier Marco, 226, 239, 249
Berwick, Robert C., 377
Bhatia, Sudeep, 365
Bibolotti, Benigno, 344
Bjorklund, David F., 117
Blackburn, Patrick, 325
Blasi, Damián, 381
Blench, Roger, 86
Blevins, James, 53
Block, Ned, 365
Bloom, Paul, 116, 119
Bod, Rens, 359
Boden, Margaret A., 381
Boersma, Paul, 313
Bole-Richard, Rémy, 86

Name index

- Bolinger, Dwight, 332
Bommasani, Rishi, 354, 362, 365
Bonami, Olivier, xiii
Borsley, Robert D., xi
Bos, Johan, 168, 325
Bouma, Gosse, 160
Bowern, Claire, xiii
Bowman, Samuel R., 361, 364–367,
374
Brachman, Ronald J., 324
Bresnan, Joan, 25, 227
Brocher, Andreas, 253
Brown, Guy J., 90
Brown, Tom, 354
Bruderer, Alison G., 123
Bruening, Benjamin, 327
Burge, Tyler, 212, 220

Carbin, Michael, 373
Carcassi, Fausto, 389
Cardenosa, Jesús, 328
Carlson, Barry F., 276, 279–281
Carnie, Andrew, 372
Carruthers, Peter, 116
Castañeda, Hector-Neri, 212
Caucheteux, Charlotte, 365
Cennamo, Michela, 226, 228, 229,
250, 255, 264
Centineo, Giulia, 227, 228
Chang, Kent K., 362
Chater, Nick, 372, 374, 377, 378
Chaves, Rui P., 159, 388
Chen, Sihan, 201, 203, 315
Chen, Stanley F., 369
Chernela, Janet, 43
Chi, Ethan A., 365
Chierchia, Gennaro, 226, 247
Chisholm, Roderick M., 212
Chomsky, Noam, ix, xi, xv, 3, 28, 51,
91, 111, 112, 125, 327, 331, 332,
362, 363, 368–370, 375, 377,
380
Christiansen, Morten H., 359, 365,
371, 374, 377, 378
Clark, Alexander, 371, 374
Cohen, Antonie, 91
Collinder, Björn, 197
Collins, Chris, 370, 380
Collins, Katherine M., 366
Collobert, Ronan, 332
Comrie, Bernard, 47, 226
Connell, Bruce, 86, 92
Contreras Kallens, Pablo, 371
Cosmides, Leda, 116
Coupé, Christophe, 378
Couper-Kuhlen, Elizabeth, 90
Cover, Thomas M., 370
Crain, Stephen, 371
Croft, William, 368
Crutchfield, James P., 380
Culicover, Peter W., 371, 372
Cummins, Fred, 90, 93
Cuneo, Nicole, 388
Curry, Haskell B., 147, 325, 328
Cysouw, Michael, 196

D'Ausilio, Alessandro, 123
Dąbrowska, Ewa, 378
Dahl, Östen, 202, 203
Dale, Rick, 203, 378
Dalrymple, Mary, 167, 361
Darnell, Michael, 283, 300
Dasgupta, Ishita, 365
de Carvalho, Fernando O., 310, 311
De Marneffe, Marie-Catherine, 359
De Miguel, Elena, 228, 263
De Villiers, Jill, 389

- Delétang, Grégoire, 363
Denić, Milica, 378
Derbyshire, Desmond C., 46, 47, 51,
197
Deutscher, Guy, 191, 197
Dilley, Laura Christine, 89
Dilts, Philip, 276
Dixon, Robert M. W., 25, 197
Doak, Ivy G., 276
Dobrin, Lise, 24
Doron, Edith, 228
Dowty, David R., 226, 239, 242, 253
Dreger, Alice, 32, 35
Dryer, Matthew S., xv
Duanmu, San, 92
Dutoit, Thierry, 91

Eberhard, David M., 202
Edelman, Shimon, 359, 362, 365, 382
Elbourne, Paul, 160, 167
Eldan, Ronen, 367
Elman, Jeffrey L., 358, 361, 365, 377,
382
Engels, Friedrich, xiv
Englebretson, Robert, 197
Epstein, Sam, ix
Ergin, Rabia, 203
Evans, Nicholas, xv, 26, 318, 378, 389
Everaert, Martin BH, 375
Everett, Caleb, 306, 307, 311, 313–316,
378
Everett, Daniel L., 6, 7, 12, 14, 17,
30, 31, 36, 37, 41–44, 48, 50,
51, 53–60, 62, 64, 66, 75, 77,
81, 86, 93, 133, 180, 195, 275,
300, 306, 307, 310, 316, 378
Everett, Keren M., 306, 310, 311
Fanselow, Gisbert, xvi

Fedorenko, Evelina, 376
Feigenson, Lisa, 113, 114, 120, 121
Felizes, Pedro Vaz, 68
Fernández-Lagunilla, Marina, 228,
263
Ferrigno, Stephen, 389
Feys, Robert, 325, 328
Filimonova, Elena, xv
Fitch, W. Tecumseh, ix, xi
Fitch, William., 122
Fodor, Jerry A., 324, 327, 329, 332,
376
Fokker, Jeroen, 329
Foley, William A., 25, 196
Folli, Raffaella, 226, 228, 229, 250
Frank, Michael C., 9, 306
Frankfurt, Harry G., 216
Frankle, Jonathan, 373
Freidin, Robert, xv
Fujisaki, Hiroya, 91
Futrell, Richard, 12, 14, 18, 44, 66, 180,
306, 359, 363, 378

Gage, Philip, 332
Galves, Antonio, 90
Gauthier, Jon, 366
Ge, Ruifang, 359
Geiping, Jonas, 367
Gelman, Susan A., 116
Gerdts, Donna B., 276, 283, 284, 300
Gervain, Judit, 124
Gibbon, Dafydd, 86, 88–93, 98, 101
Gibson, Edward, 12, 14, 17, 19, 41–43,
59, 62, 64, 376, 378, 389
Gil, David, 6, 18, 180, 181, 187, 202,
203
Gill, Wayne, 343, 347
Givón, Talmy, 25, 58, 191, 197
Glass, James, 361

Name index

- Goddard, Cliff, 335
Godfrey-Smith, Peter, 385
Gold, E. Mark, 372
Goldberg, Adele E., 359, 362, 388
Goldberg, Yoav, 356
Goldin-Meadow, Susan, 18
Goldstein, Ariel, 365
Goldstein, Jeffrey, 379
Goldstein, Tom, 367
Gómez, David M., 124
Gonen, Hila, 365
González-Vergara, Carlos, 228, 263
Goodman, Joshua, 369
Goodman, Nelson, 377
Goodman, Noah D., 370, 376
Gordon, Matthew K., 309
Grace, George W., 187, 202, 203
Grand, Gabriel, 365
Greenberg, Joseph H., 121
Griffiths, Sascha, 93
Grohmann, Kleantes K., ix
Gross, Charles, 34
Gulordava, Kristina, 356, 368

Hahn, Michael, 378
Haider, Hubert, xv
Hale, Kenneth, 25, 186
Halle, Morris, 91
Hardt, Daniel, 167
Harley, Heidi, 228, 229, 250
Harris, Randy Allen, xi, 24, 353
Haspelmath, Martin, 203, 226, 253, 264
Hauser, Marc D., ix, xi, xvi, 1–4, 6–9, 18, 27, 378, 389
Hawkins, John A., xv
Hay, Jennifer, 203, 226, 239, 242
Hayes, Bruce, 122, 124
Hays, David G., 359

Heavey, Christopher L., 376
Heidinger, Steffen, 263
Heine, Bernd, 349
Heinrichs, Arlo, 48
Henrich, Joseph, 202
Hérault, Georges, 87, 88
Hess, Thom, 278
Hewitt, John, 374
Hill, Felix, 359, 365
Hirschberg, Julia, 92
Hockett, Charles F., 376, 378
Höllein, Dagobert, 144
Hood, Bruce, 117
Hoover, Jacob Louis, 364
Hopcroft, John E., 3
Hopper, Paul J., 277
Hornstein, Norbert, ix, 29, 43, 44, 59
Hosseini, Eghbal A., 367
Hossenfelder, Sabine, 386
Hu, Jennifer, 363
Huang, Norden E., 89
Huddleston, Rodney, 52
Hukari, Thomas E., 276
Hume, David, 215
Hurford, James R., 187
Hurlburt, Russell T., 376
Hutter, Marcus, 372

Iacobini, Claudio, 234, 255
Inden, Benjamin, 90

Jackendoff, Ray, xvi, 6, 8, 18, 53, 180, 183, 184, 227, 239, 252, 359, 362
Jacob, François, 122
Jacobs, Robert A., 376
Jacobson, Pauline, 161, 325
Jakubiček, Miloš, 227, 229, 264
Jansche, Martin, 92

- Jeffreys, Harold, 385
Jelinek, Frederick, 384
Jespersen, Otto, 226
Jezek, Elisabetta, 226, 228, 229, 250
Jiang, Xinjian, 389
Jiménez-Fernández, Angel, 243, 263
Johnson, David, 377
Johnson, Keith, 90
Johnson, Kent, 372
Johnson, Kyle, 160, 170
Joshi, Aravind K., 325
Jurafsky, Dan, 365
- Kailuweit, Rolf, 226, 248, 261
Kalmár, Ivan, 25, 58
Kanerva, Jonni, 227
Kanwisher, Nancy, 365
Kaplan, Jared, 366
Karlsson, Fred, 65, 93, 191, 193, 197
Katz, Jerrold, 329, 332
Katz, Yarden, 379
Katzir, Roni, 389
Kayne, Richard S., xv
Kehler, Andrew, 388
Keil, Frank C., 116
Kemmer, Suzanne, 227, 244
Kemp, Charles, 370, 378
Kempson, Ruth M., 80
Kennedy, Christopher, 168
Kim, Najoung, 360, 361
Kim, Taeuk, 374
Kinkade, M. Dale, 276, 278, 287, 294,
300
Kinzler, Katherine D., 119
Kiparsky, Paul, 53, 226, 240, 254, 327,
333
Kirby, Kathryn R., 202
Kirby, Simon, 19, 378
Kluender, Robert, 388
- Knuth, Donald E., 331
Kocijan, Vid, 356
Kodner, Jordan, 387, 390, 391
Koehn, Ed, 51, 197
Koehn, Sally, 51, 197
Koenig, Jean-Pierre, xvi
Koller, Alexander, 365
Koontz-Garboden, Andrew, 228
Koptjevskaja Tamm, Maria, 51
Kornai, András, 64, 324, 325, 333–
335
Koza, John R., 360
Krakauer, David C., 112
Kroeber, Paul Demarest, 276, 278,
279, 283, 286, 289, 290, 300
Kubota, Yusuke, 151, 155, 156, 158–
160, 163, 164, 170
Kuipers, Aert H., 300
Kuno, Susumu, 158, 163
Kutas, Marta, 388
Kuteva, Tania, 349
Kwiatkowski, Tom, 359
- La Fauci, Nunzio, 259
Labelle, Marie, 226, 228
Lake, Brenden M., 365, 366, 370
Lambek, Joachim, 172, 173
Langacker, Ronald W., 28
Langley, Pat, 360
LaPolla, Randy J., 76, 80, 227, 252,
257, 258
Lappin, Shalom, xi, 371, 377
Lasnik, Howard, 28, 332, 363, 371–
373
Laszakovits, Sabine, xiii
Laurence, Stephen, 371
LeCun, Yann, 324, 333, 354
Ledgeway, Adam, 259
Legate, Julie Anne, 371, 372

Name index

- Legendre, Géraldine, 226, 242, 361,
374, 382
Legg, Shane, 372
Levesque, Hector, 324
Levin, Beth, 225–227, 239, 244, 250,
267
Levin, Nancy S., 170
Levin, Samuel R., 90
Levine, Robert, 151, 155, 156, 158–160,
163, 164, 170
Levinson, Stephen C., xv, 26, 318,
378, 389
Lewalter, Thorsten, 89
Lewis, David, 212, 326
Lewis, Harry R., 3
Li, Yuanzhi, 367
Liang, Percy, 359
Liao, Diana A., 389
Lieberman, Mark, 57, 90, 98
Lidz, Jeffrey, 371, 372
Lin, Yongjie, 374
Linzen, Tal, 359, 361, 363, 374
Lipson, Hod, 360
Liu, Nelson F., 360
Liu, Yingtong, 159, 388
Lobina, David J., 27
Loporcaro, Michele, 259
Lüderitz, Berndt, 89
Lupyan, Gary, 203, 376, 378
Lyons, John, 227, 261

Maas, Andrew, 365
MacWhinney, Brian, 376, 378
Maddieson, Ian, 308, 309, 314
Madora, Keren, 306
Mahoney, Patrick, 159
Mahowald, Kyle, 356, 366, 374, 376,
378
Mameli, Matteo, 112

Mammeri, Mouloud, 201
Manente, Mara, 228
Manning, Christopher D., 359, 361,
364, 369, 374, 379
Marblestone, Adam H., 365
Margolis, Eric, 371
Martin, Fabienne, 226, 248, 261
Marvin, Rebecca, 363
Mascalzoni, Elena, 126
Matras, Yaron, 343
Matsuyama, Tetsuya, xvi
Mattina, Anthony, 276, 279, 293
Mattingly, Ignatius G., 121
May, Robert, 154
McCauley, Stewart M., 359
McCawley, James D., xv
McClelland, James L., 361, 362, 365,
370, 371, 379, 382
McCloy, Daniel, 307
McManus, Kevin, xiii
McNally, Louise, 241
McWhorter, John H., 203
Meier, Ernst, 200
Meir, Irit, 203
Mengarini, Gregory, 276, 279
Merchant, Jason, 158
Merlo, Paola, 226
Mermin, David, 387
Métraux, Alfred, 344
Michelson, Karin, xvi
Michelson, Truman, 344
Mikolov, Tomas, 324, 365, 368
Miller, Philip, 163
Milway, Daniel, 386–388
Mithun, Marianne, 25, 58
Mollica, Francis, 366, 378
Montague, Richard, 154, 326
Montler, Timothy, 279
Mooney, Raymond, 359

- Moore, George Edward, 212
Moortgat, Michael, 173
Moran, Steven, 307
Morikawa, Hiroyoshi, 91
Moro, Andrea, 388
Möttönen, Riikka, 123
Müller, Stefan, iii, xi, xiii, xv, xvi,
xviii, 19, 328, 361
Murphy, Gregory L., 365, 366
Murty, Shikhar, 359
Muskens, Reinhard, 160

Nadel, Lynn, 381
Nash, David, 25
Nettle, Daniel, 203
Nevins, Andrew, 7, 29, 31, 43, 51, 58
Newell, William Wells, 200
Newman, Stanley, 283, 300
Newmeyer, Frederick J., 368
Newport, Elissa L., 367, 370
Nichols, Joanna, 263
Nieminen, Tommi, 90, 93
Noce, Sergio, 314
Nordenskiöld, Erland, 344
Norvig, Peter, 368, 369
Nöth, Winfried, 153
Nowak, Martin A., 112
Nunes, Jairo, ix

O'Dell, Michael L., 90, 93
O'Neil, Wayne, 26, 58
O'Neill, Michael, ix
Oehrle, Richard T., 154, 173
Ohala, John, 90
Oliveira, Miguel, 58

Packard, Norman H., 359
Papadimitriou, Christos H., 3
Papadimitriou, Isabel, 365

Partee, Barbara, 152, 327
Patel, Roma, 365
Pater, Joe, 362, 382
Patterson, Karalyn, 371
Pavlick, Ellie, 359, 361, 365
Pawley, Andrew, 65, 187
Pearl, Lisa, 371
Peirce, Charles Sanders, 144, 173
Pensalfini, Rob, 25
Pereira, Fernando, 359, 368
Perfors, Andrew, 371, 374
Perlmutter, David. M., 248
Perry, John, 212, 220
Petersen, Erika, 365
Piantadosi, Steven T., xviii, 12, 319,
359, 365, 366, 370, 376, 378,
389, 391
Piattelli-Palmarini, Massimo, 381
Pierrehumbert, Janet Breckenridge,
92
Pietroski, Paul, 371
Pinker, Steven, xv, 116, 225, 370
Piñon, Christopher, 253
Pires, Telmo, 365
Plank, Frans, xv
Platt, Melanie, 117, 124
Pollard, Carl, 154
Port, Robert, 90, 93
Postal, Paul, 353
Potts, Christopher, 365
Pratt, Vaughn, 172
Precoda, Kristin, 308
Prince, Alan, 90, 98, 361, 370
Priva, Uriel Cohen, 311
Procter, Paul, 331
Przepiórkowski, Adam, 144
Pulleyblank, Douglas G., 122
Pullum, Geoffrey K., ix, xi, xv, 1, 2,
18, 25, 46, 47, 52, 53, 163, 195,

Name index

- 197, 306, 353, 361, 364, 371,
378, 380, 389
- Pustejovsky, James, 239
- Putnam, Michael T., 159, 388
- Pylyshyn, Z. W., 376
- Quilty-Dunn, Jake, 376
- Radford, Alec, 368
- Rappaport Hovav, Malka, 226, 239,
250, 267
- Rathcke, Tamara, 90
- Raviv, Limor, 203
- Rawski, Jon, 385, 386
- Reali, Florencia, 371, 378
- Recski, Gábor, 335
- Redington, Martin, 374
- Reed, Homer B., 376
- Regier, Terry, 378
- Regolin, Lucia, 126
- Reis, Marga, xvii
- Restall, Greg, 172
- Richards, Blake A., 365
- Richie, Russell, 365
- Ridley, Matthew, 112
- Rodrigues, Cilene, 43
- Roebuck, Hettie, 376
- Rogers, Anna, 361
- Rogers, Timothy T., 365
- Rooth, Mats, 152
- Ross, John Robert, xv, 162, 381
- Ross, Malcolm D., 187
- Rossi, Simone, 122
- Rossini, Nicla, 86, 92
- Rothemberg, Mira, 228
- Rule, Joshua S., 370
- Rumelhart, David E., 362, 370
- Saffran, J. R., 370
- Sag, Ivan A., 154, 362
- Sakel, Jeanette, 12, 24, 55, 57, 86, 341–
343, 345, 348
- Salffner, Sophie, 86
- Samardžić, Tanja, 226
- Samarin, William J., 55
- Samuels, Richard, 112, 113
- Sandalo, Filomena, 60, 62, 195
- Sandler, Wendy, 18
- Sauerland, Uli, 9, 14–16, 18, 59
- Schäfer, Florian, 226, 248, 261
- Schmid, Monika S., xiii
- Schmidt, Michael, 360
- Scholz, Barbara C., 25, 197, 371, 378
- Schönfinkel, Moses, 325
- Schrimpf, Martin, 364–366
- Schuller, Rudolph, 344
- Schütze, Hinrich, 332, 369
- Schuyler, Tami, 159
- Schwartz, Saul, 24
- Selkirk, Elisabeth, 91, 95
- Selting, Margret, 90
- Setoh, Peipei, 118
- Shain, Cory, 364
- Shan, Chung-chieh, 155
- Shannon, Claude E., 369, 370
- Shattuck-Hufnagel, Stefanie, 91
- Sheehan, Michelle, xiii
- Sheldon, Steven N., 44
- Shen, Yeshayahu, 203
- Shen, Zheng, xiii
- Shibatani, Masayoshi, 287
- Shoemaker, Sydney, 212
- Shtulman, Andrew, 125, 126
- Siegelmann, Hava T., 363
- Siskind, Jeffrey M., 359
- Slobin, Dan I., 187
- Smailus, Ortwin, 201
- Smalle, Eleonore H. M., 123

- Smith, Kenny, 378
Smith, Norval, 47
Smolensky, Paul, 226, 361, 374, 382
Soare, Robert I., 27
Sobolak, Frances, 276
Socher, Richard, 365
Solan, Zach, 359, 371
Solomonoff, Ray J., 372
Sontag, Eduardo D., 363
Spalek, Alexandra A., 241
Spelke, Elizabeth S., 119, 126, 376
Spenader, Jennifer, 168, 389
Springer, Ken, 116
Squartini, Mario, 226, 239, 249
Stabler, Edward, 370, 380
Stapert, Eugenie, 12, 57
Stassen, Leon, 51
Stearns, Laura, 12
Steedman, Mark, 163, 325, 359
Steinert-Threlkeld, Shane, 378, 389
Sternefeld, Wolfgang, xvii
Stowell, Timothy, 53
Strubell, Emma, 362
Sugihara, George, 359
Sutskever, Ilya, 332
Syder, Frances, 65
Szabolcsi, Anna, 325, 328
Szymanik, Jakub, 378, 389

't Hart, Johan, 91
Tabor, Whitney, 374
Takens, Floris, 359
Tarski, Alfred, 326
Tchagbale, Zakari, 92
Tegmark, Max, 360
Tenenbaum, Joshua B., 370
Tenney, Ian, 360, 361, 374
Tesnière, Lucien, 359
Thomason, Lucy, 276
Thomason, Sarah G., 275, 284, 300, 306
Thompson, Bill, 378
Thompson, Laurence C., 277, 283, 286, 287
Thompson, M. Terry, 283, 286, 287
Thompson, Sandra A., 277
Thompson, Stith, 199
Thurston, William R., 202
Tilsen, Sam, 90
Todd, Neil P. McAngus, 90
Tomasello, Michael, 359, 362
Tooby, John, 116
Traunmüller, Hartmut, 90
Trecca, Fabio, 318
Trim, John Leslie Melville, 91
Trinh, Tue, xvii
Trudgill, Peter, 203
Tubino-Blanco, Mercedes, 243, 263
Turing, Allan M., 356

Udayana, I. Nyoman, 227
Udrescu, Silviu-Marian, 360
Ullman, Tomer D., 370

Vallortigara, Giorgio, 126
van de Pol, Iris, 389
van Eijk, Jan P., 278
van Heijenoort, Jean, 325
van Hout, Angelik, 242
van Noord, Gertjan, 160
Van Valin Robert D., Jr., 75, 76, 80, 227, 228, 231, 252–255, 257, 258, 266
Varley, Rosemary, 376
Vaswani, Ashish, 332, 354, 361
Venable, William N., 312
Vendler, Zeno, 252
Vitányi, Paul, 372

Name index

- Vivanco, Margot, 226, 228, 249
Voegelin, Carl F., 55
Vogt, Hans, 278, 290, 292, 294, 300
Voloh, Benjamin, 389
Voorhoeve, C. L., 201
- Wagner, Petra, 90
Wang, Avery Li-Chun, 89
Wang, Jinjing, 113, 114, 120, 121
Warstadt, Alex, 361, 364–367, 374
Wasow, Thomas, xi
Waterfall, Heidi, 359, 362
Watkins, Kate E., 123
Wedel, Andrew, 311
Wei, Jason, 379
Weissweiler, Leonie, 359
Wells III, Rulon S., 331
Wertz, Annie E., 118
Wexler, Kenneth, 371, 372
Wichmann, Søren, 203, 313
Wickham, Hadley, 16, 313
Widmer, Manuel, 65
Wierzbicka, Anna, 335
Wilcox, Ethan, 360, 366, 372, 382
Wilkins, David P., 254
Winckel, Elodie, 388
Wittenberg, Eva, 8, 18, 180, 183, 184
Wittgenstein, Ludwig, 112
Wolff, Phillip, 239, 266
Wood, Randall, ix
Wray, Allison, 202, 203
Wright, Richard, 121
Wu, Yonghui, 333
Wynn, Karen, 118
- Yang, Charles D., 371, 372
Yang, Yuan, 370, 391
Ye, Hao, 359
Yule, George, 91
- Zaslavsky, Noga, 378
Zhang, Chiyuan, 373
Zhang, Yian, 366
Zipf, George Kingsley, 378
Zribi-Hertz, Anne, 226, 228, 237, 248,

From fieldwork to linguistic theory

Dan Everett is a renowned linguist with an unparalleled breadth of contributions, ranging from fieldwork to linguistic theory, including phonology, morphology, syntax, semantics, sociolinguistics, psycholinguistics, historical linguistics, philosophy of language, and philosophy of linguistics. Born on the U.S.-Mexican border, Daniel Everett faced much adversity growing up and was sent as a missionary to convert the Pirahã in the Amazonian jungle, a group of people who speak a language that no outsider had been able to become proficient in. Although no Pirahã person was successfully converted, Everett successfully learned and studied Pirahã, as well as multiple other languages in the Americas. Ever steadfast in pursuing data-driven language science, Everett debunked generativist claims about syntactic recursion, for which he was repeatedly attacked. In addition to conducting fieldwork with many understudied languages and revolutionizing linguistics, Everett has published multiple works for the general public: *Don't sleep, there are snakes*, *Language: The cultural tool*, and *How language began*. This book is a collection of 15 articles that are related to Everett's work over the years, released after a tribute event for Dan Everett that was held at MIT on June 8th, 2023.