

*Sapir Whorf Redux: what might just be right about Linguistic Relativity?*¹

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February 2011

I begin with an apology to those of you who came along² expecting the sequel to the talk I gave in December last year, in which I argued that the psychologist Richard Nisbett was wrong to believe that Asians think differently because of their culture: we're not going to get there this hour!³ This is because, in preparing for this lecture, I came across some problems and misunderstandings so vexing and wrong-headed that they make Nisbett's proposals seem attractive by comparison. So instead, I thought it would be more worthwhile to use the time to talk through some of them, and—by so doing—to introduce you to the kinds of linguistic questions with which I torture myself and my students on a regular basis.

This paper is largely composed of quotations, the perennial crutch of the semi-articulate. One of my favorites, from the author and humorist Robert Benchley, is also the most apposite I could think of for a talk that will be largely concerned with what I'll suggest are false dichotomies.

There are two kinds of people in the world: those who believe there are two kinds of people in the world, and those who don't (Robert Benchley).⁴

The main purpose of this presentation will be to argue that most of the key controversies in theoretical linguistics—as perhaps is true in other areas of science—are based on myths and misconceptions about natural classes: about which notions are intrinsically linked and which are only contingently associated

¹ Version published: February 21, 2011.

² This is the revised and expanded text of a talk I gave in early 2010 at the University of Sheffield, a contribution to the "Thursday Taster" series of the Centre for Linguistic Research. Although it probably has no future in any peer-reviewed journal, it still has some merit, I think, as it represents an attempt to find a middle way in the great Universals debate.

³ ([f you are interested, please see this post](#))

⁴ Not the author of Jaws, that was Peter Benchley. This is not Robert Benchley's only contribution: he is also credited with "Anyone can do any amount of work provided it isn't the work he is supposed to be doing at the moment" and "It took me fifteen years to discover that I had no talent for writing, but I couldn't give it up because by that time I was too famous; see below.

through ideology or by doctrinal practice. Specifically, I'll be concerned with the relationship between Language Universals, Universal Grammar and Linguistic Relativism, and some other controversial terms, and with the question of why people generally think these notions are in opposition. Also, just so my general theoretical position is clear, with why on earth anyone might think that (Theretical) Relativism is an attractive idea...

To set the stage, consider the labels in Figure 1: on the left, a set of notions associated with the people I characteristically hang out with, and with the journals I try to get published in; on the right, keywords—descriptors, if you like—that would generally be associated with functional and cognitive linguists, such as our erstwhile colleague, Ewa Dabrowska (now at the University of Northumbria), and which tend to inspire something close to fear and loathing in others. Many of the most vitriolic debates that afflict the fields of linguistics, philosophy, psychology and anthropology involve people hurling missiles across the line drawn between these notions, and running back for ammunition. That's definitely not the purpose of this lecture.



The two notions we might start with are LANGUAGE UNIVERSALS on the one hand, and LINGUISTIC RELATIVISM⁵ on the other, since it is in the opposition of these terms, taking an historical perspective, that we can most clearly see what appears to be a cycle, with Relativism in the ascendant before 1959, replaced by Chomskyan Universalism and UG, until recently—just as some of us thought it was safe!—when Relativism has made something of a comeback. So, for example, on one side of the debate, we have Martin Joos' famous words:

Languages [may] differ from each other without limit and in unpredictable ways. (Martin Joos, ca. 1958).

Or those of Edward Sapir (one of the key paragraphs cited to explain what has become known as the *Sapir-Whorf hypothesis*):

The fact of the matter is that the 'real world' is to a large extent unconsciously built upon the language habits of the

⁵ In this talk, I use the terms "relativism" and "relativity" interchangeably. Though the latter term may be etymologically more neutral, in practice proponents of linguistic relativity are also advocates of linguistic relativism.

group. No two languages are ever sufficiently similar to be considered as representing the same social reality. The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached... Sapir (1958 [1929])

For about 30 years following this quotation, it is fair to claim that the Chomskyan view was dominant:

All languages known and conceivable are cut from the same pattern....[The true job of linguistics] must be to develop an account of linguistic universals that, on the one hand, will not be falsified by the actual diversity of languages and, on the other, will be sufficiently rich and explicit to account for the rapidity and uniformity of language learning, and the remarkable complexity and range of the generative grammars that are the product of language learning (Noam Chomsky).

For many of us, these are still words to live by, but since the early 1980s there have been rumors of a counter-revolution, with a number of significant works, including *Rethinking Linguistic Relativity* (edited by John Gumperz and Stephen Levinson: (Gumperz and Levinson 1996)) as well as, more recently, the position paper of Nick Evans, and—once again—Steve Levinson: *The Myth of Language Universals*, which will form the centrepiece of this discussion. The following quote from Janet Pierrehumbert, cited by Evans & Levinson, and which directly recalls Joos, is typical of this new relativism:

Languages can differ systematically in arbitrarily fine phonetic detail...The vowel space – a continuous physical space rendered useful by the connection it establishes between articulation and perception – is also a physical resource. Cultures differ in the way they divide up and use this physical resource (Janet Pierrehumbert, cited in Evans & Levinson 2009).

Or, consider this from Pederson *et al* (1998):

Far more than developing simple habituation, use of the linguistic system, we suggest, actually forces the speaker to make computations he or she might otherwise not make...That is, the linguistic system is far more than just an available pattern for creating internal representations: to learn to speak a language successfully requires speakers to develop an appropriate mental representation which is then available for

non-linguistic purposes ((Pederson et al. 1998):586).

From what I've said so far, it may be clear where I stand: the general conclusion of this talk will be that the philosopher Jerry Fodor was right, when he quipped:

I hate relativism. I hate relativism more than I hate anything else, excepting, maybe, fiberglass powerboats. (Fodor, *Précis of The Modularity of Mind BBS* 1985)

Aside from the fact that I have no particular objection to fibreglass, this pretty much sums it up for me too. And so we could stop now: the answer to the question in the talk's subtitle being...to paraphrase Edwinn Starr: '[absolutely nothing](#)'). The first reason that I simply don't pack it in, or spend the rest of this hour justifying my and Fodor's distaste for relativism in all its forms—I'll do this, but not for the whole hour!—is, in the first place, that it would be uncharitable, not just to relativists in the audience, but to everyone here: you would have come along on the basis of false advertising to be fobbed off with a glib answer.

Another reason for continuing is that—through gritted teeth—I shall end up making a case for Linguistic Relativism (albeit in tightly attenuated form). The grounds for this are mostly those of grim practicality: because there's a urgent need to mobilise work on language documentation, and relativists tend to do such descriptive work better than universalists, for reasons that should become clear. However, there are also some good empirical reasons for this teeth-gritting, to the extent that the research I talked about in December supports a weak form of Linguistic Relativity (specifically, Dan Slobin's "*Thinking for Speaking*" hypothesis: Slobin 2003); in spite of my ideological commitments and preferences, I still believe in following the evidence.

A third reason for taking time on this is that there is a great deal of misunderstanding, both from the political left and, more disturbingly, from the far right, about the type of linguistics I'm advocating here. If both ends of the political spectrum agree that Universal Grammar is a bad thing, and or even that relativism is a good thing, I feel there's a duty to try to set things straight. To see what I'm talking about, consider the next few illustrations: the first from a consultant on diversity and multiculturalism, the second a quotation from a right-wing blogger:

From the left...



The scale of the misunderstanding (from Rosado 2004) (who would want to be close-minded?!)

From the right...

'Many conservatives regard Chomsky as a linguist who falters out of his field. Unfortunately, they are giving Chomsky too much credit. Chomsky's linguistics are as warped as his politics. As someone with a PhD in linguistics, I think I am qualified to judge his professional credentials. Prior to Chomsky, linguists engaged in a lot of data collection to understand the diversity of human language. I'm vehemently anti-PC, but in this case, I think the word 'diversity' is justified. There's a lot out there, and someone's got to catalog it...However, Chomsky rejected this approach. He wanted to look into something 'deeper' (academese for 'pretentious and nonexistent'). So he invented something called 'universal grammar' which is somehow programmed into us at birth. Now it is obvious to anyone who's studied a foreign language that there is no such thing as 'universal grammar': there are a lot of differences between any two languages' structures. How does Chomsky account for these differences? He claims that we formulate 'deep structures' in our heads using 'universal grammar'. Then we use 'transformations' to change these (invisible, nonexistent) 'deep structures' into 'surface structures' (which are what we actually say and write). There are innumerable problems with this...'

Indeed there *are*, though with the text, rather than with Chomsky: this rant goes on for many more paragraphs. It would be easy to dismiss the author of the post as an ignorant obscurant, were it not for the fact (a) that he has a PhD in Linguistics (which must count for something!); more seriously, (b) that his anti-Chomskyan views—though obviously extreme—are not so far removed from mainstream prejudice. As an example of this, a couple of months ago, I gave a version of last month's talk—*Whorf, Chomsky and the Student of Linguistics* (the title an *homage* to an article by George Steiner that is still very much worth reading for anyone interested in the Universals debate)—to an audience of applied linguists and language teachers on the other side of the world. The talk itself was well received, but I had to amend the title: the department chair told me that if Chomsky was mentioned, 'it would put a lot of people off, particularly teachers.'

The main reason, however, for not rejecting linguistic relativism outright is the fact that I have been fortunate to learn from great linguistics teachers and colleagues—all of them very smart people, and many

of them relativists, or at least, variously, anti-universalists, anti-generativists, or anti-nativists, which as I'll some take time to argue in the third part of this talk, are not the same thing at all.

Also, since this was advertised as a Taster Session—which is, as Andrew Linn suggested last week, an opportunity to give a covert inaugural lecture—I thought it might be helpful, especially to postgraduate students, to explain how it is that in spite of a good education, I've ended up a committed universalist. And so this is the subsidiary purpose of the first part of this talk: to explain, through a brief autobiographical sketch how one can end up a universalist—and generativist to boot—believing in a form of Universal Grammar, all the while remaining highly sceptical of claims about Language Universals, and seriously doubting whether all children converge on uniform end-state grammars. It's also an opportunity to put some faces to famous names. Andrew's talk was well-illustrated with dead Scandinavians—except for Chomsky, whose influence on my own presentation will be so obvious that he doesn't need a picture. My own brief autobiography is full of characters from Linguistics, Philosophy and Psychology, the majority of whom all very much alive, and largely research active.⁶

[Readers with no interest in my autobiography should skip ahead to page 5, second column]



As chance would have it, my first linguistics lecturer was in fact Steve Levinson, who in 1981 looked even more youthful than in this more recent picture: best known to most undergraduate students of subsequent generations for his work with Penny Brown on politeness, and for his CUP text book on Pragmatics, it was Steve's enthusiasm for linguistics, as well as that of my undergraduate director of studies, an equally youthful Nigel Vincent, that inspired me to continue with

⁶ The tragic exceptions are first, my original PhD supervisor, Osvaldo Jaeggli, himself a student of Chomsky's and most talented syntactician in his own right, second, my friend and most esteemed colleague Clive Perdue, and latterly—in the last few days (February 2011)—the key member of my final dissertation committee, Jean-Roger Vergnaud (also one of Chomsky's students). All of these individuals were remarkable linguists, sorely missed by those who knew them. Osvaldo was also the person that told me—only partly in jest—with reference to a power struggle between formalists and functionalists that was raging in the USC linguistics department at the time: "This is a battle for souls: you need to make your mind up." I took his Jesuitical advice, and wrote my thesis on Modern Irish syntax, leaving psycholinguistics and language typology for the summer vacations. The result has been a rather fractured, *zwiespaltig* academic career, with a foot in both camps, completely at home in neither...

Modern and Medieval Languages, when I should have been reading Law instead.⁷

Cambridge in the early 1980s was an interesting place to study linguistics, which was taught in the style summed up by Geoff Pullum in his 1984 commentary *If it's Tuesday, this must be Glossematics*.⁸ Pullum's article continued a debate that had started some years earlier, which, as he noted:

...concerned two different views of how linguistics should be taught. The distinction between the two views had been put into very sharp relief in a letter that David Lightfoot published in the *Times Higher Education Supplement* on the first day of 1982. Lightfoot referred to the belief in "eclecticism and a belief in the value of surveys" that seems to pervade linguistics teaching in Britain.

As well as I can judge, not much has changed in the interim, to the moderate frustration of those educated in the other style. The alternative, North American, approach involves teaching one theory, and teaching it well. No doubt both perspectives have their merits. For me, the chief downside of British eclecticism was that generative grammar was taught by linguists whose enthusiasm for Chomsky's approach was lukewarm and grudging at best: it *had* to be taught, but you didn't necessarily have to endorse it. Some, like Terry Moore, were openly hostile to generative grammar. Mostly though, TGG, as it was called then, was damned with faint praise. After such an introduction, it took me nearly four years of therapy to come to appreciate formal syntax again. Unusually, for two of those four years I was taught by philosophers moonlighting in a psychology department.

Barry Smith



Max Coltheart



Martin Davies



Margaret Harris

After Cambridge, I moved to London, and a year later I enrolled on a part-time MA at Birkbeck College:

Philosophy and Psychology of Language. This course, which unfortunately only ran three or

four times before three of the principal convenors left Birkbeck, was a co-production of the Philosophy and Psychology departments. The most inspirational teachers on the course were Martin Davies and Barry Smith (on the Philosophy side) and Max Coltheart and Margaret Harris (in Psychology).

⁷ My mother may never come to terms with this, but at this point it's far too late: Robert Benchley's other quote is relevant here: see footnote 3 above.

⁸ *Natural Language and Linguistic Theory*, 1984.

Barry tried to teach us elementary logic,⁹ and formal syntax using Terry Winograd's formidable (and formidably expensive) tome; Martin and Barry both straightened us out on basic issues in *Philosophy of Language and Philosophy of Mind*. The core module of the second year was based on one text: Jerry Fodor's *Modularity of Mind*, and the *BBS* article and peer commentaries derived from it, in which Fodor's distaste for relativism—as well as his genius for scathing metaphor—first came to my attention. I was also introduced to the work of William Marslen-Wilson and Lorraine Tyler, whose 1987 paper *Against Modularity* provided one of the key platforms for opponents of Fodorean modularity within *Cognitive Psychology*. A year later—and for two summers thereafter—I was lucky enough to work as a research assistant for William and Lolly, gaining some hands-on knowledge of experimental psycholinguistics, and further insight into the gulf between linguistic and psycholinguistic views of the world. This gulf is nicely alluded to by Tom Roeper in the following often cited quote:

'when psychological evidence has failed to conform to linguistic theory, psychologists have concluded that linguistic theory was wrong, while linguists have concluded that psychological theory was irrelevant (Roeper 1982).'

In 1987, I started my PhD at the University of Southern California, having been recruited by Jack Hawkins on a visit to Birkbeck, with the intention of studying processing explanations for language change. However, the absence of a real processing lab, and the intellectual attractions of committed generativists such as Osvaldo Jaeggli, Joseph Aoun and Jean-Roger Vergnaud (who arrived in my second year) won me over to formal syntax: so, after two years of 'generals courses' with Bernard Comrie, Larry Hyman and Doug Pulleyblank, I decided to focus on a theoretical description of a fairly understudied and interesting language—albeit one with lousy cuisine and lousier weather, namely, Modern Irish.

A point to note about Joseph Aoun, Jean-Roger



Joseph Aoun



Bernard Comrie



Jean-Roger Vergnaud



John A. Hawkins



Maria Luisa Zubizarreta

Vergnaud, and his wife Maria Luisa Zubizarreta (my dissertation committee members) is that they all were

⁹ The implicature here is intended to refer to our capacity for learning, not Barry's talent for teaching: see Green's (1974) discussion of double objects.

first educated in France; more importantly, they—and all their students at USC—were comparativists (in the linguistic sense): all of their students produced dissertations involving language data from a wide variety of languages—Indo-European for the most part, but also drawing on data from more exotic languages. This is important because generativists are often accused by descriptivist typologists, developmentalists and others, of being Anglocentric, and criticised for ignoring the facts of language diversity. This accusation that is completely belied by the fact that virtually every MIT syntax dissertation at least since 1980 has treated syntactic variation head-on, offering analyses of languages from every part of the world, and from every language family, the work of Mark Baker (Mohawk, Chichewa), Lisa Travis (Malagasy, Navajo), Norvin Richards, Maria Bittner (Greenlandic Eskimo), and Lisa Cheng (Mandarin), being the only best known examples of a commitment to understanding syntactic variation.

This, then brings me to the first ‘take-home message’ of the talk (a phrase I first learned from Martin Davies):

THM #1: Generativist linguistics is not Anglocentric in practice.

Since obtaining my PhD in 1991, I have had the continued good fortune of working with people whose exemplary research is normally rather clearly identified with one or other side in the Universalist/Relativist debate, as it is popularly understood.



First, in Germany between 1991-1993, I worked as a Post-Doc for Harald Clahsen who, with Pieter Muysken (who arranged for me a guest fellowship at the University of Nijmegen in 2003-2004), is perhaps best known for a joint assault on the idea that second language acquisition is constrained by Universal Grammar (Clahsen & Muysken 1986). At McGill from 1993, I worked closely with Lydia White, Lisa Travis and Mark Baker, all of whom have made persuasive arguments for the role of Universal Grammar in Language Typology, and in First and Second Language Acquisition. Then in 1999, thanks to my wife Ayumi Matsuo, who got a job there, I found myself for a couple of years at the Max Planck Institute for Psycholinguistics in Nijmegen, where I was surrounded by some very smart and distinguished linguists, none of whom thinks a great deal of

generative grammar, UG or modularity: the then co-directors of the MPI were the *genial* figures of Wolfgang Klein, Anne Cutler, Pim Levelt...and Steve Levinson. Melissa Bowerman must not go unthanked, either!

Given this privileged education, I’ve spent the time since trying to reconcile what appear to be incommensurate views without fudging the issues in compromise (something that only the English could consider a virtue!; as Churchill said, *The English never draw a line without blurring it*).

Moving from autobiography to more substantive issues, this next section is devoted to commentary on an article that has recently attracted a good deal of attention, one which makes a seductive case for linguistic relativism, but that also incorporates many of the misconceptions that dog constructive debate between those on either side of that ‘police line’ I alluded to at the outset.¹⁰ The article in question, which appeared in BBS in 2009, is entitled *The Myth of Language Universals: Language diversity and its importance for cognitive science*. In it, Nicholas Evans and Stephen Levinson construct a polemic that is at once anti-UG, anti-Universalist, and anti-abstract. It begins with the following paragraph:

Languages are much more diverse in structure than cognitive scientists generally appreciate. A widespread assumption among cognitive scientists, growing out of the generative tradition in linguistics, is that all languages are English-like but with different sound systems and vocabularies. The true picture is very different: languages differ so fundamentally from one another at every level of description (sound, grammar, lexicon, meaning) that it is very hard to find any single structural property they share. The claims of Universal Grammar, we argue here, are either empirically false, unfalsifiable, or misleading in that they refer to tendencies rather than strict universals. Structural differences should instead be accepted for what they are, and integrated into a new approach to language and cognition that places diversity at centre stage (Evans and Levinson 2010).

Following this robust introduction, the authors develop each of their themes in turn: first, the assertion that Cognitive Science has been led by Chomsky and his followers into a *false* belief regarding the underlying commonality of human language (Universal Grammar); second, the claim that there are no universals of language “in the direct sense that all languages exhibit them”; finally, the conclusion that linguistic diversity itself is the proper object of study, and—as a consequence—that the adoption of a relativist stance to theory construction will yield

¹⁰ For a shorter version of this section, see Duffield (2011).

greater insights and progress in understanding natural languages than the Universalist approach has so far achieved.

There is much to commend in Evans & Levinson's paper, not least the glimpse it affords into the fascinating and vital work of field linguists, who are involved in a race against time to document the world's endangered and moribund languages (which means most language varieties): for this direction alone, Evans & Levinson's paper makes urgent reading.¹¹ This is, however, the most positive aspect of their contribution: as implied by their introductory paragraph above, the greater part of the paper offers a much more negative agenda, in which the authors attack Chomskyan approaches and proclaim Language Universals to be a myth. In this, Evans & Levinson's assertions go to the very heart of the issues we are concerned with here, in that they not only set up a false dichotomy (between surface diversity and UG) but also make a false identification (between UG and Language Universals), while resting their case on rhetoric and a spurious use of empirical evidence.¹²

With respect to the alleged non-existence of language universals, Evans & Levinson consider three putatively universal properties of natural language grammars: (i), the notion of 'grammatical subject'; (ii), the universality of hierarchical constituency relations; (iii), the universality of recursion (recursivity). In each case, Evans & Levinson's form of argument runs as follows: generativists have proposed a certain property P as a language universal; there are languages that fail to instantiate P ('in any direct sense'); therefore, P is not a universal. The syllogism is sound, just as long as the premises hold. But they don't, for in each instance either the first premise is false, or the second fails, or both. Moreover, as I discuss further in the third section of the paper, the authors' arguments are further confounded by the confusion of Language Universals with Universal Grammar.

Consider first the notion of grammatical SUBJECT, which for centuries has played a crucial and familiar role in traditional descriptions of Indo-European languages. Evans & Levinson argue clearly but otiosely, that the notion of subject that is most relevant to describing surface constructions in English cannot be applied directly, if at all usefully, to the description of the grammars of many other languages. Specifically, they claim:

'Many languages around the world have grammatical relations that map straightforwardly onto the clusterings of properties familiar from English 'subject' and 'object'...[L]inguists have also known for some time that the notion 'subject' is far from universal, and other languages have come up with strikingly different solutions... Having a [unified] subject relation is an efficient way to organize a language's grammar because it bundles up different subtasks that most often need

to be done together. But languages also need ways to indicate when the properties do not coalesce...Given languages like Dyirbal, Acehnese or Tagalog, where the concepts of 'subject' and 'object' are dismembered in language-specific ways, it is clear that a child pre-equipped by UG to expect its language to have a 'subject' could be sorely led astray [emphases mine: NGD].

Most of this quotation is unexceptionable: it is certainly true that, 'Linguists have known for some time that the notion 'subject' is far from universal.' But this knowledge is shared by generativist linguists as well! Indeed, one of the hallmarks of Chomskyan generative grammar—which distinguishes it from other formal approaches such as, for example, Relational Grammar or Lexical-Functional Grammar—is that it eschews the notion of 'subject' as anything other than a shorthand for a bundle of features which may or may not coalesce in other languages (or, come to that, in non-canonical transitive constructions in English). The generativist position is stated most clearly and directly in the following quotation from (McCloskey 2001):

In the intellectual tradition represented by the frameworks of 'Government and Binding', 'Principles and Parameters' and the 'Minimalist Program', the notions ['subject' and 'object': NGD] play no (recognized) role at all. That tradition has always insisted that talk of 'subjects' and 'objects' is either illicit or casual, and that reference to such terms is to be cashed out in terms of more primitive notions (phrase-structural measures of prominence, featural properties of heads, the theory of A-movement and so on)... — James McCloskey (2001).

Chomskyan generativists may disagree with functionalists about precisely *which* grammatical primitives interact to yield epiphenomenal 'subject effects', but there is really no general disagreement about whether 'subject' has any theoretical status. It follows from this that no generative acquisitionist assumes (as Evans & Levinson appear to believe) that a child "equipped by UG" should expect his language to have a subject, for the simple reason that this is not, and has never been claimed to be, part of UG. Nor—*an equally important but separate point*—do generativists claim that 'subject' is even a Language Universal.

A similar confusion arises in respect to Evans & Levinson's discussion of hierarchical constituency relations:

In nearly all recent discussions of syntax for a general cognitive science audience, it is simply presumed that the syntax of natural languages can basically be expressed in terms of constituent structure, and thus the familiar tree diagrams for sentence structure (Pinker 1994, p. 97ff; Jackendoff 2002; 2003a; Hauser, Chomsky & Fitch 2002)... But in fact constituency is just one method, used by a subset of languages, to express constructions which in other languages may be coded as

¹¹ See especially, Jim McCloskey's excellent paper "[A global silencing](#)"

¹² Spurious, at least from a generativist point of view.

dependencies of other kinds (Matthews 1981, 2007).

As evidence of non-universality of constituency relations, Evans & Levinson cite examples from Classical Latin (from Matthews 2007) and Jiwarli, both of which illustrate different types of discontinuous dependencies. Of the Jiwarli case (example 15 below), Evans & Levinson write:

...all linked nominals (part of a noun phrase if there was such a thing) are marked with case and can be separated from each other; there is no evidence for a verb phrase, and there are no major constraints on ordering (see Austin & Bresnan 1996). (15) illustrates a discontinuous sequence of words which would correspond to a constituent in most European languages; ‘the woman’s dog’ is grouped as a single semantic unit by sharing the accusative case.

(15) Kuuju-lu kaparla-nha yanga-lkin wartirra-ku-nha
child-ERG dor-ACC chase-pres woman-DAT-ACC

‘The child chases the woman’s dog. (Austin 1995, p 372)

There are several points one might make about such examples. First, in respect of Classical Latin poetry, it is legitimate to wonder whether Vulgar Latin made the same use of discontinuous dependencies as its more formal cousin: even Modern English poetry violates word-order conventions for effect, and the Latin example would be that much more convincing if it came from a conversational register. Similar remarks apply to the Jiwarli case: one has to look no further than very ordinary external possession constructions in English to see parallel discontinuities in an Indo-European language.

(a) ‘The dog bit *both of the man’s legs* as hard as he could.’

(b) ‘The dog bit *the man* as hard as he could *on both legs*.’

In short, much hangs on the translation here: if one were to take (b) sentence as representative of English possessor constructions, one might claim that even without identificational case marking, English fails to respect constituency.

Having said that, it is undeniable that in some language varieties discontinuous dependencies are much more prevalent and unmarked than in others, such that it is useful, for example, to speak of a Configurationality Parameter (*cf.* Hale 1983). But this does not mean, as the work of Mark Baker clearly demonstrates, that non-configurational languages do not reveal effects of constituency when considered at a more abstract level (see especially (Baker 2001).

Now it should be noted that Evans & Levinson are aggressively dismissive of this type of response: as is clear from the following quotation, abstractness is viewed as a ploy, rather than a principled strategy, and

the notion of parameter setting is deemed to have been empirically disconfirmed:

The cognitive sciences have been partially immunized against the proper consideration of language diversity by two tenets of Chomskyan origin. The first is that the differences are somehow superficial, and that expert linguistic eyes can spot the underlying common constructional bedrock. This, at first a working hypothesis, became a dogma, and it is wrong, in the straightforward sense that the experts either cannot formulate it clearly, or do not agree that it is true. The second was an interesting intellectual program that proceeded on the hypothesis that linguistic variation is “parametric”; that is, that there are a restricted number of binary switches, which in different states project out the full set of possible combinations, explaining observed linguistic diversity (Chomsky 1981; see also Baker 2001). This hypothesis is now known to be false as well: its predictions about language acquisition, language change, and the implicational relations between linguistic variables simply fail (Newmeyer 2004; 2005). The conclusion is that the variation has to be taken at face value – there are fundamental differences in how languages work, with long historico-cultural roots that explain the many divergences.

A whole lecture could be given on just this one paragraph. To avoid further digression, I will simply point out first that though experts may disagree fundamentally about certain things—as is true in any branch of science—there can be no disagreement that *some* level of abstraction (and abstractness) is vital to any scientifically valuable thinking about grammars...beyond list grammars, and untagged corpora. Therefore, the dispute here is over how much abstractness is functionally and aesthetically acceptable. Personally, I *share* with Evans & Levinson the view that Minimalist theory is unduly abstract, in so far as it goes even deeper than the ‘constructional bedrock’ they disparage, but that is because—like them—my primary interest is with language (albeit *I-languages*), rather than with UG, the initial state of the language faculty. I’ll come back to this point presently.

The other comment to make about the paragraph is that I simply disagree with Evans & Levinson’s interpretation of Newmeyer’s conclusions about parameter-setting. In any case, one can empirically challenge certain postulates of a theory, such as for example the validity of the version of the Null Subject Parameter as formulated by Hyams (1981), but it is not clear, as a matter of principle that one can disprove a whole theory. Matters are even harder with ‘programs’! Therefore, with respect to constituency relations, Evans & Levinson both misrepresent generativists’ claims about constituency as a Language Universal, and fail to demonstrate, even if those claims had been correctly presented, that it is false.

Finally, with regard to Evans & Levinson’s discussion of the empirical evidence for Language

Universals, consider the property of recursion (recursivity). Evans & Levinson write:

We turn now to recursion, the feature which is at the heart of recent heated discussions: indeed, Hauser, Chomsky & Fitch 2002, p. 1569 hypothesize that recursion is “the only uniquely human component of the faculty of language”... In this context where recursion has been suggested to be the criterial feature of the human language capacity, it is important for cognitive scientists to know that many languages show distinct limits on recursion in this sense, or even lack it altogether...
...The most radical case would be of a language that simply disallows recursion altogether, and an example of this has recently been given for the Amazonian language Pirahã by Everett (2005), which lacks not only subordination but even indefinitely expandable possessives like ‘Ko’oi’s son’s daughter’. This has been widely discussed and we refer the reader to that paper for the details...

In this case, Evans & Levinson correctly represent the first premise: core Minimalists—though by no means all generativists—do believe that a particular kind of recursivity is a (perhaps *the*) universal property of human languages. It’s worth noting, though, that this is a *different* subset of generative linguists from those who espouse parametric theory—indeed, core Minimalists such as Cedric Boeckx explicitly deny the existence of parameters in core grammar:

One thing that a minimalist should resist at all costs is the claim that Rizzi’s Relativized Minimality principle (in any of its economy incarnations) [or any similar principle: NGD] is parameterized, meaning that some languages abide by it, while some other languages would be free of its effects. Such a possibility clearly falls out of the set of minimalist grammars. Put differently, if Relativized Minimality is a parameter, the minimalist program is not a program worth pursuing (Boeckx 2006: 104-5).

What this means is that in presenting their empirical evidence, Evans & Levinson are attacking two completely different groups of Chomskyan linguists as though they were one: those who believe in the existence of parameters in Universal Grammar, and those who believe that recursivity is the criterial feature of the language capacity. Of course, either or both of these construals of UG could be mistaken, but they do not constitute a monolithic generativist agenda, as Evans & Levinson appear to believe.

As for the alleged empirical evidence against recursivity as a universal property, it is simply not true—as *implied* by the ellipsis (but deftly not *asserted*)—that there is a consensus view about Pirahã. Rather, the jury is out on this one. If one refers to the literature, it should be clear that Dan Everett’s interpretation of the Pirahã data is only one of several; in particular, his analysis has been strongly

challenged—to my mind convincingly—by a subsequent paper, also published in *Language* in (2009), by Andrew Nevins, David Pesetsky and Celine Rodriguez (Nevins, Pesetsky, and Rodriguez 2009). But, whether one agrees with Everett or with Nevins *et al*, this issue is emphatically not as cut-and-dried as Evans & Levinson suggest.

This is all I have to say here about the *empirical* arguments presented by Evans & Levinson against Language Universals and UG. I have tried to show why I believe the evidence they present is insufficient for anyone to conclude that Language Universals are a myth, and to highlight some of the ways in which their empirical assault on the generativist position is misguided: either because no generativist (in recent years) has in fact advanced the claims they reject, or because different groups of generativists have advanced internally heterogeneous claims about Universal Grammar, such that Evans & Levinson are unwittingly supportive of one or other of these positions.

These remarks notwithstanding, the most important issues raised by Evans & Levinson’s paper are not, I think, *empirical*, but *conceptual*. For the main point is that even if Evans & Levinson’s empirical arguments were sound, and *Language Universals* were chimaeral, this would tell us very little, perhaps nothing, about Universal Grammar.

In this penultimate section, I want to examine four false assumptions concerning commonly perceived oppositions between the labels on either side of police line I talked about earlier, which is reproduced again here:



If it can be shown that these popular assumptions are really misconceptions, then it should become clear—to return to the Benchley quotation we started with—why I do not believe that there are just two kinds of linguists (generativists *vs.* functionalists, nativists *vs.* emergentists, universalists *vs.* relativists, *etc.*)—though there may very possibly be two (or more) kinds of linguistics, as is equally true of biology and psychology, as well as of other branches of the physical and social sciences.

The first myth, perpetuated by many commentators, is the idea that there is some necessary correlation between Universal Grammar and Language Universals,

such that UG entails the existence of universals. If this proposition were correct, then the *absence* of Language Universals ($\sim q$) as alleged by Evans & Levinson *would* pose a real logical problem for advocates of UG, strong nativists in particular. But it's false, and the reason for this is ontological: that in contrast to language universals, which describe shared properties of *end-state* grammars, UG is a ultimately a theory of the INITIAL STATE of the child's grammatical knowledge, of what the child knows *apriori* before she has been exposed to any particular language. This then is the second take-home message of the talk:

THM#2: UG does not entail the existence of Language Universals.

In order to elucidate this point, it is useful to consider step-by-step two pairs of theoretical terms: the first, the relatively familiar distinction between I-LANGUAGE and E-LANGUAGE; the second, a less familiar, but equally crucial contrast between theories of *L* (attained states) and a/the theory of *FL* (the Faculty of Language). Taken together, these terms offer a kind of 'double articulation', dissociating UG (a theory of FL) from surface grammatical facts.

Let us first consider E-language and I-Language. It is often assumed by many non-generativists that the fundamental difference between these two notions is the level of abstraction. This is incorrect: UG is not a 'deeper' theory of descriptive Language Universals; rather, it pertains to a fundamentally different object of study. This is clear from the following quotation from Norbert Hornstein:

The proper objects of inquiry are I-languages; 'I' standing for intensional, internal and individual. An I-language is individual in that each speaker has one. This focus turns the common wisdom on its head. E-languages like English, Swahili and so forth are (at best) radical idealizations for Chomsky, or (at worst) incoherent pseudo-objects. At best, E-languages are the intersection of the common properties of various I-languages. Thus, for example, it is not that speakers communicate because they have a language in common; rather, wherever I-languages overlap communication is possible.— Norbert Hornstein (<http://www.chomsky.info/bios/1998----.htm>)

Given this, it is misleading to suppose that E-language labels such as 'French', 'Jiwarli', 'Arrente' have any interesting theoretical status, except as loose cover terms: just as modern physics dispenses with Ptolomaic notions like 'sunset' and 'sunrise,' or even with lay notions such as the 'end of the rainbow', or 'over the horizon', so Minimalism accords little or no theoretical value to notions like 'the English Language', or even 'the variety of English spoken in

Sheffield'. This is not to say that such notions may not be useful or meaningful in the context of other inquiries (for example, in sociolinguistics, historical linguistics, or applied linguistics—under particular operational definitions), just that they play no role in formal grammatical theory.

Notice that once I-language is established as the object of inquiry, many of the descriptive claims that are labelled as Language Universals (in the traditional Greenbergian sense, or the "direct sense" that Evans & Levinson refer to) cannot be sensibly related to grammatical theory or to UG, for the obvious reason that individual speakers have no implicit access to them, nor do such generalizations inform any aspect of language use. For example, it may indeed be the case that all languages have vowels, or that if a language has voiced obstruents, it will have a voiceless series, or that all languages with canonical VSO order in main clauses also allow an alternative SVO order, but this knowledge is simply not accessible to most adult speakers of a given language—other than to typologists as metalinguistic propositions. As suggested by Fig. 1. Greenbergian generalizations of this sort are not part of anyone's I-language, and so have no bearing on a theory of I-language anymore than the generalization that all tigers have stripes—supposing this to be true—forms any part of an interesting biological or ethological theory of tigerhood.¹³

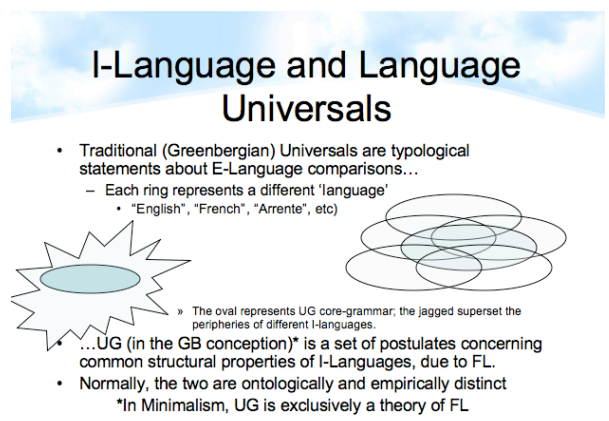


Figure 1. Why Language Universals are irrelevant to UG (I)

Even if one considers the more intensional universals discussed by Evans & Levinson, the universality of notion "subject", of constituency relations, and of recursivity, only the last of these is even a plausible candidate as an I-language property, constituency being either merely a notational device to designate properties of sentences generated by a particular grammar (*L*), or—at best—a derivative

¹³ This is not to say either that Greenbergian universals based on E-language comparisons cannot provide useful hints to the nature of UG, or that UG may not provide part of the explanation for these surface facts. What it does show, however, is that UG and Language Universals belong to wholly separate ontologies.

property of more fundamental syntactic computations, including the recursive operation *Merge*.¹⁴

Hence, the distinction between E-Language and I-Language means that most so-called Language Universals have no relevance to UG, even if UG is considered as a theory of the commonly shared intensional properties of attained I-Languages. This dissociation is schematized in Fig. 1 above.

In fact, a closer reading of the more recent literature reveals that Mainstream Minimalists are not even greatly interested in I-Languages, the internalized intensional representations of attained grammatical states, except derivatively, in so far as the content of I-Language is directly identifiable with that of the initial state (*FL*). This is relatively clear from the following recent quotation from Chomsky:

I understand *L* (for me, some variety of English) to be an attained state of a genetically-determined faculty of language *FL*... (Chomsky, cited in Stemmer 2009)

Crucially, it is this distinction between *L* and *FL*—more than that between E-Language and I-Language—that breaks the inferential link between UG (viewed as a theory of *FL*) and Language Universals, a set of observations about formal structural commonalities among different I-languages (or, possibly, E-languages).¹⁵

¹⁴ As discussed earlier, subject does not even count as a theoretical construct for most generativists.

Blueprint or Recipe?

Metaphors are significant here. The metaphor most often employed to characterize UG is that of the blue-print: see, for example, Crain & Thornton (1998):

Another hallmark of innateness is universality: If a linguistic principle is part of the human biological blueprint for language growth, then it is expected to be manifested by children learning any natural language—hence the term Universal Grammar.

Upon reflection, this is probably the wrong metaphor, for there is a strong isomorphism between a blue-print and the final shape of the constructed building: provided that the structural engineers, and the builders they instruct, do their job correctly, one can see from a blueprint how a building will turn out. Arguably, a more appropriate metaphor—and a more homely one—is given by Mark Baker in his (2001) book *The Atoms of Language*, essential reading for anyone interested in the Language Universals debate, where UG is presented not as a biological blueprint, but as a recipe (in this case, for bread). The recipe may determine the product, but there is no clear isomorphism, no point-for-point similarity, between a list of ingredients and instructions and the resulting product: one cannot point to a loaf of bread and say “There is the yeast”, “There is the baking at 200 degrees for 75 minutes”). Moreover, we may all follow essentially the same recipe, but external conditions, such as the hardness of the water, the particular strain of flour, the strength of the oven, the experience and temperament of the baker, the

It is worth noting that many generativists used to be more interested in these attained states: in the GB era, UG was presented as a theory of the universal principles and parameter-settings that characterized mature I-languages. Within Minimalism however, the goal of grammatical inquiry is not to determine the universal properties of attained states, but something more fundamental, to give a characterization of the initial state (*FL*) itself. It should also be noted that Chomsky has long entertained the possibility that apriori knowledge of language (UG) may not be sufficient in itself to determine the final shape of attained grammars. As far back as 1975, Chomsky wrote:

I have been assuming that UG suffices to determine particular grammars (where again, a grammar is a system of rules and principles that generates an infinite class of sentences with their formal and semantic properties). But this might not be the case. It is a coherent and perhaps correct proposal that the language faculty constructs a grammar only in conjunction with other faculties of mind. If so, the language faculty itself provides only an abstract framework, an idealization that does not suffice to determine a grammar ((Chomsky 1975): 41) [emphasis mine: NGD].

In this conjecture, then, we see the explicit dissociation of *FL*—the Language Faculty, or Language Acquisition Device (LAD), as it was then—from *L*.

Read carefully, Chomsky never claims that the attained states (I-languages) of individual speakers will all be identical, much less that the indirect products of these attained states (E-language phenomena) will be equivalent crosslinguistically. Given this, there is no reason to suppose from a generative point of view that there should be any directly observable Language Universals.

Nor—if such Language Universals are shown to exist—should one expect that these necessarily have their origins in, or be explained by, UG; the *source* of particular language universals is a separate empirical question. For this reason, I suspect that many generativist linguists would generally concur with

ambient atmosphere in the room, etc., can lead to a wide variety of outcomes, some of them delicious, others unpalatable. Most important perhaps is the quality and quantity of optional ingredients: though not necessarily part of the core recipe, it is usually these that most forcefully determine the taste and texture of the bread.

These optional ingredients may be understood either a “parameterized options” or as “additional elements”. So, the recipe might read “use either white or wholemeal flour/” or “add/do not add yeast” (the parametric alternatives), or “add walnuts, or other nuts, as desired” or “the particular type of flour will depend on availability. Both options give rise to diverse outcomes; depending on whether one prefers “under-specification” to “over-specification”, the UG theory of bread will contain these instructions or not. See (Baker 2007) for the suggestion that overspecification is to be preferred, even though it may appear less parsimonious.

Evans & Levinson's conclusions about some—if not all kinds—putative surface universal:

Although there are significant recurrent patterns in organization, these are better explained as stable engineering solutions satisfying multiple design constraints, reflecting both cultural-historical factors and the constraints of human cognition.

This is surely true of many surface regularities, but it may have little real bearing on the correctness of UG. What this means, in conclusion, is that one can and should investigate evidence for or against Language Universals independently of any theory of the initial state; conversely, one can and should develop a computational theory of the initial state (FL) independently of theories of I-Language commonalities or of observations about E-language universals. Both are equally legitimate, but separate, objects of inquiry. While there may be reasons to hope that the two are reconcilable, there is no particular reason to expect that one will be reducible to the other, any more than a theory of anatomical universals or of ethology should reduce to or replace biochemistry or molecular biology.

Before continuing, I want to make clear that my intention is not to assert the superiority or attractiveness of a UG-centred approach to grammatical theory over any other. Personally, I find a good deal of Minimalist research to be aesthetically unattractive and remote from my research interests and concerns; my preference is for syntactic theories that tackle variation head on, as well as for explanations of universal tendencies that are better related to the facts of language processing and language development. But this subjective assessment is really neither here nor there...

The second variation on misconceptions that divide us is the idea that there is some tension or possible contradiction between UG and the extent of surface Linguistic diversity. This idea is certainly implicit in the following crass remark from the Neo-Conservative critic of Chomsky's cited earlier:

...Now, it is obvious to anyone who's studied a foreign language that there is no such thing as 'universal grammar...'

Much more significant than the sarcastic remarks of an ill-informed blogger, the assumption also plays a central, if implicit, role in Evans & Levinson's argumentation (the *then* in the following quotation):

Linguistic diversity *then* [italics mine: NGD] becomes the crucial datum for cognitive science: we are the only species with a communication system that is fundamentally variable at all levels. Recognizing the true extent of structural diversity in human language opens up exciting new research directions for cognitive scientists, offering thousands of

different natural experiments given by different languages, with new opportunities for dialogue with biological paradigms concerned with change and diversity, and confronting us with the extraordinary plasticity of the highest human skills.

For Evans & Levinson diversity, rather than commonality, is assumed to be the distinctive and most defining feature of human languages.¹⁶ As other commentators have pointed out, this assumption is at variance with that in just about every other area of natural science, though it seems to accord well with some of the social sciences, especially anthropology.¹⁷

If we look at other areas of inquiry in the natural sciences, the anomaly of Evans & Levinson's position comes into sharper relief. Consider, for instance, our view and understanding of the natural world. What immediately fascinates and impresses the non-scientist is undoubtedly species diversity: the weird and wonderful contrasts among plant and animals, and their exquisite adaptation to the myriad ecosystems they inhabit. No-one can fail to be awed and entertained by well-made nature documentaries such as those of Sir David Attenborough, nor by the other daily offerings by equally talented film makers on *Discovery Channel*, or *Animal Planet*. Even as we are engineering and witnessing an unprecedented mass extinction of non-human life on this planet, it is clear that we have hardly begun to document or understand the near unfathomable natural diversity to which this planet is host, and which continues to astonish (increasingly so, as digital film technology improves and documentary makers become ever more adventurous).

¹⁶ I use the plural here deliberately, since for Evans & Levinson and other relativists the term 'human language' has no real currency; indeed, it is part of the generativist misinformation strategy.

¹⁷ Arguably, it is not accidental that linguistic relativism finds more support among mainstream anthropologists and social psychologists than among cognitive scientists. However, the preference for universalist explanations in the latter camp is not because of any undue reverence for Chomsky, but because it accords better with the principles of good science.

Analogy to other Natural Sciences

- Who's right?
 - David Attenborough?
 - Or Watson & Crick?

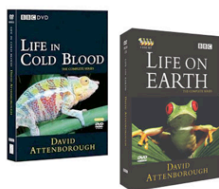


Figure 2. Contrasting approaches to Natural Science

However, this fascination and enchantment does not mean that viewing endless nature documentaries will bring us any closer to a scientific understanding of the fundamental biological facts of life. The dominant scientific view in biology, and elsewhere in the physical sciences, holds not merely that underlying *commonalities* are vastly more significant than surface differences, but also that the proper object of inquiry is the *genotype*, not the *phenotype*. A Nobel prize went to Watson & Crick, not to David Attenborough, and rightly so, for their discovery of the double helix helped to transform not only our scientific understanding of the biological world, but also our relationship to other living species, and our moral and philosophical view of our place in the universe.

That said, most people—including, I suppose, most biologists—would far rather watch the entire *Life on Earth* series than sit through a two-hour lecture on genetics. (We are, after all, a rather lazy and superficial bunch: genetics lectures—like syntax lectures—may be informative, but they're not generally eye-catching or sexy, and they demand concentration.) On the other hand, most people would feel that that a university degree course on biology that took diversity as its object of inquiry, or which restricted itself to taxonomic classification was missing the whole point, a squandering of time and money: in the past two centuries, we have moved beyond Linnaeus (something that most of us regard this as progress).

Returning now to UG, if the theory of UG is indeed a theory of the initial state, of *apriori* knowledge of language, then it is almost incoherent to expect to find new clues to the structure of UG by attending to surface variation. Just as decades of genetic research made progress by restricting attention to one species of fruit fly (*Drosophila Melanogaster*), so it may be argued that our understanding of all the world's languages has been advanced more by an in-depth study of English "reconstruction effects" than by a comprehensive crosslinguistic survey of "Passive" or "Topicalization" or of "Subjecthood", as interesting as these phenomena may be from a typological or taxonomic point of view.

In this regard, GB/P&P can be seen a scientific attempt to have one's cake and eat it, to probe the

deeper commonalities of the world's languages, while handwaving in the direction of a theory of surface syntactic variation (or at least of I-language variation). By contrast, Mainstream Minimalism largely gives up on this more ecumenical project, sticking in a resolute (*konzequent*) fashion to the deeper genetic project (the *Drosophila-only* approach to nature study). This naturally makes it, to outsiders, a less accessible and less attractive scientific program, but accessibility and attractiveness are irrelevant to the validity or application of the theory itself.

Page last updated at 00:49 GMT, Saturday, 5 December 2009

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Worm could offer Parkinson's clue

Scientists believe that worms could hold the key to why some people develop Parkinson's Disease.

Worms share 50% of their genes with humans, including those involved with inherited Parkinson's.

Dundee University researchers will study a simple worm called *C. elegans* to try to work out why the condition causes patient's brain cells to die.



Scientists will study the *C. elegans* worm for clues to Parkinson's Disease

The Parkinson's Disease Society has given the university £190,000 to carry out the research.

Eventual cure

There are about 120,000 people with Parkinson's in the UK. In up to 5% of those cases, the disease is believed to be directly inherited.

Parkinson's is a progressive neurological condition affecting movements such as walking, talking and writing. It occurs as a result of a loss of nerve cells in the brain.

Figure 3. Distant relatives? A "UG" view of worms

In summary, there is no real contradiction between dramatic surface diversity and a theory of an invariant UG, any more than there is a tension—except possibly for literal Creationists—between the extravagant variety of flora and fauna and the largely shared genetic code of all primates, mammals, vertebrates, even of all eukaryotic organisms (as the Figure above suggests). This does not mean that we should not be impressed by such variety, only that the fact of diversity—and with it the alleged Myth of Language Universals—does not speak to issues of a deeper universality.

Furthermore, to reiterate a point made earlier (THM#1), it is simply wrong to portray generativists as unaware, or willfully negligent, of the facts of language variation. As is clear from the quotation cited earlier, repeated here, Chomsky has repeatedly stressed the importance of such diversity for theory development:

...[The true job of linguistics] must be to develop an account of linguistic universals that, on the one hand, will not be falsified by the actual diversity of languages and, on the other, will be sufficiently rich and explicit to account for the rapidity and uniformity of language learning, and the remarkable complexity and range of the generative grammars that are the product of language learning."

The theoretical and empirical question on is *not* whether languages vary—the fact of diversity is inescapable—but rather what the locus of this variation is. Whereas relativists such as Evans & Levinson are inclined to view diversity as an intrinsic and thorough-going fact of grammatical comparison (‘language as a communication system that is fundamentally variable at all levels’) Mainstream Minimalists—to the extent that they are interested in describing attained states at all—assume that syntactic variation is an essentially peripheral feature of human language, arising at the interface between an invariant narrow syntax (NS), a parametrized set of lexical features LEX displaying constrained variability, and highly variable phonological component (ϕ). The following quote from (Chomsky 2001) makes this clear:

Assume further that L has three components: narrow syntax (NS) maps LA to a derivation D-NS; the phonological component ϕ maps D-NS to PHON; the semantic component Σ maps D-NS to SEM. Σ is assumed to be invariant for all L; NS is as well, if parameters can be restricted to LEX (as I will assume). ϕ by contrast is highly variable among Ls (Chomsky 2004, *Beyond Explanatory Adequacy*.)

On this construal, UG is first and foremost a theory of narrow syntax, which is by definition something much less than a fully-fledged I-Language, and the locus of grammatical variation is taken to be the lexicon (LEX), rather than narrow syntax itself.

Many of us regard this view of things as being empirically well motivated. Consider, for example, the comparison of three E-languages—English, Vietnamese, and Japanese—and the capacity of the speakers of these varieties to learn each other’s languages. The object that we intuitively understand as the grammar of English is dramatically and obviously different from Vietnamese almost entirely in virtue of its lexical stock and its associated phonology: if one were to re-lexify Vietnamese grammatical structures with English words, the result would generally be well-formed, if slightly awkward sentences. Contrast this with relexifying Japanese or Korean, where the results would be close to ‘word-salad’, as can be easily attested by anyone with experience of marking Japanese undergraduates’ English compositions. Ignoring phonology/pronunciation for a moment, these contrasts are well played out in English proficiency scores, in which Vietnamese speakers significantly outperform Japanese learners (at least when measured in terms of relative time and financial investment). Incidentally, this comparison offers a nice example of parameterization at work, a phenomenon to which Evans & Levinson—and, ironically, most Minimalists—would deny any theoretical status).¹⁸

¹⁸ In addition to general generative assumptions about the main locus of variation, however, there are also significant

To summarise the discussion thus far in this section, the main purpose has been to show that theories of Language Universals and of UG pertain to qualitatively distinct objects of inquiry: UG doesn’t merely offer a ‘deeper’ description of the same object; it characterises a fundamentally different set of phenomena. Given this fact, the tremendous range of surface variability of grammatical structures no more negates a theory of UG than should phenotypic plasticity cause us to reject a uniform theory of genetics. And so, this is the third take-home message of the lecture:

THM#3: Linguistic Diversity (per se) is no challenge to Universal Grammar

But now, even having clearly articulated the distinction between UG and Language Universals, let us suppose, purely for the sake of argument, that Evans & Levinson are right on both scores: that there are no Language Universals, and that—independently of this—UG is a myth. Would this mean that we should embrace linguistic relativism?¹⁹

The answer to this hypothetical question depends on a number of factors, including, notably, the correctness of one of the other terms we started with—namely, Modularity, or better, the Modularity Hypothesis.

In *The Modularity of Mind*, the type of relativism that Jerry Fodor so objects to is the relativism implied by the *interpenetrability—non-encapsulation—of grammatical knowledge* (Fodor 1983). Whorfianism—and broader Cultural Relativism—trades crucially on the idea that language (and—through language,—higher-order cultural beliefs) can interpenetrate perception, such that the particular language we speak influences the way we perceive the world and the world-view that we end up with; conversely, that our view of the world shapes the grammatical affordances of our language. In short, Whorfianism presupposes Strong Interactionism.

auxiliary assumptions on which these rest, and which are more contentious. These are, first of all, that narrow syntax is autonomous from lexical properties (a point of division between generativists and many other formal linguists and acquisitionists: see (Newmeyer 1999), for discussion; cf. especially (Bates and Goodman 1997)); second, that NS-L is directly identifiable with NS-FL, which is the true object of Minimalist inquiry (a point of division between core Minimalists, and many other generativists, including myself: see Duffield, in prep. Chapter 2).

¹⁹ In its theoretical, as opposed to its methodological version; see final section below.

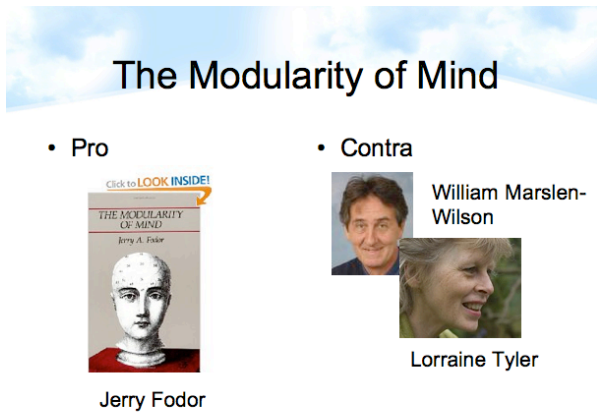


Figure 4. The crucial test for relativity: is language encapsulated?

A full discussion of the case *for* this type of relativism, which many people, including Lera Boroditsky, John Lucy and Steve Levinson would advocate, goes well beyond the scope of this paper (I discuss some of the arguments *against* in Duffield & Tajima 2010, Tajima & Duffield, submitted). Suffice it to say that it is a theory that I believe should be resisted except as a last resort, unless it is empirically irrefutable, and in this, I stand with Fodor.

Notice, though, that other scenarios are possible. Suppose that the human language faculty is encapsulated, as Fodor proposes, but that this encapsulated system is essentially void of *a priori* knowledge; as it might be, a blank formatted partition on a hard drive, to use an imperfect computer analogy. If this were the case, then each I-language grammar could in principle be infinitely different from every other, subject only to external functional and contextual constraints. This would be Modularity without UG or (intensional) universals. Under this scenario, relativism would be the only viable approach to characterising the nature of grammatical systems, but it would also be essentially harmless: the variability of I-language grammars would not pose a threat either to universal perception on one side, or to universal belief fixation ('Language of Thought') on the other.

Personally, I don't know anyone who subscribes to this view: it just doesn't seem to be plausible or explanatory of much. There is, however, a different but related scenario, which is I believe not only plausible given all of the evidence from language acquisition, typology, evolutionary linguistics, but also potentially explanatory. It is this: there is UG in the initial state, but there are no immediately observable or descriptively relevant Language Universals at the level of attained I-languages, since these arise through learning. Call this a version of the GIALIL hypothesis ('Grammar Is Acquired, Language is Learned'). This acronym nicely sums up my own conclusions about language acquisition. If this were the case, relativism would a good approach to characterizing surface language variation, but UG would remain the right theory of the initial state. As I hope to have explained

in this section, there is no contradiction between these two assumptions: the absence of language universals may sanction a relativist approach to attained grammatical systems, but it can co-exist happily with a universalist approach to the initial state, as shown in Fig. 5. below.

Notice though that for this project to succeed what is crucial is logical encapsulation. Fodor must be right about that, otherwise there can be no coherent initial state for FL to be a theory of.

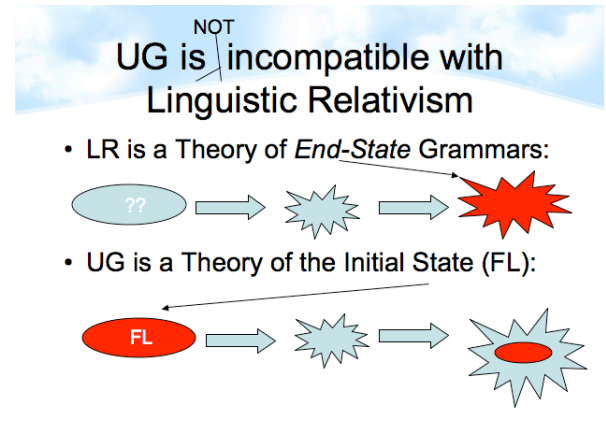


Figure 5. Minimalist UG vs. Linguistic Relativism

Before winding up this discussion of apparent conceptual oppositions, I'd like to deal with one final commonly-held assumption: the idea that a commitment to linguistic nativism entails a commitment to Minimalist UG. Although it is contingently true that most linguistic nativists are generativists, a moment's reflection tells us that this assumption is false. A wide range of alternative scenarios are possible, two of which seem to me to be very likely. First, FL might be highly specified, but with semantic, rather than structural content, or with parameterized construction-specific rules, rather than general Minimalist operations (Move, Merge).²⁰ Alternatively, FL might consist of highly specific *learning algorithms*—Slobin's "Operating principles", for example—rather than any specifically grammatical propositional content. Both of these nativist possibilities have been advanced by acquisition researchers over the years, some of them generativist, some not. The point is that nativism is both logically

²⁰ I would interpret much of Stephen Crain's more recent work on semantic acquisition as consistent with the former option, where the concern in this style of UG acquisition research is almost exclusively on children's innate knowledge of semantic conditions, such as downward entailment, and De Morgan's Laws, than with abstract structural relations. As for the latter option, one could imagine an Optimality-theoretic approach to construction-grammar, in which all UG-licit constructions are available at the outset, with different languages selecting different rankings. On this approach, Minimalist-style UG would be interpreted as an evolutionary principle, giving rise to constructions but not itself forming part of any synchronic theory.

and actually dissociable from Minimalist UG: one can subscribe to the idea there is an innate Faculty of Language without signing up to the idea that this innate knowledge is syntactic in nature, let alone Minimalist. This then is the final THM:²¹

THM #4: A commitment to Nativism does not entail a commitment to Minimalist UG

In summary, Linguistic Relativism is often presented as though it is diametrically opposed to, and a threat to UG. In fact the two are separated from each other by so many other theoretical variables that it is hard to see how they might intersect, let alone present a challenge to one another. Of course, this has not prevented the battle lines being drawn, and positions being entrenched: in this respect at least, the field of linguistics is just as irrational and emotionally unhinged as the politics of the mob (for example, the received view in England and the US when I was growing up that the Northern Irish Troubles were about religion).

If I am correct, what all this discussion implies is the field of linguistics is much less polarized than most people suppose: rather than there being “two kinds of linguistics”, there is a large range of coherent intermediate domains that one can inhabit (and whose hypothesis may turn out to be empirically correct). As well as I can determine, the extremes are marked by uniform acceptance or rejection of all of these postulates:

- There are no Language Universals of any kind.
- UG (either in terms of innate structural principles or innate learning principles) does not constrain language acquisition in any way (emergentism is right)
- End-state grammars are not encapsulated (strong interactionism is right, Fodorean Modularity is wrong.)
- Attained grammars are “learned”, rather than acquired— (input really matters; there is no strong convergence is the attained states of individual child learners)
- There is no universal mentalesse or “Language of Thought” (Fodor is wrong about this too!)

It is certainly the case that theoretical linguists, acquisitionists, psychologists and other cognitive scientists disagree with each other about the right answers to each of these questions. But they also disagree amongst themselves! There are undoubtedly some researchers who would accept every one of these claims, and some researchers who would reject them all: as mentioned at the outset, I have worked (generally amicably!) with members of both extreme factions. Yet, in my experience, the silent majority consists of people who are agnostic or equivocal about

one or more of these claims. In linguistics, as in politics, and other spheres of social existence, there are not really ‘two kinds of people’—even if the generally uncharitable rhetoric and mudslinging of the extremists would have us believe otherwise.

[This concludes the third section of the paper. In the final, hopefully brief (!), section to follow, I will summarize the points made thus far, and indicate why—in spite of everything that has been mentioned so far—I consider (methodological) relativism to be an invaluable and urgently needed approach to the study of natural languages; simply, why relativism is nevertheless a good thing...]

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²¹ There is no doubt a punchier way of presenting some of these THMs, but at present, I cannot think of one. Perhaps I'll be able to in future revisions of this paper.