

# A Potpourri of Chomskyan Science

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June 2013

## 1. Introduction

This paper is an extension of a review of *Noam Chomsky: The Science of Language – Interviews with James McGilvray*, providing additional textual evidence supporting my conclusion that *Noam Chomsky: The Science of Language – Interviews with James McGilvray* is not a book that illustrates *why* Noam Chomsky is considered to be a “founding genius of modern linguistics” (Stainton). I provide (i) elaborations of some of the points I made, (ii) additional passages from the text showing that the examples originally selected are representative of the quality of the volume, and (iii) a brief discussion of Chomsky’s intellectual contributions. My main focus is on linguistic and scientific issues. However, given that Chomsky provides also extensive ethical advice and advocates “consciousness raising” (p. 119), I will highlight instances where his own work violates standards he sets for others.

## 2. The Review<sup>1</sup>

### *Noam Chomsky: The Science of Language – Interviews with James McGilvray*

This volume is endorsed as “truly exceptional in affording an accessible and readable introduction to Chomsky’s broad based and cutting edge theorizing” (Stainton, back cover).

Chomsky made undeniably important contributions to modern linguistics but his Minimalist Program (Chomsky, 1995) and subsequent developments have been severely

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<sup>1</sup> The review has been accepted for publication and can be accessed at <http://journals.uvic.ca/index.php/pir> in early May.

criticized (e.g., Pullum, 1996; Johnson & Lappin, 1997; Culicover, 1999; Lappin et al., 2000; Postal, 2004; Seuren, 2004; Culicover & Jackendoff 2005; Jackendoff 2011). Hence a cutting edge account addressing these criticisms would be indeed desirable. The volume promises to cover a wide range of topics relevant to a 21<sup>st</sup> century science of language. Twenty-five interviews are grouped in two sections. Part I introduces the reader to Chomsky's thought on the design and function of human language, language evolution, representationalism, the nature of human concepts, optimality and perfection of Universal Grammar, and Chomsky's intellectual contributions. Part II includes discussions of human nature, evolutionary psychology, morality, epistemology and biological limits on human understanding. In addition McGilvray provides twelve appendices, chapter-by-chapter commentaries and a glossary.

In spite of the impressive table of contents, hope for finding cutting-edge insights and meaningful engagement with long standing criticism fades quickly. Most arguments for domain-specific innate biological endowment, saltational language evolution, semantic internalism, and computational optimality have been proposed for decades and are unsupported by evidence and/or citation of sources. Further, it will be difficult, especially for the lay reader, to follow the presentation because terms are not clearly defined, the conversation meanders through countless obscure, irrelevant digressions, and far-reaching conclusions are often drawn from meager premises.

For example, Chomsky argues that the function of human language cannot be communication because: "...probably 99.9% of its [=language, CB] use is internal to the mind. You can't go a minute without talking to yourself. It takes an incredible act of will not to talk to yourself" (Chomsky, p. 11). No evidence supports the claim that 99.9% of

language-use is internal. It seems to be based on Chomsky's introspection. Further, showing that language is currently used mainly for internal thought does not rule out its having originally evolved for communication. Selection acts only on aspects of traits that make a difference to the trait carrier's inclusive fitness, irrespective of what other aspects these traits may have and exaptations occur (Gould & Vrba, 1982).

The Argument from Norman Conquest, defending Chomsky's dismissal of the significance of empirical data for linguistic theorizing, is equally unconvincing:

Take the Norman Conquest. The Norman Conquest had a huge effect on what became English. But it clearly had nothing to do with the evolution of language - which was finished long before the Norman Conquest. So if you want to study distinctive properties of language - what really makes it different from the digestive system ... you're going to abstract away from the Norman Conquest. But that means abstracting away from the whole mass of data that interests the linguist who wants to work on a particular language. There is no contradiction in this; it's just a sane approach to trying to answer certain kinds of far-reaching questions about that nature of language. (Chomsky, p. 84)

The vague formulation of this argument makes evaluation difficult. If, when studying  $L_1$ , one should abstract away from *the whole mass of data* of interest to the linguist about  $L_1$ , the same logic would hold for  $L_2 \dots L_n$ . So one would have to abstract away from everything of linguistic interest about all languages to uncover the nature of language and explain how it differs from digestion. Idealization and abstraction are of course part of the scientific method but given how little is currently known about the core properties of language, such wholesale abstraction is hardly responsible. Even on a more charitable reading, the Argument from Norman Conquest is incompatible with Chomsky's view that "the linguist is *always* involved in the study of *both* universal and particular grammar ... his formulation of principles of universal grammar must be justified by the study of their consequences

when applied in particular grammars” (Chomsky, 1968, p. 24, emphasis added). It is remarkable that most arguments offered in support of Chomsky’s position are as vague as the Argument from Norman Conquest.

While *The Science of Language* cannot be recommended for the positive arguments it contains, even worse are numerous attacks on opponents, who are often not even named. None of the criticisms are supported by solid evidence. Instead, one finds vague assertions, misattributions and distortions:

... a very good English philosopher wrote a paper about it. [it = Everett’s work on Piraha, CB]. It’s embarrassingly bad. He argues that this shows that it undermines Universal Grammar because it shows that language isn’t based on recursion. Well if Everett were right, it would show that Piraha doesn’t use the resources that Universal Grammar makes available” (Chomsky, p. 30).

The very good English philosopher informed me that he had not written an academic paper but an 800-word book review for *The Independent* (Papineau, 2008). It is an informative review and contains nothing that is ‘embarrassingly bad’.

Another unnamed opponent is criticized as follows:

Some of the stuff coming out in the literature is just mind-boggling...The last issue [of *Mind and Language*] has an article - I never thought I would see this - you know this crazy theory of Michael Dummett’s that people don’t know their language? This guy is defending it. (Chomsky, p. 57)

‘This guy’ was very surprised that Chomsky “overlooked” that his paper (Lassiter, 2008) “was attacking Dummett’s position as untenable, using arguments inspired from Chomsky (1986)” (Lassiter, p.c.). Lassiter’s paper proposes a position different from Chomsky’s on the internalism/externalism debate but nowhere does he defend Dummett.

Distortion is also the hallmark of Chomsky's arguments against evolutionary accounts of language development:

There are a lot of [theories of language evolution] but *there's no justification for any of them*. So for example, a common theory is that somehow, some mutation made it possible to construct two-word sentences; and that gave a memory advantage because then you could eliminate this big number of lexical items from memory. So that had selectional advantages. And then something came along and we had three word sentences and then a series of mutations led to five...finally you get Merge, because it goes to infinity. (Chomsky, p. 15, emphasis added)

One example hardly supports the claim that there is no justification for any existing theory of language evolution. The 'common theory' is terrible but appears to be Chomsky's invention. None of the sixteen researchers I contacted had embraced such a theory, which one of them described as "truly nonsense" (Newmeyer, p.c.), and few could imagine anyone would. The consensus was: "This is a theoretical strawman if I ever saw one" (Christiansen, p.c.). Nevertheless, many similarly unsupported attacks on the language evolution community appear throughout: "We know almost nothing about the evolution of language, which is *why* people fill libraries with speculations about it" (p. 51) and "If you look at the literature on the evolution of language, it's *all* about how language could have evolved from gesture, or from throwing or something like chewing, or whatever. *None of which makes any sense*" (p. 49, emphasis added). Chomsky does not provide any evidence or detailed analysis supporting his dogmatic dismissals.

McGilvray's appendices aim to provide additional details in support of Chomsky's position. But his arguments suffer from the same lack of engagement with criticism and at times he outdoes Chomsky in distorting others' views:

Consider, for example, Patricia Churchland's (1986, 2002) view that one must look directly at the brain to construct a theory of mind. The internalist approach to linguistic meanings cannot currently look at neurons, axons, and neural firing rates. That is because unless one has a theory in hand of what neural systems 'do' - of the computations they carry out - looking directly at neurons is as sensible as groping in the dark... Moreover, there is no guarantee at all that the current understandings of neural systems and how they operate are on the right track. (McGilvray, p. 212)

The reply from the author, perplexed by this caricature of her view, was "To say of me what McGilvray says is like saying that Darwinian evolution implies that my grandfather is a monkey" (Churchland, p.c.). Churchland explicitly argues in the works cited and elsewhere that neuroscience needs psychology to provide a description of capacities and behaviors, that neurological and psychological theories need to co-evolve, and that no neuroscientist pursues a purely bottom-up strategy. Mysteriously McGilvray entirely missed these arguments.

Finally, there is a confident dismissal of work by connectionists, based on a letter by Chomsky to McGilvray (already quoted in McGilvray, 2009, p. 23):

... take Elman's paper[s]... on learning nested dependencies. Two problems: (1) the method works just as well on crossing dependencies, so doesn't bear on why language near universally has nested but not crossing dependencies. (2) His program works up to depth two, but fails totally on depth three. (Chomsky cited by McGilvray, p. 226)

This example is particularly troubling because an earlier review brought to McGilvray's attention that Chomsky's interpretation of Elman's work is incorrect (Behme, 2009) and, as his footnote 6 indicates, McGilvray is aware of the sources provided there. Yet, he repeats the fallacious argument and draws a similarly grandiose conclusion:

Details aside, the point is clear. Those convinced that language is a learned form of behaviour and that its rules can be thought of as learned social practices, conventions, induced habits...are out of touch with the facts... Enough then of externalist or “representationalist” and clearly non-naturalistic efforts to deal with language and its meaning (McGilvray, p. 226)

Enough indeed. There are many good publications on the market that deal with the topics discussed here. *The Science of Language* is not one of them and one can only hope in future publications both authors follow the advice Chomsky gives to others:

So sure study [language] to the extent you can, but sensibly - knowing when you're talking and producing serious science and when you're gesturing rhetorically to a general public who you are misleading. Those are important distinctions and I think if we make those distinctions, a lot of this literature pretty much disappears. (Chomsky, p. 105)

### **3. Supplemental Information**

One could expand on all the points made above. I restrict myself to two that are of particular importance: (i) Chomsky's criticism of and contribution to debates on language evolution, and (ii) Chomsky's distortion of the work of others. These topics were selected because Chomsky's views about language evolution reveal the full extent of the double standards evident throughout. He ridicules the work of an entire field, without ever citing the views he considers problematic. His own view is put forward authoritatively as the only rational option. This creates the impression that he is popularizing tidbits of a massive body of scientific work he has conducted. Yet, no supporting evidence is cited, and none of his

speculations are based on work he has completed himself. The tendency to distort and denigrate the work of others is not confined to language evolution and warrants additional attention as I document directly.

### **3.1. Speculations about Language Evolution**

For decades Chomsky has been claiming that communication is not an important function of language because language is badly suited for this purpose. This highly controversial proposal plays a crucial role supporting the equally controversial suggestion that language evolution occurred basically overnight when one mutation ‘slightly rewired the brain’ and ‘installed Merge’. Given its central importance, one would expect the proposal to be well defended. But it turns out to be a ‘just so story’ (JSS).

Now let’s take language. What is its characteristic use? Well, probably 99.9 percent of its use is internal to the mind. You can’t go a minute without talking to yourself. It takes an incredible act of will not to talk to yourself. We don’t often talk to ourselves in sentences. There’s obviously language going on in our heads, but in patches, in parallel, in fragmentary pieces, and so on. So if you look at language in the way biologists look at other organs of the body and their subsystems - so you take into account all its functions in talking to yourself - what do you get? What are you doing when you talk to yourself? Most of the time you’re torturing yourself [laughter]. So you might think you’re being conned, or asking why does this person treat me that way? Or whatever. So you could say that the function of language is to torture yourself. Now, obviously, that’s not serious. (Chomsky, pp. 11/2)



Chomsky provides no evidence supporting these claims and at least some readers may have a different experience concerning their use of language. Worse, there is no attempt to specify a function of language ‘in the way biologists do when they look at other organs of the body’. Instead, Chomsky states his only identifiable proposal was not serious. McGilvray fails to ask for a *serious* proposal and allows Chomsky to continue his musings: “It’s perfectly true that language is used for communication. But *everything you do is used for communication* - your hairstyle, your mannerisms, your walk, and so on and so forth. So sure, language is also used for communication” (Chomsky, p. 12). Again, no research is cited supporting this assertion. It might be based on personal experience. Presumably Chomsky is unaware of fellow humans who do not do *everything* for communication. His speculations continue:

In fact, a very tiny part of language is externalized - what comes out of your mouth, or from your hands if you’re using sign. But even that part is often not used for communication in any independently meaningful sense of the term “communication” ... the overwhelming mass of language is internal; what’s external is a tiny fraction of that [and what’s used in communication in some serious sense is a smaller fraction still]. As functions are usually informally defined, then, it doesn’t make much sense to say that the function of language is communication. (Chomsky, 2012, p. 12)

Overall, by stressing non-linguistic means of communication and diminishing the role of language in communication, Chomsky comes perilously close to claiming that virtually everything we do, *except language*, is used for communication.

Finally Chomsky asserts: “*Every* animal down to ants has a communication system” (p. 12, emphasis added). Undoubtedly, *many* animals do have communication systems. But Chomsky’s categorical claim requires that there are no animals without communication

systems. What and to whom would an endoparasite like *Taenia saginata* communicate, one wonders. Why would solitary sessile creatures like *Corella willmeriana* have a need to communicate? It is of course possible to conceive of a definition of “communication” that entails that these species communicate. But, if we invoke Chomsky’s own standards: “if by “communication” you mean any form of interaction, ok, [there is] communication. However, if you want the notion of communication to *mean* something, let’s say conveying information” (p. 12), then it is dubious that these or many other animals communicate with conspecifics. This example is a representative illustration of Chomsky’s tendency, to try to have things both ways, or *all* ways.

Turning to language evolution, two tendencies emerge. First, Chomsky expresses contempt for and repeatedly ridicules the work of others. Second, his own accounts reveal an astounding lack of elementary-level understanding of biology, psychology, and evolutionary theory. Moreover, they violate basic principles of scientific argumentation. Documentation of both these failures follows.

As discussed in the review reproduced above, Chomsky does not critique actual work on language evolution. Instead, he has invented an allegedly common account that bears no resemblance to any known theory. Researchers I contacted to inquire about it were insulted that anyone would imply they might defend such a theory. The comments ranged from “a caricature” (Corballis, MacWhinney), “willful ignorance” (Lieberman), “very annoying” (Jackendoff), “vaguely incoherent” (Studdert-Kennedy), “ridiculous” (Bickerton) to terms not suitable for citation. Given the variety of existing theories and the willingness of researchers to clarify their views and engage with targeted criticism, detailed discussion

and competent critique of existing theories would be a valuable contribution. Instead of doing this Chomsky is satisfied with creating and dismissing a strawman argument.

Next, Chomsky alleges that current evolutionary theorizing is too narrowly focused on natural selection: "... a pure form of selectionism that no serious biologist could pay attention to, but which is [a form of] popular biology - ...It's like a sixth grade version of the theory of evolution. It can't possibly be right" (p. 67). Chomsky links this view to Skinner and Quine but gives no example of evolutionary biologists holding it. He goes on to argue that natural selection could not have played a role in language evolution:

But what's advantageous about having a concept RIVER that has the features we seem to be sensitive to that could have no discernible bearing on survival or selection? We can make up thought experiments about RIVER which you couldn't even imagine if you're a New Guinea native. Imagine a small phase change that turns the Charles River into a hard substance, which is apparently possible. And then you paint a line on it, and you start driving trucks on both sides of the line, so it becomes a highway and not a river. You can't explain that to a New Guinea native; none of the other notions you need to entertain the thought of a river undergoing a phase change and becoming a highway are around; so how could selection have played a role in leading us to acquire the features RIVER has that come into play when we engage in thought experiments like these, ones that lead us to declare that a river has become a highway? In fact, the native has the same concept; if he or she grows up here or there, he or she's going to have the concept RIVER. So he or she's got it. But how could it possibly be selected? What function does it have in human life, for that matter? And... that's true of every elementary concept... (Chomsky, 2012, p. 47)

Chomsky provides no evidence establishing that every New Guinean native has exactly the same concept RIVER as he does and it is unclear what this argument establishes. It is possible that the concepts we currently have may not have provided a selectional advantage for our distant ancestors. Whether or not they had the same concepts as we is a matter of

speculation. Chomsky provides no evidence that they did and it is difficult to imagine what such evidence could possibly consist of. Assuming for arguments sake that they did have the same concepts, it would appear that the argument mainly threatens Chomsky's controversial innatism/internalism. If having the concepts we do has no selectional advantage and if the concepts are also not reliably related to external objects, then one has to ask *why* are these concepts and not different ones allegedly encoded in our genome? Why are they invariant for all humans and do not differ like, say, eye-colour or body-height (which are also both: genetically determined and not linked to survival)? These questions seem not to arise for Chomsky and he assumes his argument has established natural selection is mostly irrelevant for language evolution. This leads him to ridicule the proposals of others:

...the overwhelming assumption is that language evolved slowly through natural selection. Yet that doesn't seem at all consistent with even the most basic facts. If you look at the literature on the evolution of language, it's all about how language could have evolved from gesture, or from throwing, or something like chewing, or whatever. None of which makes any sense. (Chomsky, 2012, p. 49)

Chomsky does not reveal who overwhelmingly assumes prolonged gradual linguistic evolution, or what 'the most basic facts' are, and he provides no reference to specific accounts that don't make any sense. The continuation of his attacks - "We know almost nothing about the evolution of language, which is why people fill libraries with speculation about it" (p. 51) - is also not supported by any evidence such as citation of problematic speculations. The same holds true for: "You can't just tell stories about something; you have to show that those stories have some substance. That's why so much talk about

evolution is basically uninteresting; it's just stories" (p. 128). Since Chomsky does not even provide a name of anyone who propagates 'just stories', it is impossible to evaluate whether his criticism is justified. The attacks continue:

Take the evolution of language. It's a question; and so is the evolution of bee communication a question. But just compare sometime the literature on one with the literature on the other. There are libraries of material on the evolution of human language and some scattered technical papers on the evolution of bee communication, which mostly point out that it's too hard to study, although it's vastly easier to study than evolution of human language. *This is just irrational* ... So sure, study it [language evolution, CB] to the extent you can, but sensibly - knowing when you're talking and producing serious science and when you're gesturing rhetorically to a general public who you're misleading. Those are important distinctions, and I think if we make those distinctions, a lot of this literature pretty much disappears. (Chomsky, 2012, p. 105, emphasis added)

Chomsky's premise that the study of language evolution is vastly more difficult than the study of bee-communication does not support the conclusion that it is irrational to study the former. If the resulting insights are more valuable, it is sensible to spend more resources on that task. By analogy, not knowing how to cure cancer in less complex organisms does not make it irrational to search for a cure of cancer in humans.

Obviously, serious scientific work should be free of rhetorical gesturing. But Chomsky does not provide a single example of such gesturing and never identifies the researchers who are producing the mass of allegedly worthless literature. Only in one case, Chomsky does provide a name:

Many of these people, like Dawkins, regard themselves very plausibly as fighting a battle for scientific rationality against creationists and fanatics and so on. And yes, that's an important social activity to be engaged in, but not by misleading people about

the nature of evolution – that’s not a contribution to scientific rationality. (Chomsky, 2012, p.105)

One looks in vain for citation of any non-rational argument that Dawkins has offered, much less any counterevidence to Dawkins (unidentified) non-rational arguments, or any characterization of what it is that makes these (unidentified) arguments irrational. The reader is apparently supposed to walk away from this passage convinced that one of the major evolutionary theorists of the past century has provided not merely incorrect arguments, but arguments which are ‘not a contribution to scientific rationality’ - on the basis of nothing more than Chomsky’s declaration that this is so.

Given his harsh criticism of the work of others, one would expect that Chomsky’s own contribution is ‘done seriously and without pretense’, and that his arguments are carefully crafted and supported by solid evidence. But this is not the case. He cites no own research and his familiarity with the work of others seems superficial at best. He argues that, obviously, we have to dismiss gradual language evolution because other animals have adaptations similar to those of humans and it is not clear that any of the language related changes in our anatomy evolved *for* language. A typical “argument” is provided here:

There might be some adaptations for language, but not very much. Take, say, the bones of the middle ear. They happen to be beautifully designed for interpreting language, but *apparently* they got to the ear from the reptilian jaw by some mechanical process of skull expansion *that happened, say, 60 million years ago*. So that is something that just happened. The articulatory-motor apparatus is somewhat different from other primates, but most of the properties of the articulatory system are found elsewhere, and if monkeys or apes had the human capacity for language, they could have used whatever sensory-motor systems they have for externalization, much as native human signers do. Furthermore, it seems to have been available for hominids in our line for hundreds of thousands of years before it was used for language. *So it doesn’t seem as if there*

*were any particular innovations there.* (Chomsky, 2012, pp. 25-6, emphasis added)

First, Chomsky has not completed the research he discusses here. By omitting references he shows disrespect for the researchers and prevents the reader from accessing this work. Second, scientists do not claim that apparently something happened, say 60 million years ago, but, rather, give specific time frames for specific events. Third, this superficial survey of very few factors that need to be considered for the evolution of the multitude of capacities involved in language production and comprehension fails to support the conclusion that there were not *any* ‘particular innovations’ for language. Rather, for Chomsky it is a forgone conclusion that only Merge is in need of an evolutionary explanation and everything else just happened to be in place, presumably, ‘for hundreds of thousands of years’.

Another revealing aspect of Chomsky’s dealings with the language evolution literature is that he accepts without hesitation those proposals that support his own speculations. For example, it is crucial for his hypothesis that *all* modern day humans descend from one small breeding group in Africa that only dispersed after the alleged Merge-mutation occurred. He takes as established fact that “[w]e *know* by now that human language does not postdate about sixty thousand years ago...when the trek from Africa started” (p. 13, emphasis added). Chomsky neglects to mention that one hypothesis (e.g., Frayer et al., 1993) proposes a multiregional origin of modern humans (MOH). Most mainstream language evolutionists consider MOH unlikely (for discussion see Lieberman, 1998). But, as the history of plate tectonic theory (Wegener, 1929) shows, at times theories initially opposed by the mainstream turn out to be correct. Given that it is *necessary* for Chomsky’s

arguments that MOH is false one would expect he would either provide novel arguments, refuting MOH or, at least, cite specific research providing very strong supporting evidence for the mainstream ‘out of Africa’ hypothesis. He does neither. Instead, he accepts uncritically a hypothesis proposed by people who, according to him, are “gesturing rhetorically to a general public who [they’re] misleading” (p. 105), take “a highly irrational approach to inquiry” (p. 20), and provide “no justification for any of [their theories]” (p. 15). Seemingly, what makes the ‘out of Africa’ hypothesis acceptable is neither the reputation of its proponents nor compelling evidence but the fact that it offers support for Chomsky’s speculations.

Further, given how important the precise dating of the ‘trek from Africa’ and the ‘sudden leap’ are, it is surprising that Chomsky offers a fairly wide variety of dates for these events: “maybe sixty thousand years ago, language was there, in its modern form” (p. 13), “effects of having a complex symbolic system are barely there before 60,000-100,000 years ago” (p. 13), “this massive cultural revolution, which is quite striking, probably about sixty or seventy thousand years ago” (p. 17), “groups that got separated about fifty thousand years ago” (p. 27), “a ‘great leap forward’ in human evolution in a period of roughly 50,000-100,000 years ago” (p. 70), “it couldn’t have happened later than about fifty thousand years ago” (p. 71). Chomsky cites no work by other researchers and could have easily been more consistent. The inconsistency regarding such an important detail shows that neither he nor his editor cared to avoid the impression of sloppiness. Finally, Chomsky also claims: “You can argue fifty thousand years more or less, but that doesn’t matter” (p. 51). Given the dates Chomsky offers this implies the language mutation could have happened as early as 150,000 years ago or as late as yesterday. One might think more precise timing *does* matter.



Unsurprisingly, Chomsky's speculations about language evolution are on a superficial and unsophisticated level and do not meet any criteria of scientific theorizing. I reproduce here only three of his numerous attempts to come up with an account:

... some small genetic change led to the rewiring of the brain that made this human capacity available... Well, mutations take place in a person, not in a group. We know, incidentally, that this was a very small breeding group - some little group of hominids in some corner of Africa, apparently. Somewhere in that group, some small mutation took place, leading to the great leap forward. It had to have happened in a single person. Something happened in a person that that person transmitted to its offspring. And apparently in a very short time, it [that modification] dominated the group; so it must have had some selectional advantage. But it could have been a very short time in a small [breeding] group. Well, what was it? The simplest assumption - we have no reason to doubt it - is that what happened is that we got Merge. You got an operation that enables you to take mental objects [or concepts of some sort], already constructed, and make bigger mental objects out of them. That's Merge. As soon as you have that, you have an infinite variety of hierarchically structured expressions [and thoughts] available to you. (Chomsky, 2012, pp. 13-14)

This account has the hallmarks of (very superficial) backward engineering. Chomsky is convinced that Merge is the essential computational operation of language. Therefore Merge must have evolved, and this must have happened in one mutation, and this mutation immediately conveyed such a tremendous advantage to a single person that his/her descendants took over the breeding group and the world. Chomsky presents no evidence for his JSS. Furthermore, he neglects to mention that the 'great leap forward' hypothesis has been challenged (e.g., McBrearty & Brooks, 2000). It is a matter of ongoing scientific debate, whether the great leap forward occurred in all human groups. Independently, it is not entirely clear that a detectable change in technology is a reliable indicator for an increase in overall intelligence and/or the arrival of linguistic abilities. By analogy,

comparing the “archeological record” of human technology of the 17<sup>th</sup> and 20<sup>th</sup> century a scientist of the 44<sup>th</sup> century might conclude that our species underwent a dramatic increase in intelligence during this time period. But we have little reason to believe that such an increase took place. Hence, Chomsky needs to establish not only that ‘the great leap’ took place but *also* that it would provide proof that language evolved at exactly the same time. He does neither. For readers unconvinced by the previous JSS, Chomsky offers a slightly modified version:

... every living human being has basically the same [concepts]. So they must have been there before the separation - before the trek from Africa - which means roughly fifty thousand years. So they predate fifty thousand years. And there’s no real evidence that Merge really existed before roughly that time... There’s lots of interesting work showing adaptations of the sensory-motor system that appear to be language-related. So for example, the ear and articulatory muscles seem to be geared to the range of sounds that are used in language. But that doesn’t tell you anything. All that that tells you is that whatever grunts hominids were using may have played a role over hundreds of thousands of years in changing the structure of the middle ear. That wouldn’t be too surprising. It’s like any other animal - take frogs. Take a particular species of frogs; their auditory systems will be correlated with their articulatory system. But that’s precursors of language. Yes, *that’s going to be true for every organism*. So everything that’s found about the sensory-motor system - at most, what it’s telling you is, well, these are precursors to language of the kind that you find in frogs. But there has to be that point at which you suddenly get that explosive growth - this great leap in creative activity going on. It looks as though it’s roughly at the point of the separation of the breeding group all over the world. (Chomsky, 2012, p. 77-8).

One is told that, allegedly, our very distant ancestors had concepts that are virtually identical to our own because ‘every living human being has basically the same ones’. Concepts that remain the same over millennia, regardless of being used only in internal

thought or in communication with other members of the species or not at all are more reminiscent of immutable Platonic forms or the craftsman-stamp of the Cartesian God than of objects of 21<sup>st</sup> century naturalistic science. The superficial discussion of precursors to language in frogs indeed ‘doesn’t tell us anything’ and, given that *The Science of Language* is populated with remarks about pigeons, insects, nematodes and bacteria, one wonders if these groups are included in ‘any other animal’. For anyone with biological training, it is impossible to take this “account” seriously, and even non-biologists ought to wonder about its plausibility.

One final passage demonstrates how little empirical foundation there is to Chomsky’s evolutionary theorizing:

Take phonology. It’s generally assumed - plausibly, *but not with any direct evidence* - that the mapping from the narrow syntax to the semantic interface is uniform. There are lots of theories about it; but *everyone’s theory* is that this is the way it works for every language - which is not unreasonable, since you have only very limited evidence for it. The narrow syntax looks uniform up to parameters. On the other hand, the mapping to the sound side varies all over the place. It is very complex; it doesn’t seem to have any of the nice computational properties of the rest of the system. And the question is why. Well, again, there is a conceivable snowflake-style answer, namely, that *whatever the phonology is, it’s the optimal solution to a problem that came along somewhere in the evolution of language* - how to externalize this internal system, and to externalize it through the sensory-motor apparatus. You had this internal system of thought that may have been there for thousands of years and somewhere along the line you externalize it; well, *maybe the best way to do it is a mess*. That would be the nicest answer, although it’s a strange thought for me. (Chomsky, 2012, p. 40).

Here Chomsky claims that even though there are many (unidentified) theories, *everyone* believes that the mapping from the narrow syntax to the semantic interface is uniform for

every language. Not a single citation supports this sweeping claim. Further, how something that ‘is a mess’ could be the ‘optimal solution’ to anything would have required detailed justification. *The Science of Language* contains roughly 20 pages of evolutionary “theorizing” by Chomsky without stating a single proposal that could be tested scientifically. McGilvray’s attempts to clarify notions such as ‘biological function’ (pp. 169-175), ‘natural selection’, and ‘third factor’ reveal that he is also confused about the intricacy of biological processes. In sum, while Chomsky requests that theorizing about language evolution “has to be done seriously and without pretense” (p. 105) one sees that such requirements are not met in his work.

Last not least, readers not familiar with language evolution research may conclude that all work in this field is on the same level as Chomsky’s or even inferior. This is an entirely wrong impression. Given the complexity of the subject matter and the lack of direct evidence, there is certainly some speculation involved, as in all evolutionary study of behavior. But any serious proposal is supported by detailed hypotheses that are based on careful analysis of available evidence (e.g., Deacon, 1997; Lieberman, 2006; Tomasello, 2008; Botha & Knight, 2009; Bickerton, 2009; Hurford, 2011; Arbib, 2012). The line between well-supported assumptions and currently unconfirmable conjectures is clearly drawn, and in debates between opposing views the focus is on facts and evidence (e.g., contributions to Christiansen & Kirby 2003; Tallerman, 2005; Sampson, Gil & Trudgill, 2009; Tallerman & Gibson, 2012). It goes without saying that careful scientific analysis and serious engagement with alternative views requires more writing than dismissals based on allegations of irrationality. Hence, it is a lot less surprising than Chomsky implies, that language evolution research has generated a considerable amount of literature. Concluding

from the volume of this literature alone that the field has “a highly irrational approach to inquiry” is as unwarranted as it would be to claim that generative linguists of the 1960s were highly irrational because they generated unprecedented volumes of publications.

### **3.2. Distorting the work of others**

One of the most troubling aspects of *The Science of Language* is that Chomsky and McGilvray repeatedly distort the work of others even though it has to be assumed that they are aware that the accounts they give are incorrect. I defend this serious allegation by expanding on two cases briefly discussed in the review.

#### ***3.2.1. Dan Lassiter’s paper on semantic externalism/internalism***

Dan Lassiter published in 2008 (when he was a doctoral student at NYU) a paper in *Mind and Language*. He attempted to reconcile “descriptivism, mentalism, and externalism by construing community languages as a function of social identification” (Lassiter, 2008, p. 607). If Chomsky thought that this project was unsuccessful, he should have provided factual criticism. Instead, he accuses Lassiter (whom he only calls ‘this guy’) of defending a crazy theory of Michael Dummett. As discussed in the review, on the contrary, Lassiter does not defend but attacks Dummett. Anyone who had read the paper would have hardly missed that.

Dummett argues that communalects must be able to [provide a guarantee of mutual understanding] because otherwise, ‘for all [a

speaker] knows, or can ever know, everyone else may attach to his words or to the symbols which he employs a meaning quite different from that which he attaches to them' (ibid.). This consequence is intended as a reductio, but attention to the empirical facts of language shows it to be a positive boon: only a theory that does not provide such a guarantee can provide a convincing account of language variation and change (Lassiter, 2008, pp. 631-2)

Chomsky could have missed this explicit point only if he did not read the entire paper.

Criticizing work he has not read would be irresponsible. However, the situation is worse.

McGilvray replies to Chomsky's enraged comment about some guy defending Dummett's crazy theory:

Terje Lohndal [a graduate student in linguistics at the University of Maryland] - he and Hiroki Narita [a linguistics graduate student at Harvard] - wrote a response to it. I think it's good; I don't know if it will be published. I hope so. [See Lohndal & Hiroki (sic) 2009.] (McGilvray, 2012, p. 57)

Given that Lohndal & Narita (2009) is found in the bibliography (from which Lassiter (2008) is missing), it is odd that McGilvray claims he does not know if it will be published. Further, these authors acknowledge that they "are indebted to Noam Chomsky, Jim McGilvray, and Paul Pietroski for valuable comments and advise (sic) on this piece" (Lohndal & Narita, 2009, p. 231). Given that this paper deals virtually exclusively with Lassiter's arguments, Chomsky and McGilvray could not have provided 'valuable comments', had they not been familiar with the relevant details of Lassiter (2008) long before *The Science of Language* went in press.

Lohndal and Narita allege "that Lassiter's arguments are flawed and based on a serious misunderstanding of the internalist approach to the study of natural language ... and conclude that Lassiter's socio-linguistic approach is just another instance of externalist

attempts with little hope of scientific achievement” (Lohndal & Narita, 2009, p. 321). At one point the authors acknowledge that Lassiter holds that “the philosophically dominant tradition of semantic externalism (led by people like Hilary Putnam, Tyler Burge, Michael Dummett, and David Lewis) can [not provide] ... a linguistic theory that incorporates individuals’ intentional contributions to the meaning/reference of linguistic expressions” (Ibid., p. 322). However, they also frequently conflate Lassiter’s view with externalism (e.g., “his alleged ‘theory’ is just another instantiation of externalism”, p. 323; “He fails to provide convincing arguments for the feasibility or legitimacy of constructing an externalist linguistic theory of the sort he envisages”, p. 329).

In 2010 Lassiter published a reply to Lohndal & Narita (2009) defending his account and specifically stating: “I expended considerable energy to refute precisely this type of externalism, using Dummett as the prototype of an externalist whose theory is unworkable (Lassiter 2008: 611-617)” (Lassiter, 2010, p. 138). The further details of the dispute are irrelevant here. Striking is that at the time of publication of *The Science of Language* Lassiter’s original paper and his reply to Lohndal & Narita (2009) had been available to Chomsky. In both, Lassiter states clearly and unambiguously that he objects to Dummett-style externalism. One cannot plausibly assume that Chomsky was *unable to understand* Lassiter’s arguments. He harshly attacked an author whose paper he knowingly distorted. This would be a reprehensible act no matter who commits it. But given the status and exalted influence Chomsky enjoys, it is outrageous that he would resort to such unprofessional behaviour to demean someone who disagrees with him.

Also relevant here are the grounds on which Chomsky defends semantic internalism:

Take children stories; they’re based on these [internalist, CB] principles. I read my grandchildren stories. If they like a story, they

want it read ten thousand times. One story that they like is about a donkey that somebody has turned into a rock. The rest of the story is about the little donkey trying to tell its parents that it's a baby donkey, although it's obviously a rock. Something or another happens at the end, and- it's a baby donkey again. But every kid, no matter how young, knows that that rock is a donkey, that it's not a rock. It's a donkey because it's got psychic continuity, and so on. That can't be just developed from language, or from experience. (Chomsky, 2012, p. 27)

This argument is supposed to show that children could not have learned the concept 'psychic continuity' from experience or from instruction. Developmental psychologists study the conditions under which children impute intentionality to objects. But Chomsky does not cite any such work. Instead, he derives his data from a fairytale. It is of course not surprising that his grandchildren could not have learned from experience with the actual world that donkeys turning into rocks and back into donkeys retain their psychic continuity. But this hardly establishes that they must have an innate concept of the nature suggested. Further, Chomsky claims based on the same "case study" that for "other cultures ... the basic properties [of concepts] are just *identical*" (p. 27, original emphasis). Throwing in the additional example of 'river', Chomsky claims that all infants in all cultures recognize continuity of objects that change their appearance: "... these things are there. They show up in *every* language; whether they are there independently of language, we have no way of knowing. We don't have any way of studying them" (Ibid., emphasis added). Without providing any evidence he claims that every human being shares the 'continuity of identity' concept and that we cannot study these matters. Chomsky provides exclusively arguments of the donkey-tale quality to support his own view; yet, he calls the views of others 'crazy'. Chomsky advocates superior ethical standards, writing that we need "consciousness raising: get people to recognize that there's nothing natural about domestic abuse, for example" (pp.



119-20). There is also nothing natural about distorting the view of one's opponent to achieve an advantage in academic debates, about providing poorly supported arguments, and about creating the impression of having conducted a massive amount of research without providing any citation of specific results from this research. The failure of Chomsky's writings to conform to serious standards of scientific and academic practice contrasts strikingly with his preaching about ethical standards and consciousness raising.

### ***3.2.2. Jeff Elman's early connectionist work***

The misconstrual of the work of others can be based on ignorance, genuine misunderstanding, or willful distortion. Consulting the relevant literature can eliminate the first and likely the second of these reasons. In the case discussed below neither Chomsky nor McGilvray have the questionable "excuse" of being unaware of this literature.

In July 2009 it has been brought to their attention that the letter from which McGilvray cited in *Cartesian Linguistics* (2009) contained serious misinterpretations of Elman's work (Behme, 2009). The authors were provided with several papers by Elman showing clearly that his work had been misrepresented. In *The Science of Language* McGilvray cites *these* papers in a footnote, which must be taken as indication that he has read them. Nevertheless, he claims in that footnote "Chomsky was wrong to think that the view is expressed in a single paper" (p. 226) and continues to support the incorrect conclusions Chomsky draws about Elman's work.

Anyone who has read the papers knows that "the view" is not expressed in them. Elman has not claimed that his method works "just as well on [nesting and] crossing dependencies"

(Chomsky, p. 226). Instead, the papers cited explicitly report differences in performance: “... given the prediction task, the network is more successful at right-branching structures than center-embedded ones<sup>2</sup>” (Weckerly & Elman, 1992, p. 414, see also below). One important finding of this work was that it showed that “the network’s performance parallels that of human listeners” (Ibid., p. 418). It is not relevant here whether this conclusion is correct but that Elman explicitly stated the difference in performance.

Further, Chomsky claims, “[Elman’s] program works up to depth two but fails totally on depth three” (Chomsky, p. 226). However, Elman reported:

In the current simulation, the *representation degraded after about three levels of embedding*. The consequences of this degradation on performance (in the prediction task) are different for different types of sentences. Sentences involving center embedding (e.g., 9c and 9d), in which the level of embedding is crucial for maintaining correct agreement, are more adversely affected than sentences involving so called tail-recursion (e.g., 10d). (Elman, 1991, p. 215)

Elman does not say that his program fails totally at level three but that representations degrade. Further, he specifically states that at that level are differences for sentences

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<sup>2</sup> It should be noted that Chomsky and Elman use terminology differently. Elman draws a distinction between (i) left/right branching and (ii) center embedding. Chomsky calls both nested dependencies: “The examples I’ve seen of what they call “center embedding” are nested dependencies, usually self-embedded. These are the terms from the literature in mathematical linguistics. If they want to refer to nesting as “right-branching dependency,” that’s OK, though pointless terminological confusion” (Chomsky, p.c., 5<sup>th</sup> August 2009). Chomsky further claimed: “Their [Elman et al., CB] results are negative because they do not distinguish these from the very rare and marginal crossing dependencies. The Dutch cases are the ones I was referring to. They are very marginal, and are understood involve special rules and properties, hence do not belong in this discussion apart from footnotes, at least if it is intended to be a contribution to science” (Ibid.). This supercilious dismissal is problematic for at least two reasons. First, Elman never made claims regarding crossing dependencies. Given that it has been known for a long time that human speakers have more difficulties processing center embeddings than right branching structures (e.g., (Blaubergs & Braine, 1974; Blumenthal, 1966; Blumenthal & Boakes, 1967; Cairns, 1970; Fodor & Garrett, 1967; Larkin & Burns, 1977; Marks, 1968; Miller & Isard, 1964; Schlesinger, 1968 – cited by Weckerly&Elman, 1992) he drew an explicit distinction between these types of structures which Chomsky lumps under the term ‘nested dependencies’. Second, work by Christiansen (1994) on crossing dependencies showed that these are processed similarly to center embeddings. This might be surprising given that the latter are so rare in human language. Discussing the implications of this work in detail would of course be desirable. But the blanket dismissal of a caricature of Elman’s work does not contribute to a better understanding of how humans process language.

involving different types of recursive structures. One could, of course, question *these* findings and/or the conclusions Elman draws from his work. But neither Chomsky nor McGilvray cites Elman's work. Instead, they dismiss it based on an obvious misinterpretation. In addition, explicitly referring to the added references may well create the entirely wrong impression that Chomsky's interpretation *is* based on Elman's work.

Such deception violates generally accepted scientific conduct. It also makes one wonder about the sincerity of Chomsky's writings about morality: "If you regard yourself as a moral agent – you're trying to think about your actions, or plans, or ideas that might make human life better" (p. 101). Any moral agent has an obligation to think about the consequences of misrepresentation, especially an agent who claims in the same publication in which this misrepresentation occurs: "We apply to ourselves the same standards we apply to others – probably more rigorous standards if you're serious" (Ibid.).

#### 4. On Interpretation

I am grateful to some of the discussants at the *Faculty of Language* blog<sup>3</sup> for bringing to my attention, that [at least] two sentences in the passage below are ambiguous.

There is a lot of discussion these days of Dan Everett's work with a Brazilian language, Piraha – it's described in the *New Yorker*, among other places. David Pesetsky has a long paper on it with a couple of other linguists [(Nevins, Pesetsky, Rodrigues 2007)], and according to them, it's just like other languages. It's gotten into the philosophical literature too. Some smart people - a very good English philosopher wrote a paper about it. **It's** embarrassingly bad. **He** argues that this shows that it undermines Universal Grammar, because it shows that language isn't based on recursion. Well, if

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<sup>3</sup> Full details of the discussion are at: <http://facultyoflanguage.blogspot.ca/2013/02/whats-chomsky-thinking-now.html?showComment=1361615751775#c7817763063087520177>

Everett were right, it would show that Piraha doesn't use the 'resources that Universal Grammar makes available (p. 30, my emphasis).

The referent of the bolded 'it' could be either 'a paper' [written by a very good English philosopher] (as I had assumed in my review, p. 4) or 'Dan Everett's work'. Subsequently, the referent of 'he' could be either 'a very good English philosopher' or 'Dan Everett'. Both readings seem legitimate because both Everett and Papineau have claimed that the Piraha findings undermine UG, and Chomsky has criticized both for claiming this.

Given that a reviewer ought to use care in her interpretation, did she have an obligation to discuss this ambiguity? Such an obligation can of course only arise if the ambiguity is recognized as such. In this case we seem to have a Neckar-cube type Gestalt shift and people who 'see' one interpretation do not 'see' the other unless it is pointed out to them (again see the discussion linked in the footnote). Therefore, the reviewer might respond: Chomsky could have easily eliminated the ambiguity by using either 'the paper' or 'Everett's work' instead of the ambiguous 'it'. Since he wrote ambiguously, he has to be prepared that people misinterpret him. Such response is not entirely unreasonable.

A different response is to compare both interpretations and decide which one was likely intended. If this is not possible one might adopt (but is not obligated to) the interpretation that is more charitable. I will attempt this below. The passage gave rise to three criticisms:

1. Chomsky did not mention Papineau's name, making it difficult for the reader to identify whom he referred to.
2. Chomsky called a book-review in the popular press a 'paper', which in the context of the discussion was likely to be mistaken for an 'academic paper'.

3. Chomsky alleged 'it' was embarrassingly bad without providing sufficient evidence to substantiate such a charge.

The ambiguity has no effect on (1) and (2) but only on (3). Here two scenarios (A) and (B) are possible:

- (A) Chomsky alleges Papineau wrote *one* embarrassingly bad paper and the only potentially relevant evidence he provides for this claim is: "He argues that this shows that it undermines Universal Grammar, because it shows that language isn't based on recursion" (p. 30). Anyone who has read Papineau's review knows that, in addition to the one claim Chomsky mentions here, it contains a discussion of many different issues. So calling the entire review 'embarrassingly bad' based on only one claim made in this review seems an exaggeration at best.
- (B) Chomsky alleges Everett's work is embarrassingly bad. This is of course a far more serious allegation than claiming that just one paper is embarrassingly bad as in case (A). What is the evidence Chomsky provides to support such a serious charge? First, he refers to one publication (Nevins et al., 2007) that challenges Everett's finding. But he fails to mention that Everett has responded numerous times to this publication, and that there is an ongoing debate about this issue. Some linguists share Chomsky's view that Nevins et al. have refuted an important claim made by Everett, others disagree. Therefore, in a volume aimed at "specialists *and* newcomers" (Pietrosky, back-cover, my emphasis) Chomsky should have mentioned the publications challenging Nevins et al. (2007). He could have provided arguments against these or, at the very least, leave it up to his readers to decide which evidence they find more convincing. Further, even assuming Nevins et al (2007) were correct, they challenge one particular finding in

one particular language. Given that Everett has published numerous findings on several languages, and work in anthropology, showing that he had made a mistake would hardly support the claim that ‘Everett’s work is embarrassingly bad’.

Next, Chomsky supports his allegation by claiming:

But that’s [refuting UG based on the Piraha evidence, CB] as if you found a tribe of people somewhere who crawled instead of walking. They see other people crawl, so they crawl. It doesn’t show that you can’t walk. It doesn’t show that you’re not genetically programmed to walk [and do walk, if you get the relevant kind of input that triggers it and are not otherwise disabled].

This analogy is problematic because it presupposes what it aims to establish: that UG exists and enables us to learn a language with recursion (the property Piraha allegedly lacks) if we receive the relevant input. Everett challenges this very claim and proposes (based on much more than the recursion data) that the UG hypothesis should be rejected. This claim might be false but this needs to be established. And, again, even if it were false, it does not follow it is that Everett’s work is ‘embarrassingly bad’. To support that claim one needs further evidence

Finally, Chomsky claims: “What Everett claims probably isn’t true anyway” (p.30). He provides no evidence to back up this allegation. Instead, he continues “but even if it were, that just means this language has limited lexical resources and is not using internal Merge ... No language uses all the options that are available” (p. 30). This indicates that Chomsky has not sufficient evidence to establish that what Everett claims is not true and one has to wonder why the claim was made in the first place. Regardless of what the justification for the ‘even if he were right’ qualifier is, it hardly can be used as evidence to support the claim that Everett’s work is embarrassingly

bad. Given that this is the only discussion of Everett's work in *Science of Language* it appears that Chomsky has made a very serious allegation against a fellow linguist without providing sufficient evidence.

Regardless of which interpretation of this ambiguous passage is correct, Chomsky did not follow the high standards for academic conduct he proposes for others. Again, one has to wonder about the sincerity of "We apply to ourselves the same standards we apply to others – probably more rigorous standards if you're serious" (p. 101). Furthermore, given that (B) is a far more serious violation of the relevant standards than (A), this passage also illustrates how important it is that an author takes his responsibility as writer seriously and provides unambiguous statements. Not everywhere in *Science of Language* has the poor quality of writing as potentially damaging consequences as here but it should not have occurred anywhere.

## **5. Putting things into context**

It has been suggested repeatedly that I took things out of context, selected only examples that supported my conclusion, and ignored those that would undermine it. I have two replies to this allegation. First, the examples I discussed in the review should not have appeared in *any* published academic volume called "The *Science of Language*", far less in one by the celebrated intellectual leader of linguistics. Second, these examples are no exceptions in an otherwise flawless volume but representative of the quality of the work. Below are further examples of "arguments" offered by Chomsky and accepted by McGilvray without questioning.

*Science of Language* contains numerous allegations that researchers outside the Chomskyan framework are irrational (e.g., “It’s a highly irrational approach to inquiry” (p. 20), “... that’s not a contribution to scientific rationality” (p. 105), “... a tribute to human irrationality” (p. 116), “That’s just irrational” (p. 123)) but none of these criticisms are based on analysis of specific work. Instead, denigration of the work of mostly unnamed others and blanket accusations are the rule.

If you look at the articles in the technical journals, such as, say, *Science* or *Nature*, most of them are pretty descriptive; they pick around the edges of a topic, or something like that. And if you get outside the hard-core natural sciences, the idea that you should actually construct artificial situations in an effort to understand the world - well, that is considered either exotic or crazy. Take linguistics. If you want to get a grant, what you say is “I want to do corpus linguistics” - collect a huge mass of data and throw a computer at it, and maybe something will happen. That was given up in the hard sciences centuries ago. Galileo had no doubt about the need for focus and idealization when constructing a theory. (Chomsky, p. 19)

This description stands in stark contrast to how work in biology, the science in which Chomsky locates linguistics, is usually described. Scientists and funding agencies are aware that data collection cannot be divorced from theory construction: “Of necessity, both the interpretation of experimental data and the design of new experiments depend on extensive and sophisticated theoretical analysis of the possible relationships that can be brought into consistency (or inconsistency) with the data at hand” (Fox-Keller, 2002, p. 236). It is, of course, possible that *some* work might be of questionable value, and in *some* cases projects might get funded because they are data oriented. But Chomsky condemns a whole field without citing a single problematic case.



In addition to unsupported accusations, Chomsky repeatedly makes assertions that are not mutually consistent. At times, the members of such incoherent sets are contained in a single answer:

*If somebody can tell me what a general learning mechanism is, we can discuss the question. But if you can't tell me what it is, then there's nothing to discuss. So let's wait for a proposal.* Hilary Putnam, for example, has argued for years that you can account for cognitive growth, language growth and so on, by general learning mechanisms. Fine, let's see one.

Actually, there is some work on this which is not uninteresting. Charles Yang's (2004) work in which he tries to combine *a rather sensible and sophisticated general learning mechanism* with the principles of Universal Grammar, meaning either the first or the third factor - we don't really know, but something other than experience - and tries to show how by integrating those two concepts you can account for some interesting aspects of language growth and development. I think that's perfectly sensible.

Here Chomsky asserts simultaneously (i) that he is unaware of *any* sensible account of general learning mechanisms *and* (ii) that Yang's work concerns a sensible and sophisticated general learning mechanism. Surprisingly, McGilvray, a professional philosopher, did not question this incoherent set of assertions.

It seems the only criterion Chomsky applies consistently is claiming work that is done in his framework is superior, scientific, and rational while any other work is inferior, unscientific, and irrational. This is illustrated by the following typical dismissal:

And connectionism seems to me about at the level of corpuscularianism in physics. Do we have any reason to believe that by taking these few things that we think - probably falsely - that we understand, and building up a complex structure from them, we're going to find anything? Well, maybe, but it's highly unlikely. Furthermore, if you take a look at the core things they're looking at, like connections between neurons, they're *far* more complex.

They're abstracting radically from the physical reality, and who knows if the abstractions are going in the right direction? But, like any other proposal, you evaluate it in terms of its theoretical achievements and empirical consequences. It happens to be quite easy in this case, because they're almost nonexistent. (Chomsky, 2012, p. 67)

Again, it is remarkable McGilvray accepts this "argument" without any questioning. What are "these few things"? How does the complexity of connections between actual neurons differ from that of the models Chomsky never identifies? Why is abstracting away from physical reality problematic when done by connectionists but a hallmark of good science when done by Chomsky? Recall that he advocates: "... abstracting away from the whole mass of data that interests the linguist who wants to work on a particular language" (p. 84). This abstracting had been justified by Chomsky because "Galileo had no doubt about the need for focus and idealization when constructing a theory" (p. 19). So one is implicitly told to accept, without any evidence, that Chomsky can know that his abstractions aim in the right directions but connectionists cannot know this.

Finally, one finds: "But, like any other proposal, you evaluate it in terms of its theoretical achievements and empirical consequences. It happens to be quite easy in this case, because they're almost nonexistent." Chomsky never reveals what *the proposal* is. Connectionism is a complex field that cannot be reduced to one proposal. Claiming that an entire field lacks theoretical achievements and empirical consequences without providing a shred of evidence challenges Postal's judgment of a passage from Chomsky (2002) as "the most irresponsible passage written by a professional linguist in the history of linguistics" (Postal, 2009, p. 2009; for details see Postal, 2004, chapter 11). And this entirely unsupported claim is not the only contender for this dubious honour.

Similarly irresponsible remarks are scattered throughout *The Science of Language*. Here are a few examples of the many attacks on other individuals or other fields: “And if you get outside the hard-core natural sciences, the idea that you should actually construct artificial situations in an effort to understand the world – well, that is considered either exotic or crazy” (p. 19), “modern philosophy of language and mind...is just off the wall on this matter [=externalism, CB]” (p. 26), “representational theories of mind are bound to a concept of representation that has ... no particular merits as far as I know” (p. 32), “*Nobody* in linguistics works on the meaning of WATER, TREE, HOUSE, and so on; they work on LOAD, FILL,- and BEGIN - mostly verbal concepts” (p. 35, emphasis added), “Take a look at the history of the advanced sciences. No matter how well established they are, they almost always turn out to be wrong” (p. 38), “... this crazy theory of Michael Dummett’s, that people don’t know their own language” (p. 57), “[connectionists] start from the simplest thing we understand – like a neural connection – and make up some story that will account for everything” (p. 67), “Behavioural science is, in principle, keeping to the data; so you just know that there’s something wrong with it” (p. 67), “selectionism...which is [a form of] popular biology... is like a sixth grade version of the theory of evolution” (p. 68), “And [in contemporary neurophysiology, CB] is nothing in the way of any depth of theory. There is a slogan – that the mind is neurophysiology at a more abstract level” (p. 74), “In fact, common sense – at least in the advanced sciences – has been completely abandoned” (p. 75), “Most linguists...are so data oriented that they find it scandalous to accept methodological principles that really ought to be obvious” (p. 84), “From the tides to the flight of birds, the goal of the scientists is to find that nature is simple; and if you fail you’re wrong” (p. 88), “Mysterianism is the belief that our cognitive capacities are part of

the natural world, so therefore these capacities have scope and limits” (p. 97), “Leninism... is a natural position for intellectuals, because they are going to be managers” (p. 98), “... the kind of pop biology that’s common today [in evolutionary theorizing, CB]” (p. 104), “[Dawkins’ work] is not a contribution to scientific rationality” (p. 105), “science shines often penetrating light on extremely simple questions... if the helium atom is too hard to study you give it to the chemists” (p. 106), “formal debates are based on a principle of profound irrationality, namely that you can’t change your mind” (p. 116), “there are distinguished figures who...literally can’t see any difference between adopting what is called ‘innatism’ – meaning scientific rationality – and belief in God” (p. 123), “Most scientists tend to accept the Cartesian dogma” (p. 124), “the entire discussion [about the meaning of the sentence ‘water is H<sub>2</sub>O’, CB] on all sides is basically vacuous. And that’s the primary theme in contemporary analytic philosophy. It’s just not about anything” (p. 127), “You can’t just tell stories about something; you have to show that those stories have some substance. That’s why so much talk about evolution is basically uninteresting; it’s just stories” (p. 128), “[Tyler Burge] is an intelligent person trying to engage with the issues; most philosophers don’t even engage with them” (p. 131), “... it’s one of the joys of evolutionary psychology. You can have it any way you like it” (p. 142), “[using complicated words] tends to make economists like physicists, and then the political scientists want to look like economists” (p. 144), “[social science has] just the superficial trappings of science” (p. 144), “The kinds of questions where real progress has been made are typically very simple ones. That’s part of the reason that physics has made such progress” (p. 145), “[Elman’s theory] is about as interesting as a theory of arithmetical

knowledge that handles the ability to add 2+2 but has to be completely revised for 2+3” (p. 226).

The key point, revealing the essential nature of this work, is that absolutely none of these statements (ranging from trivialities to harsh accusations) is supported by any evidence. Even in cases where Chomsky names individuals allegedly holding the view he objects to, he does not provide any references to their work. Instead he “adopt[s] a god-like point of view” (p. 29) and condemns, trivializes, or ridicules wholesale the work of others.

## **6. Chomsky’s contributions**

One reader of my review suggested I ought to elaborate Chomsky’s contributions to linguistics, before launching into such harsh criticism of one work. Given the extensive literature celebrating Chomsky’s work (e.g., Leiber, 1975; D’Agostino, 1986; Salkie, 1990; Barsky, 1997; Smith, 1999; McGilvray, 2005; Sperlich, 2006; Collins, 2008) this seemed an odd request. But, given the severe criticism that emerged especially after the publication of the minimalist program (e.g., Pullum, 1996; Johnson & Lappin, 1997; Culicover, 1999; Lappin et al., 2000; Levine & Postal, 2004; Postal, 2004, 2012; Seuren, 2004; Culicover & Jackendoff 2005; Jackendoff 2011) an up to date assessment of these contributions might be in order. No one is in a better position to provide such assessment than Chomsky, and, certainly one would expect him to make a strong case *for* the value of his work. When McGilvray asked him about his intellectual contributions Chomsky provided the following reply (cited in its entirety):

JM: Noam, let me ask about what you take to be *your most important contributions*. Do you want to say anything about that?

NC: Well, I think that the idea of studying language in all its variety as a biological object ought to become a part of future science - and the recognition that something very similar has to be true of every other aspect of human capacity. The idea that - there was talk of this in *Aspects*, but I didn't really spell it out - the belief ...

[Wait; I'll start over. B. F.] Skinner's observation is correct that the logic of behaviorism and the logic of evolution are very similar - that observation is correct. But I think his conclusion - and the conclusion of others - is wrong. Namely, that that shows that they're both correct. Rather, it shows that they're both incorrect, because the logic of behaviorism doesn't work for growth and development, and for the same reason, the notion of natural selection is only going to work in a limited way for evolution. So there are other factors. As I said in *Aspects*, there's certainly no possibility of thinking that what a child knows is based on a general procedure applied to experience, and there's also no reason to assume that the genetic endowment is just the result of various different things that happen to have happened in evolutionary history. There must be further factors involved - the kind that Turing [in his work on morphogenesis] was looking for, and others were and are looking for. And the idea that maybe you can do something with that notion is potentially important. It's now more or less agreed that you can do something with that notion for, say, bacteria. If you can also do something with it for the most recent - and by some dimension most complex - outcomes of evolutionary history like language, that would suggest that maybe it holds all the way through. (p. 76)

At first glance this passage seems to hint at a wide variety of research activity. But how much of this research has been carried out by Chomsky? He was asked about *his most important* contributions. He has undoubtedly talked a lot about language as biological object but he has never done any research in biology and certainly has made no contribution to work on, say, bacteria. Further, it is misleading to imply that Chomsky has contributed to theories that could be tested by natural scientists: "... in four decades [Chomsky] has not specified *a single physical property* of any linguistic object" (Postal, 2009, p. 113, original emphasis). The language centers in the brain (Broca area, Wernicke

area, etc.) and genes (FOXP2) involved in language processing were discovered prior to or independently of Chomsky's work. Furthermore, what could possibly be *Chomsky's* intellectual contribution to the idea that: "there's also no reason to assume that the genetic endowment is just the result of various different things that happen to have happened in evolutionary history". He attributes this 'insight' (that there must be further factors involved) to Turing; so whatever its value, it is not Chomsky's contribution. Remaining as genuinely Chomskyan contributions are some unspecified talk in *Aspects* that never got spelled out and the refutation of Skinnerian behaviourism. These contributions date back more than 45 years and are best described as contributions to psychology. This leaves Chomsky, *by his own account*, without any important specifically linguistic contribution.

Even more startling than this admission is McGilvray's reaction. Far from expressing surprise about the absence of any identifiable contributions he suggests Chomsky's work led to "pretty *radical progress* [because] we're actually at the stage now where we can begin to ask for language the old question, "Why are things the way they are?" (McGilvray, p. 77, emphasis added). Celebrating the arrival at a stage where we can *begin to ask* an old question, as radical progress, would be unusual under any circumstances. But, Chomsky already had announced that we were in a position to formulate precise fundamental questions in the first edition of *Cartesian Linguistics*:

[Modern linguistics] has sought, *with much success* to achieve significantly higher standards of clarity and reliability than those reached in earlier studies of language. At the same time, there has been continuing interest in theoretical questions that *led to significant clarification* of the foundations of linguistics. *These advances make it possible to formulate, in a fairly precise way, the fundamental question* of how experience and maturational processes interrelate within the framework of innate limiting conditions to yield the linguistic competence exhibited by a normal speaker of a language (Chomsky, 1966, p. ix, emphasis added)

Here Chomsky spoke of success that had been achieved and advances that made it possible to formulate precise questions. McGilvray, who has edited two subsequent editions of *Cartesian Linguistics*, could not have been unaware of the fact that what he calls ‘radical progress’ is a substantial retreat from earlier pronouncements. This is by no means a new discovery; critics of Chomsky’s work wrote years ago: “[Chomsky’s] claims and promises made during the early years of his academic activity...have over time largely proved to be wrong or without real content and the promises unfulfilled” (Levine & Postal, 2004, p. 203).

In the passage cited Chomsky seems to agree, and other answers confirm the impression that early promises remain unfulfilled, and that lasting contributions to linguistics are unidentifiable. When McGilvray asks about the strong minimalist thesis, currently *the* centerpiece of the biolinguistic enterprise, Chomsky’s answer begins in a promising way: “Maybe it’s even true” (p. 54). However, this is followed by 237 words of speculation about interfaces, the Norman Conquest, mapping constraints, and “new questions” (but no answers), leading up to this grand finale:

It’s interesting that people have expectations for language that they never have in biology. I’ve been working on Universal Grammar for all these years; can anyone tell you precisely how it works [-how it develops into a specific language, not to mention how that language that develops is used]? It’s hopelessly complicated. Can anyone tell you how an insect works? They’ve been working on a project at MIT for thirty years on nematodes. You know the very few [302] neurons; you know the wiring diagram. But how does the animal work? We don’t know that. (Chomsky, 2012, p. 54)

It was of course Chomsky, who substantially raised the expectations for linguistic work by setting a very ambitious research agenda: “The study of language form will ultimately find



its place in a broader framework that will incorporate considerations of meaning and use, just as the study of grammar will ultimately find its place in a richer investigation of how knowledge of language is acquired” (Chomsky, 1972, p. 119). Four decades later, Chomsky admits that ‘how language develops and is used’ is ‘hopelessly complicated’. In other words, his work has not advanced our understanding of “the creative aspect of language use [which he had made] a central concern of linguistics” (Chomsky, 1966, p. 72). Nor has he given by now a “sharp and clear formulation of some of the central questions of psychology and [brought] a mass of evidence to bear on them” (Chomsky, 1968, p. 59) as promised decades ago.

Instead of taking responsibility, Chomsky claims that no one can tell us how an insect works. Linguists are not comparative zoologists, so what is known about insects seems irrelevant to linguistics. Further, if Chomsky considers the biology of species only distantly related to humans so important for linguistics, it is curious that he apparently does not care about the fact that nematodes are not insects but roundworms. For biologists it is not surprising that the work of his MIT colleagues (likely on the model organism *Caenorhabditis elegans*) does not reveal ‘how insects work’. So this diversion not only fails to establish that we should not expect any results from linguistic work, it also reveals Chomsky’s disregard for, or ignorance of, basic facts of biology; a discipline he claims to have been working in for decades.

Astounding ignorance of biology is displayed throughout *The Science of Language*:

The idea that basically there’s one organism, that the difference ... between an elephant and a fly is just the rearrangement of the timing of some fixed regulatory mechanisms. It looks more and more like it. There’s deep conservation; you find the same thing in bacteria that you find in humans. *There’s even a theory now that’s taken seriously* that there’s a universal genome. Around the

Cambrian explosion, that one genome developed and every organism's a modification of it. (Chomsky, 2012, p. 53, emphasis added)

No argument is given that the difference between fly and elephant can be reduced exclusively to unidentified regulatory mechanisms and, given that bacteria have no language faculty, whatever similarities they share with humans is irrelevant to linguistics. Finally, Chomsky never provides details about 'a theory' or reveals who takes it seriously. The fact that *some* theory exists and is taken seriously by *some* people does not tell us much about its credibility. For example Michael Behe proposed 'a theory' of irreducible complexity, which is taken seriously by many creationists. That does not make it a respectable scientific theory. Any biologist who wants to be taken seriously would provide detailed arguments in support of the widely rejected speculation 'that there's a universal genome'.

In some cases it is difficult to discern what could have been the motivation for Chomsky's answers. McGilvray had asked if we want to allow 'that proving useful is not a condition of a biological entity'. Chomsky replies: "Take D'Arcy Thompson. If biophysical laws determine the general shape of the properties of creatures, it doesn't say that you can't build submarines" (p. 137). I leave it to others to speculate how the building of submarines might be connected to D'Arcy Thompson's work or how either relates to linguistics.

Some of the research projects Chomsky envisions seem similarly bizarre: "An interesting topic that should be addressed some day is that our internal speech is very likely fragments of re-internalized external speech, and the real 'inner speech' is very likely inaccessible to introspection" (p. 12). Before even contemplating how this 'interesting topic' could be addressed, one wonders why evolution would have equipped us with such a completely

unnecessary epicycle. The obscure and contentless character of such remarks is typical of many of the musings that comprise *The Science of Language*.

## **7. Concluding thoughts**

I have argued that *The Science of Language* is neither a volume that illustrates why Chomsky is considered to be “a founding genius of modern linguistics” (Stainton) nor “illuminating for specialists and newcomers” (Pietroski). The interviews fail to make Chomsky’s current position clear to the reader, and the “illuminating explication by interviewer James McGilvray” (Lasnik) instead of providing clarification, adds confusion and misinformation. Some have suggested that a series of interviews cannot be judged by the same standards as peer reviewed articles in professional journals, which is surely true to a degree. One would not expect to be exposed to technical jargon known only to specialists, complicated formal proofs, in depth descriptions of laboratory equipment used in specific experiments, detailed species lists, etc.

However, *any* publication aimed at newcomers to a discipline ought to provide solid information, credible references, and should be free of factual error. The position taken needs to be explained and defended in accessible terms, and competing views need to be objectively evaluated. Surely, this is especially true when the author is *the* leading authority of the field, and a public figure known well beyond the field of linguistics and, hence, enjoys more public trust than most scientists. Experts may be able (but should not be required) to ‘fill in the blanks’ throughout the interviews and correct the countless errors and misrepresentations. But lay readers are not in such a privileged position and depend on

the information provided. And, it is precisely at the level of ‘general introduction to the field’ that *The Science of Language* fails. The problem is not that Chomsky fails to give a sophisticated, detailed account of all currently discussed theories of language evolution but rather that he invents an account that no one defends, seemingly in an attempt to convince the lay reader that language evolutionists are irrational. The problem is not that Chomsky does not describe Lassiter’s challenge to his own view in enough detail but rather that he distorts it beyond recognition. The problem is not that a few ‘just of the press’ articles are not referenced but that the nine page bibliography contains virtually no entries of work that has been savagely criticized, making it impossible for the reader to check the validity of the criticism. This filtering strategy is displayed numerous times throughout *The Science of Language* and can only be described as “gesturing rhetorically to a general public you’re misleading” (Chomsky, p. 105). Embracing a tactic he condemns in others is highly revealing.

### **Postscript**

In his 2013 plenary address to the LSA David Pesetsky claimed the “real problem that should concern us... [is] the absence of right-headed responsible publications in [high profile science] journals that do engage the field of generative syntax and accurately represent its results” (Pesetsky, 2013, slide 102). He suggested that this absence “probably reflects nothing more than the complete ignorance on the part of editors and reviewers that there even is a field and a set of results that should be engaged” (Ibid.). It could be true that the editors of these high profile journals are unaware of [some or most of] the results of

generative syntacticians. But are the reasons for this ignorance exclusively those Pesetsky's gestures at? I suggest at least one reason not mentioned by Pesetsky that could contribute to the current state of affairs.

The undisputed intellectual leader of generative syntax, Noam Chomsky, has refused for over a decade to answer the question what the results of the field are (cf., Chomsky, 2002, 2009, 2010). In the present volume, the editor asked specifically what the most important intellectual contributions are. Chomsky's reply contained no trace of the putatively important work of the linguists Pesetsky lists (Cinque, Rice, Merchant, Legate, Bobaljik). None of them is cited in reference section of the volume nor do their names appear in the index. This absence could possibly be explained by such an abundance of interesting work that Chomsky focused on a sample different from Pesetsky's. However, the only work by a generative syntactician mentioned in Chomsky's reply is Tanya Reinhart's argument from the seventies that "c-command didn't involve linearity, just hierarchy" (p. 79). And not even this work is referenced in the bibliography.

Pesetsky suggests that even in the few cases in which high-profile journals have published results of generative work the coverage of specifically linguistic results is inadequate:

None of these papers, in their published form, contain any linguistic theory, any linguistic analysis or any significant set of linguistic facts.

- Most of the linguistics was excised from Hauser, Chomsky and Fitch, for example, at the insistence of the journal.
- The Royal Academy B article on innateness by Mameri and Bateson is friendly, if clarificatory, to work in generative grammar — but is a bio-philosophy paper, with no linguistics (Pesetsky, 2013, slide 106).

These are regrettable omissions. But it is hardly possible to imagine an editor more sympathetic to Chomsky's work than McGilvray. The question arises what prevented

Chomsky from giving detailed information about important *linguistic* discoveries to McGilvray. Certainly a volume titled *The SCIENCE of LANGUAGE* would be the right place to discuss them and advance the kind of public-relations work Pesetsky considers necessary.

While this volume has been called “illuminating for specialists and newcomers” (Pietrosky) it leaves them entirely in the dark not just about the achievements Pesetsky discusses but virtually any achievements of the field. Similarly, the 2009 volume *Of Minds and Language* is advertised as a “marvelous book [that] provides an engaging portrait of ... the fundamental achievements of more than half a century of research” (Freidin). But again, the reader will search in vain for any of the discoveries Pesetsky considers worthy of front-page news:

The Cinque hierarchy would have appeared first in *Nature*.  
Rice’s discovery of unity-in-diversity in Athapaskan affix-ordering would have merited a front-page article in the NY Times Science section.  
Merchant’s discovery of a link between preposition-stranding sluicing and its overt counterpart would have appeared in the Proceedings of the Royal Society, heralded in the press as “perhaps the final blow to an age-old debate about ellipsis”.  
Legate’s discovery that the left periphery of Warlpiri looks like Rizzi’s left periphery for Italian (and Cable’s for Tlingit) would have been the topic of an hour on NPR Science Friday.  
Bobaljik’s recent discoveries about comparatives and superlatives would have appeared first in *Science*, reported as an AP news item, and ended up as the theme for a joke by a late-night talk-show host (Pesetsky, 2013, slide 71).

Given Chomsky's undisputed status as leader of the generativist movement, no one would be in a better position to educate the allegedly ignorant editors and reviewers about this work. If Pesetsky considers it unacceptable that “the marvelous work of [his] colleagues and students languishes in even firmer obscurity than before” (slide 109), he might ask

Chomsky why reference to this work is entirely absent from his discussion, one that no doubt aims at popularizing the achievements of generative grammar. Why is the reader exposed to endless musings about implausible evolutionary scenarios, unscientific speculations about bacteria, slime molds, nematodes, insects, frogs, chickens, dogs, baboons; fairy tales about baby donkeys, and obscure third factor invocations relating human languages to snowflakes? Why is Chomsky suggesting that we ought to be “abstracting away from the whole mass of data that interests the linguist who wants to work on a particular language” (p. 84) instead of promoting the important work of generative syntacticians? Given that the volume has been described by prominent generative syntacticians as ‘indispensible presentation’ (Lasnik), ‘illuminating’ (Pietrosky) and by a philosopher very sympathetic to the field as ‘truly exceptional’ and ‘cutting-edge theorizing’ (Stainton) this would seem like a perfect place for the editors and reviewers of journals to educate themselves about the state of the art of the field.

Given that no ‘significant set of linguistic facts’ was discussed in the volume, some readers may question the existence of such facts. A recent debate on *Faculty of Language*, a blog designed to educate “linguistic outsiders (and even practitioners) about the foundations and results of the Generative Enterprise initiated by Chomsky” (Hornstein, 2012, blog 1), suggests that such doubts might be justified. The blog’s host proposed that “the various modules within GB elaborate a series of well-massaged generalizations that are largely accurate phenomenological descriptions of UG. I have at times termed these ‘Laws of Grammar,’ ... to suggest that those with minimalist aspirations should take these as targets of explanation” (Hornstein, 2013, blog 13). However, when asked to “provide a full list IN PRECISE FORM of “laws of grammar” as articulated in GB ... [and to] provide a

reference to a work where each “law” is characterized” (Postal, 2013, comment 1), it quickly became clear that no such list could be produced. Instead, Hornstein’s reply contained vague reference to cross over phenomena, local anaphoric licensing, Principle C effects, and that fact that islands cannot separate antecedents from the traces they bind. In his rejoinder to Hornstein’s reply Postal showed that these are either not laws at all or that we currently lack any principled way to account for multiple puzzles not covered by these putative laws.

When further questioned, Hornstein suggested it would be unreasonable to expect “that a field that is all of 60 years old should have reached the point of knowing its fundamental properties” and that modesty and a little charity in judgement are required “to appreciate just how much has been discovered”. Modesty is certainly a virtue, though not one that comes first to mind when dealing with Chomsky’s work. For decades the public has been told that Chomsky is the “Einstein of linguistics” (Leiber, 1975, p. 19), who initiated a “cognitive revolution” (Otero, 1988, p. 14), and founded “a science in the Cartesian-Galilean tradition” (McGilvray, 2005, p. 4) that is “gaining him a position in the history of ideas on par with that of Darwin or Descartes” (Smith, 1999, p. 1). To be valid, such a reputation would have to be based on accomplishments similar to those of Einstein, Descartes, Galileo, and Darwin. If the content of *The Science of Language* and Hornstein’s failure to list laws of grammar that are anything like actual scientific *laws*, let alone laws of similar significance as those discovered by Galileo, Darwin, or Einstein reflect the actual status of accomplishments, then drastically increased modesty appears to be appropriate for generative grammarians.



**Acknowledgements:**

I am greatly indebted to Avery Andrews, Michael Arbib, Derek Bickerton, Paul Bloom, Rudie Botha, Ted Briscoe, Morten Christiansen, Patricia Churchland, Michael Corballis, Peter Culicover, Shimon Edelman, Jeff Elman, Dan Everett, Dan Flage, Jim Hurford, Ray Jackendoff, David Johnson, Dan Lassiter, Robert Levine, Philip Lieberman, Brian MacWhinney, Robert Martin, Frederick Newmeyer, David Papineau, Paul Postal, Michael Studdert-Kennedy, Geoffrey Sampson, Pieter Seuren, Maggie Tallerman, Michael Tomasello, and Virginia Valian for very helpful replies to my inquiries and for commenting on earlier drafts. All remaining errors are mine.

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