

A Potpourri of Chomskyan Science

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1. Introduction

The Science of Language, published in the sixth decade of Noam Chomsky's linguistic career, should have been an impressive summary of the achievements of one of the greatest intellectuals of our time. It is not. Chomsky's scholarship has arguably been slowly deteriorating over decades, and this volume is altogether representative of the problem. I shall argue in this review article that uncritical acceptance of Chomsky's work despite its steadily declining quality has been doing the field considerable damage.

Chomsky is undoubtedly the best-known representative of linguistics. His technical innovations have set and re-set theoretical agendas for those who share his general perspective, and provided considerable input, if only in the form of novel observations, for those who work in different frameworks. He is regularly invited to deliver keynote or plenary talks at leading universities, and remains the public face of linguistics. Assuming that it is correct that “[m]ost educated people have never encountered linguistics, and have no idea what it might even mean to examine a linguistic puzzle scientifically” (Pesetsky, 2013, slide 120), it is to be expected that people who become interested in linguistics will turn their attention first to Chomsky's publications, and judge the field by their quality. But the field should not be judged by publications like *The Science of Language*.

One goal of this article is to engage in what Chomsky advocates as “consciousness raising” (119) and to encourage readers to apply to Chomsky's work the same standards that are applied throughout science. *The Science of Language* has been reviewed elsewhere (e.g. Pullum, 2012; Bishop, 2012; Behme, 2013a; Lieberman, 2013), so readers seeking a more conventional review are already well served. This review article offers only a brief survey

of the book as a whole, before focusing on certain key passages in order to discuss serious shortcomings of Chomsky's recent scholarship. I shall show that his recent work fails to meet serious scientific standards because he rejects scientific procedure, inflates the value of his own work, and distorts the work of others, and that the poor quality of *The Science of Language* is no isolated "misstep" but characteristic of many of his recent (and some not so recent) publications.¹

2. Brief overview of *The Science of Language*

The Science of Language contains twenty-five interviews. 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. Given that the volume aims (at least in part) at "illuminating [Chomsky's linguistics for] ... newcomers" (Pietroski, back cover), the interview format and the frequent focus on philosophical and political issues might seem appropriate. However, as noted by another reviewer, "there seemed no coherent structure... The style is so discursive that it's impossible to précis. [Chomsky's] rhetorical approach seemed the antithesis of a scientific argument" (Bishop, 2012). Indeed, searching for the promised

¹ This review article gives a negative evaluation of Chomsky's work that goes beyond *The Science of Language*. However, it is not an attempt to evaluate the entire body of his work, nor to diminish the importance of the technical contributions he made in publications like *Syntactic Structures*, *Aspects of the Theory of Syntax*, *The Logical Structure of Linguistic Theory*, etc. Many issues only briefly mentioned here are discussed in detail at <http://ling.auf.net/lingbuzz/001592>.

² Naturally, one would not expect novel research in *Cartesian Linguistics* (2009). Yet it advertises a "new and

“cutting-edge theorizing” (Stainton, back cover) proves futile, and any hopes for philosophical engagement with long-standing criticism of the Chomskyan paradigm are in vain. Further, it is difficult, (especially for newcomers), to follow the presentation, because terms are not clearly defined, the conversation meanders through multitudes of obscure, irrelevant digressions, fallacies of reasoning are committed, and far-reaching conclusions are often drawn from meagre premisses. The only attempt in the entire volume at a syntactic analysis (for the sentence “Harry saw a house”) is a sketch outlined by McGilvray in one of the appendices (232).

The Science of Language has a variety of shortcomings, which can be summarized as follows. Chomsky dogmatically defends a view that is visibly out of touch with recent research in formal linguistics, developmental child-psychology, computational modeling of language acquisition, and language evolution. He fails to provide novel (or at times, any) arguments supporting his belief in a domain-specific innate biological endowment, saltational language evolution, semantic internalism, or computational optimality. Instead, he proclaims as irrefutable truth what is hotly contested outside the Chomskyan camp. Most of the arguments provided are sketchy repetitions of controversial arguments that Chomsky put forward years or even decades ago. There is virtually no reference to work by Chomskyans that had not been already discussed in recently published volumes, such as *Cartesian Linguistics*² (3rd edition, 2009), *Of Minds and Language* (2009), *Noam Chomsky on Cognition and Language* (2009), and *Chomsky Notebook*³ (2010). At times Chomsky’s

² Naturally, one would not expect novel research in *Cartesian Linguistics* (2009). Yet it advertises a “new and specifically written introduction by James McGilvray, contextualizing the work for the twenty-first century” (back cover), and contains a letter by Chomsky commenting on computational work.

³ This volume was originally published as *Chomsky* (2007) in French.

replies to McGilvray's questions are almost identical to passages from such earlier works⁴. This turns the current volume into a meta-interview: pieces of earlier interviews are recycled and cobbled together in quilt-like fashion.

Apart from the poor quality of argumentation, *The Science of Language* displays numerous violations of basic publication standards. For instance, a mismatch in footnote reference pagination is allowed to accumulate from p.16 (the relevant footnote refers to p. 17) to p. 141 (the relevant footnote refers to p. 123), and work criticized by Chomsky is not cited properly in the text (e.g., "that guy" (57); "a very good English philosopher" (30)), and not listed in the reference section (Lassiter, 2008; Papineau, 2008). While ultimately Chomsky is responsible for his remarks, a share of criticism must also be attributed to the interviewer James McGilvray, and to the editors at the distinguished academic publisher. Why did they not enforce higher standards?

The problems listed above would be serious flaws in any academic work. But *The Science of Language* is no isolated instance of poor scholarship and substandard editing. The public has been exposed in short succession to five books with Chomsky's name on the cover, all of which fail to live up to enthusiastic back cover endorsements by prominent generativists (Paul Pietroski, Howard Lasnik, Robert Freidin, Robert Stainton). Despite these failings, the books have elicited the highest praise in reviews published in reputable venues: "[*Chomsky Notebook*] is one of the most comprehensive, sensitive, and imaginative

⁴ None of these earlier works reported original research either. There is some discussion of biological and psychological work in *Of Minds and Language*. But discussion of novel linguistic findings has been absent from Chomsky's publications for a long time.

representations of Chomsky's oeuvre ... The volume opens with two majestic essays by Chomsky himself” (Mukherji, 2009, *Notre Dame Philosophical Reviews*); “[*Of Minds and Language*] is a book remarkably rich in ideas” (Kljajevic, 2009, *Linguist List*); “the papers [in *Of Minds and Language*] are uniformly thoughtful and provide an excellent guide to some of the best thinking on biolinguistic themes” (Drummond & Hornstein, 2011, *Biolinguistics*). Negative evaluations (Pullum, 2012; Postal, 2012; Behme, 2013b) have elicited open hostility. All this suggests, especially to non-linguists, that work which fails to meet even minimal standards for scientific publications is held in high esteem by professional linguists.⁵

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

⁵ This is the end of the text copied from the JL RA. For the full article please access it at: <http://journals.cambridge.org/action/displayAbstract?fromPage=online&aid=9258234>

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 17th and 20th century a scientist of the 44th 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 21st 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)

Even though Lohndal & Narita (2009) is found in the bibliography (from which Lassiter (2008) is missing), 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 ones⁶” (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

⁶ 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., 5th 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. 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. It is of course possible that Chomsky had different definitions of 'learning mechanisms' in mind when he talked about Putnam's and Yang's work. But he specifies any relevant difference and, McGilvray, a professional philosopher, did not ask for clarification but accepted an 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.

5. 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, 2012a,b, 2014; 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. Had a student in one of my classes given that answer I would have failed her. Where is any reference to his technical innovations that have set and re-set theoretical agendas for those who share his general perspective and provided a considerable input, if only in the form of novel observations, for those who favour different frameworks? Instead of speaking about his field-on-fires-of excitement-setting linguistic work he make vague and confused comments about evolution and biology. He only mentions one of his many publication (*Aspects*) but not for its contributions to linguistics. Many in his intended audience who are unfamiliar with *aspects* must go away from this passage thinking it was a book about biology.

During his career Chomsky 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. He does not remind Chomsky that the reader would benefit from a detailed account of the contributions that Chomsky has made in the 1950s, which resulted in a new-orientation of linguistics. Instead, he suggests vaguely 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*.

9. Deep problems

Pesetsky (2013) claims that the scientific community is largely ignorant of work by generative linguists. Given the problems I have discussed, one almost hopes he is partly right, and not too many scientists have read Chomsky’s recent work. But, given that Chomsky is the best-known linguist, one must assume that especially non-linguists would read his books before they read any other work by linguists. Any scientist who has read *The Science of Language* will notice its dubious quality and must wonder why such work is apparently⁷ still held in the highest esteem by many generative linguists. And even if readers of *The Science of Language* would not be discouraged from reading works by *other*

⁷ I use the qualifier ‘apparently’ because the lack of published criticism of Chomsky’s recent publications by prominent generativists (e.g., Pesetsky, Nevins, Hornstein, Freidin) does not necessarily imply that they hold his work in high esteem. However, given that the same generativists have expressed scathing criticism of work by others, one cannot rule out that the appearance is correct. It would be up to those (and other) generativists to make their position regarding the value of Chomsky’s recent work clear, especially to the general public.

linguists, they would find barely any reference to recent linguistics work in the bibliography.

In his 2013 LSA keynote address Pesetsky had listed discoveries by generative syntacticians he considered so exceptional that, in a better world, they would receive front-page attention:

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).

It may be indeed regrettable that this work does not receive more attention from non-linguists. But, one certainly would expect that such allegedly ground breaking work is discussed in a volume titled *The Science of Language*. Yet, when McGilvray 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. None of the authors 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 important recent work that Chomsky focused on a sample different from Pesetsky's. However, the only

work by a generative syntactician even 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.

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 of journals like *Science* or *Nature* about this work. If Pesetsky considers it unacceptable that "the marvelous work of [his] colleagues and students languishes in even firmer obscurity than before" (Pesetsky, 2013, 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, amoeba, slime molds, nematodes, insects, frogs, chickens, dogs, baboons, to fairy tales about baby donkeys, and to 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?

Pesetsky has expressed his frustration about the fact that papers by generative linguists in high profile science journals are devoid of specifically linguistic content: "None of these papers, in their published form, contain any linguistic theory, any linguistic analysis or any significant set of linguistic facts" (Pesetsky, 2013, 105). It is easy to understand his frustration. What is harder to understand is that he does not express similar frustration

about the fact that none of the recent books by Chomsky “contain any linguistic theory, any linguistic analysis or any significant set of linguistic facts”. It is possible that the editors of science journals insisted to remove linguistic content from publications. But it is hardly possible to imagine an editor more sympathetic to Chomsky and the work done in generative grammar than McGilvray. He has given Chomsky an excellent opportunity to advertise linguistics to the general public and remedy the “deep problem” Pesetsky identifies: “Most educated people have never encountered linguistics, and have no idea what it might even mean to examine a linguistic puzzle scientifically” (Pesetsky, 2013, slide 120). Yet, how could the interested reader get an idea about how exciting linguistics is when she reads:

Well, what’s Universal grammar? It’s anybody’s best theory about what language is at this point. I can make my own guesses. There’s the question of lexical items – where they come from. That’s a huge issue. Among the properties of lexical items, I suspect there are parameters. So they’re probably lexical, and probably in a small part of the lexicon. Apart from that there’s the construction of expressions. It looks more and more as if you can eliminate everything except just for the constraint of Merge. Then you go on to sharpen it. It’s a fact – a clear fact – that the syntactic objects you construct have some information in them relevant for further computation... (41)

This eclectic collection of jargon might be informative for some generative linguists. But how are “most educated people” who have never encountered any linguistics supposed to comprehend this passage? Again, Pesetsky has excellent advice to offer: “...it’s hard work figuring out a way to make your discovery understandable to someone who knows nothing. Prepare for that!” (Pesetsky, 2013, slide 120). It is regrettable that, apparently, this advice was never offered to Chomsky.

Postscript

Since the first publication of this review several works have appeared, documenting in great detail that Chomsky's linguistic work has been deeply flawed for decades, that he made knowingly false claims, and that he failed to respond to legitimate criticism (e.g., Postal, 2012b, 2014). These works establish that the problems discussed above are no isolated late career incidences. Instead, several of Chomsky's "proposals across the decades from 1962-2002 ... [have been] thorough instantiations of the concept 'play acting at linguistics'" (Postal, 2012b, p.1). For example, in 1964 Chomsky proposed a universal grammar principle later referred to as A-over-A principle. This principle was shown to be untenable by Ross (1967/86), and Chomsky (1972) acknowledged in print that the problems raised by Ross were legitimate. Postal documents that the A-over-A principle never had the status of a legitimate universal grammar principle. Yet, for decades Chomsky used the A-over-A principle as an example of an important linguistic discovery he had made, and misled especially non-linguistic audiences about the status of this purported universal principle.

The accusations made by Postal are severe: "[Chomsky] was simply *deliberately deceiving* a (non-linguist) interviewer to make it seem he had uncovered a serious principle when he had to know that he had not!" (p. 8, original emphasis), "[Chomsky] simply lied to his nonlinguist interviewers Schenker and Mehta, and slightly less directly, to all of the many readers of these interviews" (p. 9), "[Chomsky's] failure to mention these issues of course helped disguise the failures of his proposal" (p. 10), "[Chomsky] was deliberately falsifying the scientific status of his theoretical claims" (p. 11), "an attempt to gull a partially

nonprofessional readership into thinking the [A-over-A] principle has an explanatory function” (p. 16), , “[Chomsky] gratuitously and falsely denigrate[d] the work of *one of [his] own students* just to make [his] own seem more important” (p. 31). Postal concludes that “from the point of Ross (1967 [1986]) on, talk of the A- over-A Principle was mostly disgraceful, self-promotional pretense, scorning any element of genuine integrity” (p. 32) and emphasizes “the truly ludicrous quality of the thirty year plus long series of invocations of ... a grotesquely false condition” (p. 44).

Chomsky has a well-deserved reputation for responding to criticism of his work with fierce counterattacks, often implying his critics are not able to understand his work: “Lakoff presents a very confused picture of the issues that have been under discussion ... [and is] confused beyond repair” (Chomsky, 1973). “[Cooper] has, however, ... seriously confused the issues” (Chomsky & Katz, 1975, p. 71). “McCawley has missed the point of my remarks” (Chomsky, 1980b, p. 47). “The post-1979 shift that Pullum perceives is imaginary... Pullum’s conclusions are based on serious misunderstanding throughout” (Chomsky, 1990, p. 147). “Godfrey-Smith cites only the last phrase quoted, misreading it as ... exactly the opposite of what the passage unambiguously states” (Chomsky, 1995, p. 2), “[Putnam’s] argument ... might hold if one could make some sense of it; such an argument would not however, be relevant here, even if it could be given in some coherent form” (Chomsky, 2000, p. 66), “[Dennett’s] report of informal discussion, several years ago, on which his account is in part based, seems to me rather misleading, to say the least” (Chomsky, 2000, p. 200), “[Deacon seemed to] reshape standard problems of science as utter mysteries, placing them beyond any hope of understanding, while barring the

procedures of rational inquiry that have been taken for granted for hundreds of years” (Chomsky, 2002, 83).

In addition, Chomsky has questioned the mental fitness of intellectual opponents: “[Bloomfield] was such a schizophrenic that he couldn’t bring it together” (Chomsky, 2009, p. 342), “Arguments of the sort that Matthews presents are unfortunately all too common in the linguistic literature. They are based on assumptions that can only be regarded as pathological” (Chomsky, 1986, p. 149), and even compared them to criminals “[Aarsleff published] outright falsifications . . . flat lies . . . couched in a rhetoric of a sort that might be appropriate to some criminal” (Chomsky, 1984, cited in Hill, 2007, p. 636). It is curious that a man who tolerates as little disagreement with his position as Chomsky never responded to Postal’s criticisms. Several of Postal’s most outspoken publications were published in recent years. Has Chomsky mellowed in his attacks of opponents? There is no indication for this.

In a recent response to Margaret Boden’s (2006) work Chomsky wrote: “virtually every reference to me or to (unidentified) co-workers around the world, and to the areas in which we work, is fanciful, sometimes even bringing to mind Pauli’s famous observation ‘not even wrong’” (Chomsky, 2007). He called her work “an ugly insult to hundreds of serious and productive researchers” (Ibid) and suggested that “[h]er conception of an argument, repeatedly, is to quote someone who agrees with her judgments. QED. It doesn’t matter what the facts are” (Ibid). Chomsky’s rebuttals of Boden rarely rise above invective and reveal astounding hypocrisy. He claims repeatedly she fails to provide any evidence for her

claims, especially that she fails to name the targets of her criticism: “participants in the vast ‘Chomskyan’ conspiracy -- vilified, but unnamed” (Ibid.). Yet, his rebuttals contain several references to (allegedly) hostile critics and colleagues without naming them: “she dismisses [Chomsky’s ‘theory of minimalism’, CB] as ‘risible’ (on the basis of a quote from a hostile critic). It cannot be ‘risible, because it does not exist” (Ibid). Here an unnamed hostile critic is accused of gross incompetence. The reader has no means to verify Chomsky’s charge, and can only wonder why this is not an ‘ugly insult’, especially since it has been uttered by someone who claims that “We apply to ourselves the same standards we apply to others – probably more rigorous standards if you’re serious” (Chomsky, 2012, p. 101).

In the current context, it is irrelevant, whether Chomsky’s allegations against Boden (and unnamed hostile critics/colleagues) are true. Of interest is that he reacted swiftly and ferociously to criticism of his work. So, one has to ask why he has remained silent in face of Postal’s repeated and very explicit criticisms (e.g., Postal 2004, 2009, 2012a,b). One reason could be that Postal’s criticism was not severe enough to warrant a reply. Only a cursory reading of the Boden rebuttal rules out this possibility. Consider:

Boden’s account of my ‘relentless’ battle with behaviorism opens with a section entitled ‘Political agenda,’ in which she asserts that I ‘saw [behaviorism] in political terms’ and was driven by ‘political passion.’ That is another serious charge, based as usual on zero evidence. (Chomsky, 2007)

Here Chomsky calls Boden’s assertion that he was driven by political passion a ‘serious charge’. There is no necessary connection between one’s political passion and the quality of

one's scientific work. It only becomes a problem if one promotes scientific work of inferior quality for political reasons. But Boden never claimed that Skinner's behaviourism was superior to Chomsky's framework. So the 'serious charge' is considerably less severe than Postal's charges listed above. In addition, Postal has claimed in print that Chomsky's work is "a mixture of intellectual and scholarly corruption" (Postal, 2009, 247) and stated explicitly that he considers Chomsky's framework incorrigibly flawed: "my view is that not only is there no such thing as bio-linguistics as this term is understood in the work Chomsky has influenced, *there cannot be such a thing*" (Ibid. p. 248, original emphasis).

Possibly, then, Chomsky never replied to Postal because he considers his allegations to be entirely false, and unworthy of any attention. His refutation of Boden rules out this possibility as well. In the passage cited above he continues:

The 1959 article has not even the remotest hint of any political concern. Boden's evidence about a 'political agenda' comes from a more general article on psychology and ideology many years later that dealt in part with Skinner's *Beyond Freedom and Dignity*. Since Skinner's book had a very explicit 'political agenda,' my review, like every other one, saw it 'in political terms.' The charge that my linguistic work reflects a 'political agenda' is repeated throughout, on the basis of her usual source: quotes from a hostile critic, backed by no evidence. Readers may judge for themselves the intent of these fabrications. (Chomsky, 2007)

Again, it is not relevant whether Chomsky's reply is factually correct. He holds that Boden's claims are false, calls them 'fabrications', 'backed by no evidence'. Yet, he replies to her charges in print. Postal's charges are backed by massive evidence, consisting mostly

of citations from Chomsky's work. They are far more serious than any of Boden's charges. Yet, Chomsky never replied in print. Undeniably, he has a right to remain silent. But this silence "leaves unimpeded the inference that he has not attempted a refutation [of Postal, CB] because he cannot" (Postal, 2009, p. 248). Leaving this inference unimpeded seems to be a more severe problem for those promoting the Chomskyan framework than any problems listed by Pesetsky (2013). And yet it continues to remain unaddressed by either Chomsky or his most vocal and prominent supporters.

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