A Paradigm Shift in Linguistics?
Ill-conceived Dogma and The Language Myth
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Foreword

Following the publication of my 2014 book, *The Language Myth*, I was approached, in 2015, by the editors of *Language*, the Linguistic Society of America's flagship journal, with the idea to solicit and publish five peer commentaries on *The Language Myth*, offering me the opportunity to respond with a response article. Moreover, I was also invited to provide names of five potential reviewers. The editors selected one peer commentator, from my proposed list, Adele Goldberg, and solicited peer commentaries from four others. The five commentaries, together with my response, were due to appear in the December 2015 issue of Language. But, as I was preparing my response, I was informed that the editors had received an unsolicited offer from a sixth reviewer to provide a commentary; they decided to accept this, from Iris Berent, and further, decided to delay publication until the March 2016 issue. Following submission of my response article, Helen Goodluck, the Review Editor for Language, wrote to me on 30th December 2015 informing me that *Language* declined to publish my response article. Instead, I was invited to submit a new response, with constraints being imposed on me as to what I should address, prescribing the sorts of things I was allowed to say, and limiting the number of words at my disposal. I declined to comply. In my open response, to the editors of Language, which is available from my website,[1] I explained my decision in the following terms:

"In imposing what I perceive to be unreasonable constraints on my response, you are evidencing bias towards the minimalist perspective, whether you accept that or not. And you are setting a dangerous precedent, in acceding to the pressure that has been applied to you by leading opponents of The Language Myth, those who would shut down any debate, and self-evidently do not believe in the principle of fair play in scientific discourse. This is dangerous for the pluralism of our discipline, and does great damage to the standing of *Language* as an impartial arbiter on theoretical matters."

The six peer commentaries—the first time in its history that *Language* has reviewed a book in this novel way—was published in the March 2016 issue of *Language*. Yet, this publication was marked by the peculiar distinction of not including a response from the book's author. What follows is the response that I submitted to *Language*, which the editors declined to publish, with levels of censorship that were unacceptable to me. I respond, in each of the substantive sections, to one key objection to *The Language Myth*, by each critical commentator; I also selected these as they echo complaints made about the book by Chomskyan Minimalists. I do not respond to the one positive review, by Goldberg (2016). For a representative critical review of the book by a leading minimalist, see Adger (2015), and for my response to that review see Behme and Evans (2015). For a representative positive review of the book by a cognitive-functionalist, see La Polla (2016).

Introduction

There is no doubt that the turn in theoretical linguistics pioneered by Chomsky, and his co-workers and followers, represented a hugely important development in language science, and cognitive science more generally. With Chomsky's intervention, for arguably the first time, in Anglo-American linguistics, language was viewed as a mental phenomenon. The questions that linguists

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[1] http://media.wix.com/ugd/603cc2_ac75b021850544f7ab6e3e501db3f068.pdf

subsequently began to ask, viewing language from this perspective, were and remain hugely important. And theoretical linguistics of the Chomskyan variety, taking a nativist perspective on the nature and status of language, has led to a voluminous, and impressive literature, relating to everything from language description, to language acquisition, to language processing, to cross-linguistic variation, to language pathology, amongst others.

In terms of the history of ideas, the significance of this theoretical impetus does and will continue to resonate. Many of us who, nevertheless, disavow this perspective were either trained in it, or else have in some way been influenced by it. And of course, The Language Myth (TLM) would not exist if it were not for Chomsky, and the popularisation of his ideas, and those of his collaborators and students, by Steven Pinker. In one sense, regardless of the view one takes on TLM—and everyone is entitled to their opinion—a book-length rebuttal such as the one under discussion, especially one that has attracted such infamy or praise (depending upon one's perspective), is the highest compliment that can be paid to the many achievements of the Chomskyan paradigm.

In this response article, I can do no more than touch on a smattering of the issues raised by the peer commentators, whom I thank for their insight and thoughtful reactions—even where I strongly disagree with their perspective. Space precludes a response to the very many interesting reactions, positive and negative, and in a number of cases, misunderstandings, contained in the preceding commentaries. Rather, in what follows, I have selected four prominent points or themes, that, in various ways, have attracted attention across the commentaries. My discussion of each is also informed by aspects of the wider context and issues that have surrounded the book's reception, and the theoretical issues and concerns at stake. In part, I hope that this tack will both help clarify the rationale for having written the book, the reason for the presentational style and tone, as well as elaborating further on my own views on some of the burning issues in contemporary theoretical linguistics, in which linguists of all persuasions, I believe, ultimately, have a stake.

On scientific paradigms
In their commentary article, Ackerman and Malouf (2016) advocate moving beyond the sometimes acrimonious, combative division between competing party lines. They argue that "theoretical linguistics should exhibit vigorous, substantive cross-theoretical debate both about analyses of particular phenomena and the general assumptions and methodologies that guide competing analyses". I agree that tolerant, open-minded dialogue between linguists of different theoretical persuasions is both desirable and, in principle, could better serve the reputation of the field, and better enhance progress in the object of enquiry. But for reasons I address in this section, this lofty ideal is, alas, probably not a realistic proposition.

Science can only proceed by asking questions, which build on gaps in existing knowledge. But questions are always framed in terms of a prevailing theory or paradigm. Moreover, perhaps unfortunately, theories, like cultures and people, are social phantasms, which come with baggage – good, bad and even downright ugly. Theories have leaders (often iconoclasts) as well as followers, foot soldiers and even groupies. Belonging to the right theoretical camp can, as with all sociocultural groupings, be used to leverage power. Any successful theory can bestow financial backing via research grants, and promotion, serve to recruit a steady stream of willing and intelligent graduate students who will go out and evangelise; and it bestows tenure, ensuring an intellectual empire. The consequence is that, almost inevitably, academics are invested in their theory, intellectually, emotionally and materially, and seek to ensure its propagation. And from this perspective, the rise of competing theories, that might otherwise reduce influence, perhaps inevitably, can lead to intellectual battle lines being drawn.

Prior to the publication of Thomas Kuhn's 1962 book, *The Structure of Scientific Revolutions*,

it was widely assumed that science involved a process of steady progress. Accepted facts were added to, and knowledge accumulated steadily, over time. But Kuhn showed that science could also develop in abrupt ways, where conceptual continuity can be punctuated by periods of scientific revolution. Kuhn compellingly argued that the discovery of "anomalies" can lead to the emergence of a new paradigm. And new paradigms have a habit of scrutinising the data in new ways, asking different sorts of questions, and from different perspectives. And the result is that the rules of the game are changed in the process.

One of the observations that Kuhn made is that on their own, findings of fact that militate against a theory, won't falsify it. This follows as scientific paradigms are more than mere epistemological entities, consisting of findings based on reason and supported by facts. A scientific paradigm is an ideological organism, and as intimated earlier, given sustenance by people, of flesh and blood, whose careers and even livelihoods are invested in the paradigm.

When confronted with an anomaly, one that doesn't fit with the predictions made, Kuhn observed that this can lead to a crisis for the paradigm. And one response to crisis is that a paradigm's defenders will "devise numerous articulations and *ad hoc* modifications of their theory in order to eliminate any apparent conflict." (Ibid: 78). Chomsky himself is quite explicit on this strategy. At various points in his writings, Chomsky has advocated what he dubs a Galilean approach to linguistic enquiry.

Writing in 2002, Chomsky claimed that: "[Galileo] dismissed a lot of data; he was willing to say: "Look, if the data refute the theory, the data are probably wrong." And the data that he threw out were not minor". (Chomsky 2002: 98). He continues saying that "the Galilean style . . . is the recognition that . . . it often makes good sense to disregard phenomena and search for principles" (Ibid.: 99), by "discarding recalcitrant phenomena," (Ibid: 102). And in 2009, in his opening remarks in Piattelli-Palmarini et al. (2009), Chomsky explains, in describing his scientific" approach: "You just see that some ideas simply look right, and then you sort of put aside the data that refute them" (Ibid.: 36).

One way of viewing the changing face of the various distinct theories that have populated the Chomskyan paradigm, over the years, is that in addressing recalcitrant phenomena/data, in the Galilean style, Chomsky has attenuated the Universal Grammar hypothesis; the putative formal and substantive universals of Chomsky (1965), evolved into the 'toolkit' approach of Principles and Parameters in the 1980s, and subsequently a further, pared-down version in the Minimalist Programme, positing a single fundamental principle amounting to merge/recursion—although there seems to be disagreement as to what this might amount to, even in Chomsky's own writings, including the relationship, if any, between the two.

In Everett (2005), data and argumentation was famously provided, that, Everett claimed, suggested that at least one language Pirahã, failed to exhibit recursion. Of course, Everett could be incorrect, and may have over-interpreted his data, leading to a conclusion not warranted by the linguistic facts, as suggested by Nevins et al. (2009)—although see Futrell et al. (In press), for the most recent analysis of Everett's Pirahã corpus. And whether or not Everett is correct, for present purposes, is beside the point. The issue of note is the response to this claim: if Everett were correct, then this would cause theoretical discomfort for the revised Universal Grammar hypothesis. But as Kuhn observed, anomalous phenomena—and the lack of Pirahã recursion, if correct, would certainly fit the bill—can be circumnavigated by an *ad hoc* move. Writing with colleagues Fitch and Hauser in 2005, Chomsky makes exactly such a move: "the putative absence of obvious recursion in one of [the human] languages . . . does not affect the argument that recursion is part of the human language faculty [because] . . . our language faculty provides us with a toolkit for building languages, but not all languages use all the tools" (Fitch et al.: 203-204). This Kuhnian *ad hoc* move avoids a potentially problematic issue for the Chomskyan paradigm.

Another move, and one that is potentially disingenuous, is to go beyond the recalcitrant data, and attack the opponent directly. In a 2009 Brazilian newspaper, *Folha de San Paulo*, reporting on an interview, provided to the newspaper by Chomsky, Chomsky was quoted as

describing Everett as a "charlatan"; the article headline was: "Ele virou um charlatão", diz Chomsky. Whatever one's perspective on Everett the person, this amounts to a clear attack on the credibility, motivations, and research ethics of the opponent, with potentially damaging consequences for Everett's subsequent access to the Pirahã.

The upshot of this discussion is this. Whether we as linguists want it or not, and despite the position advocated by Ackerman and Malouf, theoretical paradigms do appear to entail elements of hostility towards what are perceived as competitor theories/paradigms/perspectives. And this follows because so much is at stake in terms of the relative success, or otherwise of a particular theory, for the very human academics that are invested in them. Both TLM itself, and the response by some, to its publication, can, I think, be instructively viewed in this light.

Squaring the Chomskyan Circle and the issue of falsifiability
In his commentary, Deen (2016) takes issue, amongst other things with an apparent non-sequitur I make in TLM. My error, he opines, is to suggest that Chomsky's proposition—that elements of grammar form part of the human biological endowment—amounts to an assumption. As he puts it, "It was not Chomsky's *assumption* that our knowledge of language is an integral part of our genetic endowment, rather it was his *conclusion".* From this perspective, the error propagated throughout TLM, presumably, is fail to recognise a valid conclusion, based on findings of fact, and to misrepresent this apparent conclusion as a presumption. So what is the conclusion?

The idea, of course, is that humans possess a species-specific Universal Grammar, which constitutes a biological pre-specification for language—one that's innate—that constitutes an "initial state", enabling a cognitively-normal human child to learn a language: any language. It amounts to, possibly many, different genres of information—propositions, constraints, and so forth—that enable a child to acquire their mother tongue, that are not otherwise facilitated by more general learning mechanisms. In short, Universal Grammar is the initial-state of grammatical knowledge, that each child is born with, and which underpins any and all languages, enabling a child, perhaps together with more general learning facilities, and other factors, to learn a language based on the linguistic input—the "blooming, buzzing confusion", to borrow a phrase from Williams James—that a child encounters around it, in its early years of life. In short, it amounts to the specifically linguistic content—biologically prescribed—that enables a child to acquire a language, that could not come from elsewhere, or which would not be predicted by any other types of experience and/or mental and/or developmental and/or physiological abilities and mechanisms.

Neil Smith, writing in 2005, puts this as follows: "One of Chomsky's achievements is to have demonstrated that, despite the easily observable richness in the world's languages, there is really only one human language: that the complex richness and bewildering array of different languages surrounding us are all variations on a single theme, most of whose properties are innately given." (Ibid.: 21).

But for something to count as a conclusion it must, presumably, be based on findings of fact. But from the earliest stages in Chomsky's oeuvre, the conclusion, was based, largely, on poverty of the stimulus arguments. And this was a genre of argumentation that rested, not primarily on careful observation of linguistic facts, but, in essence, was a logical argument, given a set of arguably questionable assumptions. Indeed, the cogency of poverty of stimulus arguments have been questioned by developmental psychologists (e.g. Tomasello 2003; MacWhinney 2005) and linguists (e.g. Pullum and Scholz 2002; Sampson, 2002). So, for the conclusion to be substantiated, linguistic data is required to support what Smith claims to have been "demonstrated".

In the 1960s, Chomsky's conclusion amounted to the claim for what he dubbed formal and

substantive universals. Substantive universals were grammatical categories such as lexical classes—noun, verb, adjective and adverb—and grammatical functions like subject, and object: what we might think of as the basic 'building blocks' of grammar. Chomsky (1965: 66), suggested that languages select from a universal set of these substantive categories. Formal universals are rules such as phrase structure rules, which determine how phrases and sentences can be built up from words, and derivational rules, which guide the reorganisation of syntactic structures, allowing certain kinds of sentences to be transformed into or derived from other kinds of sentences (for example, the transformation of a declarative sentence into an interrogative sentence). But as the facts of linguistic diversity and variation emerged, especially with the emergence of the field of linguistic typology, it increasingly appeared that couching universals in these terms was untenable.

By the 1980s, a revised, and more flexible approach to Universal Grammar had emerged, dubbed Principles and Parameters. Informally, the idea was that, the constraints that populate our biologically pre-specified language faculty consist of grammatical principles that can be parameterised—set in different ways—for different languages. Switch the parameter one way rather than another, and you get a cascade of effects that makes a language like English look very different from, say, the indigenous Australian language Jiwarli. But in terms of the initial biological state, we all approach languages from the same starting point, prescribed by our common Universal Grammar. Summarising the state of the art, in his 1994 book, *The Language Instinct*, Steven Pinker summarised this perspective in the following way:

> It is safe to say that the grammatical machinery we use for English . . . is used in all the world's languages. All languages have a vocabulary in the tens of thousands, sorted into part-of-speech categories including noun and verb. Words are organized into phrases according to the X-bar system [the system used in an earlier version of Chomsky's theoretical architecture to represent grammatical organization] . . . The higher levels of phrase structure include auxiliaries . . . which signify tense, modality, aspect and negation. Phrases can be moved from their deep structure positions . . . by a . . . movement rule, thereby forming questions, relative clauses, passives and other widespread constructions. New word structures can be created and modified by derivational and inflectional rules. Inflectional rules primarily mark nouns for case and number, and mark verbs for tense, aspect, mood, voice, negation, and agreement with subjects and objects in number, gender and person. (Ibid.: 238).

But as it has turned out, there is credible evidence that most, if not all, of these claims for language 'universals' are falsified by specific languages that differ, often in startling ways from English (e.g., N. Evans and Levinson 2009). From the mid-1990s onwards, the grammatical machinery that might constitute the initial state of Universal Grammar was down-sized further, under the aegis of the so-called Minimalist programme. The current state of the art appears to be that there is a single innate operation, termed Merge—a general purpose computation, parameterised in different ways across languages, that enables the recursive—i.e., combinatorial potential of language(s)—such that any given language can combine syntactic units in a range of language-specific ways (e.g., Hauser, Chomsky, Fitch 2002). And this, thereby, gives rise to the observed complexity of grammar in and across the world's languages. But the consequence of this down-sized Universal Grammar is that other factors have to be invoked to account for linguistic variation.

For instance, Chomsky (2005) argues for three factors that are required to account for language (universals): i) the innate, biological pre-specification (aka Universal Grammar), ii) experience, and ii) non-linguistic factors, such as growth, development, and so forth. In short, today, very little, in relative terms, remains that is specifically innate, part of the biological endowment and

unique to Universal Grammar. And moreover, these so-called 'second' and 'third' factors must now play a huge explanatory role in accounting for the nature and structure of language, it's diversity, and how it is acquired. In short, in the course of around 50 years, proposals as to what amounts to the grammatical information that constitutes our biological endowment—Universal Grammar—has progressively shrunk.

If a theory must be so radically updated, so often, in order to keep accounting for new findings arising from the ongoing discoveries in terms of cross-linguistic variation, one cannot help but wonder whether it is valid to describe a commitment to a universal and biological pre-specification for grammar to amount to a "conclusion". Of course, this might be warranted if other aspects of the theory were unproblematic, or if there were no viable alternative theories. But there are alternative accounts which, arguably, are indeed viable.

In addition to field research that has demonstrated that not all human languages share language universals of the Chomskyan kind (e.g. N. Evans and Levinson, 2009), syntacticians have shown inadequacies in data interpretation in the Chomskyan paradigm (e.g. Postal, 2004; Jackendoff 2011). Moreover, experts on social cognition have shown how language structure is shaped by language use (e.g. Enfield and Levinson 2006; Everett 2012; Tomasello 2008), while computational modellers have simulated aspects of language acquisition previously claimed 'unlearnable' without the aid of a putative Universal Grammar (e.g. Christiansen and Chater 1999; Clark and Lappin 2011; MacWhinney 2010); cognitive-functional linguists have developed theoretical alternatives to grammatical organisation, (e.g. Croft & Cruse 2004; Evans and Green 2006; Geeraerts and Cuyckens 2007), and evolutionary theorists have provided arguments against the minimalist version of Chomskyan Universal Grammar (e.g. Arbib 2012; Deacon 1997; Hurford 2011; Jackendoff and Pinker 2005; Lieberman 2013; Tomasello 2008). In light of all this, it is far from clear whether it has, in fact, been demonstrated that there is biologically prescribed knowledge that is specifically linguistic, that could be claimed to be universal in the Chomskyan sense. And given the changing status of what such knowledge might amount to, in the face of the new findings, it seems to me at least, that Chomsky's position is much better labelled an assumption, rather than a conclusion.

In short, my reading of Chomsky—and he is notoriously hard to decipher, even for those with far greater familiarity with his work than me—is that Universal Grammar is an axiom—a self-evident truth, not in need of evidence—rather than a conclusion. Moreover, even commentators who, in principle, may be sympathetic to the general thrust of the Chomskyan paradigm appear to have arrived at a similar assessment. For instance, Pinker and Jackendoff (2005) suggest that contemporary research within the paradigm proceeds on the "presumption that the Minimalist Program is ultimately going to be vindicated" (Ibid.: 222).

This all brings with it further difficulties, in terms of falsifiability. There are at least two issues here. First, the search for universals in language, à la Chomsky, is based on circular reasoning: evidence for Universal Grammar is to be found in linguistic behaviour. But any evidence for Universal Grammar is predicated on the prior assumption that there is such a thing as Universal Grammar to begin with. Moreover, the linguistic "evidence" is being used in order to infer the existence of the Universal Grammar that is claimed to sanction it. It is not clear to me how this enables linguistic data of any kind to count as "evidence"; still further, how someone can maintain, with a straight-face, that Universal Grammar is a conclusion (based on findings of fact).

The second, centres on the status of Universal Grammar as a claim about human biology. The claim for a Universal Grammar, in essence, amounts to a biological, rather than a linguistic claim: whatever it is that all languages may have in common, the language faculty, common to all humans, is a consequence, ultimately, of hereditary. And, indeed, it's difficult to imagine how one *would*—or even *could*—go about testing whether there is a biological pre-specification for language, especially if we were to rely on linguistic analysis alone, or at all.

More generally, a consequence of Universal Grammar amounting to an axiom is this: not only is it not testable, given that it is a biological, rather than a linguistic claim, being an axiom—a self-evident truth—it is not in need of testing. This notion of being 'testable' amounts to the issue of

falsifiability: the litmus test for good science. Reality must be able to bite, at least potentially, in the form of counter-evidence. But as the proposition—that language is biologically pre-specified—is not testable, it is not, in principle, falsifiable. And being unfalsifiable, it is, immune to counter-evidence.

Charles Darwin was one of the earliest practitioners of what has, since the nineteenth century, become the standard scientific method. In essence, science involves developing a model based on prior observations. And then, subsequently, the model is tested against further observations, in order to assess whether the model correctly accounts for these subsequent observations; the model is examined, against these observations, to see whether it correctly predicts the phenomena in question: whether it's true or false. And if counter-evidence is provided, then the model is revised in the light of this. But, at least on my reading of Chomsky's recent remarks, (e.g., Chomsky 2012) this may not constitute a problem for Minimalism, given his novel, Galilean approach to science—see my earlier discussion of this.

In short, because Universal Grammar is an axiom—an article of faith—it's more or less acceptable to put inconvenient data aside, or even, to ignore it altogether; otherwise, this inconvenient data would get in the way of the search for the principles that populate the biologically pre-specified Universal Grammar—those that Chomsky presumes to be there.   While my bleak assessment may be unpalatable for some, I genuinely struggle to see an alternative way to account for the seeming disconnect between the Chomskyan paradigm, the linguistic data that it purports to account for, and the very different conclusions reached by many others, some of whom were cited earlier in this section.

Setting the right tone
In his commentary, Wijnen (2016) objects to the tone of TLM. As this is something others have commented on, it is appropriate to address this very issue in this response article, if only briefly. Wijnen puts his complaint in the following terms: "The tone of the book is thus polemic. It is also occasionally tendentious, and the author does not eschew derogatory qualifications of the school of thought he is attacking or its representatives, suggesting that advocates of linguistic nativism are not objective and not sensible."

The first thing to say is that nowhere do I describe anyone as a charlatan. The second is that TLM is self-evidently not a scientific monograph. It is a popular-level overview of a broad range of areas, written with a specific goal in mind. The tone of presentation was quite deliberate, and opinions appear to be divided on this issue. Those that object to the book's tone seem, more or less, to correlate with precisely those who subscribe to the views that TLM seeks to rebut. But my overall purpose, in writing the book in the way that I did, was to serve as a jolt to the field of theoretical linguistics. In so far as *Language* has taken the trouble to commission the preceding peer commentaries, together with this response, this counts as a measure of success in this regard.

The worldview associated with Chomskyan linguistics, while, at one time, novel and exciting, when the knowledge base of language science was much less than at present, now represents, in my view, a significant impediment to progress in getting to grips with the nature of language, and its interconnected relationship with mind and culture. This is because the hunt for an elusive, hard-wired, Universal Grammar means that linguists can ignore difficult facts that don't fit the theory. They are also absolved from fully engaging with findings from other disciplines in the brain and behavioural sciences, views that often militate against a nativist account, à la Chomsky—nowhere is this truer than in advances currently being made in fields pertaining to the evolution of our genus. For my money, not only is this deeply unscientific, it is also deeply distracting, absorbing intellectual, material and human resources.

The picture we now have of language is one of immense complexity. And language appears

to be symbiotically related to a raft of mental processes and mechanisms that, today, provides a reticulated picture of way in which language hooks up with other aspects of cognition, both from an evolutionary perspective as well as in terms of language processing in the mind of any cognitively-normal human alive today. The essential insight of the Chomskyan paradigm, developed in the 1960s is, in certain respects no longer apposite given the current state of the art in the 21st century, especially in other disciplines relevant to the study of language.

And this leads to a related point: the accusation of the brush strokes being too broad in the book to provide a sufficiently detailed picture of the positions under scrutiny. Berent (2016), in her review, takes issue with what he perceives to be the relatively superficial treatment of modularity, for instance, in TLM. But again, TLM is not a monograph. It does not examine a single issue in detail, poring over the minutae of divergent viewpoints, and lines of evidence used to support these. It is, after all, intended for a general readership. Of course, one is entitled to an opinion about whether I've included too little, or too much detail, or ignored relevant issues altogether, evaluating the book accordingly. But from an objective perspective, I wonder how fair it is to hold TLM to the same standards of coverage and rigour one would legitimately expect of a monograph. Of course, advocates of the views targeted in TLM might, arguably legitimately, invoke the broad strokes used in the book as grounds to dismiss it. But those colleagues were not for whom the book was written—if it were, the tone would have been different for a start. And in the final analysis, the motivation for writing the book was to target the Chomskyan paradigm, and to put its peculiar worldview under the microscope. In view of this, my presentation sought to invoke representative arguments in favour of Universal Grammar, and arguments that militate against it, in terms that could be grasped by an educated lay audience. And just to be clear, the book takes a biased stance on the matter—I'm not attempting to kid anyone that I'm a disinterested bystander.

What's the alternative?
In this final section, I address the alternative way of thinking about language and mind that I present in TLM. In his commentary, Hinzen (2016) dismisses the alternative I present in the following terms: "The hypothesis that humans are specially equipped with special 'pro-social' or 'communicative' intentions would be promising if there was evidence for the *kind* of communicative intentions that we express in language, in the absence language."

This kind of objection, in my view, amounts to a failure to fully grasp the nature of human communication, and the relative contribution of language to it. It also suggests a lack of awareness of much of the recent literature on the subject. For professional linguists, language is, naturally, of especial significance. And perhaps for some, it's difficult to conceive of communication proceeding in the absence of language.

Yet, since the 1950s, the field of nonverbal communication, conducted largely under the aegis of disciplines such as anthropology and psychology, has examined the significance of paralinguistic cues, such as prosody, as well as kinesics (including facial expression and gesture) in human communication. On one estimate, 65-70% of the meaning humans derives from spoken, face-to-face interaction in social contexts derives from these nonverbal cues (McDermott 1980). And of course, as has been tellingly observed by others (e.g., Levinson 2006; Tomasello 2008), communication can proceed in the absence of language altogether. High-profile cases in the popular press attest to this.

In 2002, for instance, a contestant on the UK Quiz show, *Who wants to be a millionaire?* was convicted of deception, in winning the £1,000,000 jackpot, by using an accomplice, in the studio

audience, who coughed just as the correct answers were read out. A cough can be a reflex behaviour, but it can also serve as an intentional signal conveying meaning. Indeed, language—whether spoken or signed—could not function, as the paradigmatic exemplar of a communicative system without a sophisticated socio-cognitive infrastructure which both enables and supports it, including paralanguage and kinesics (Tomasello 2008, 2014). Cooperation, in the Gricean sense, appears to be central to our ability to successfully deploy language (Levinson 2006). And on my account, which I elaborate in the sequel to TLM, *The Crucible of Language* (Evans 2015b), this pro-social impulse, in evolutionary terms may have provided the necessary preconditions for language to emerge in the first place.

The communicative functions associated with language are evident in non-linguistic systems, such as the home-sign studied by Goldin-Meadow (2015), as well as the way in which, since 2011, the popular use of emoji in mobile internet-equipped devices has mushroomed. For instance, in January 2015, a NY teenager was arrested for an alleged terroristic threat, by posting emoji on his FB page, which was perceived as threatening violence against members of the NYPD (Evans 2015a). This, arguably, the world's first alleged emoji terror crime, invoked the self-same ideational and interactive-interpersonal communicative functions associated with language. While language, it seems, is not necessary for successful communication, it self-evidently massively amplifies the human communication potential.

In light of these sorts of findings and observations, I argue that the raison d'etre for language is to better facilitate communication, predicated on the "shared intentionality", studied by Tomasello (2014) and others, that appears to be unique to our species. From this perspective, language is a consequence of a deep-seated cooperative intelligence, which emerged gradually over evolutionary time, a consequence of a shift in the cognitive strategy that emerged in ancestral humans, enabled by adaptions to a new bio-cultural niche within the last few million years. These are some of the issues taken up in Evans (2015b).

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