

On not having read Itkonen: empiricism and intuitions in the generative data debate*

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This rejoinder demonstrates that López-Serena's (2009) critique of Riemer (2009) is unfounded. LS's critique is characterized by errors of fact and interpretation, a failure to distinguish positions which are being conceded from those being argued, and an arbitrary redefinition of the terms of the debate. Furthermore, LS's conceptions of intuition and scientificity, which motivate her criticisms, prove to be highly problematic.

1. Introduction

One of the most striking aspects of the relationship between generative syntax and the wider discipline of linguistics has been the lack of concern with which generativists themselves have mostly reacted to theoretical criticisms from other quarters. Linguistics abounds with challenges to the various generative models of language developed since the 1950s, but these challenges rarely meet with any sustained engagement from the followers of the generative paradigm itself. The feeling seems to be that success is its own best criterion: the question of the value of a theory is only to be measured by its actual explanatory achievements, and there is little point confronting differing *a priori* theoretical positions in the absence of specific results. This is even more the case since generativists and their critics typically do not agree either about what the object of linguistic investigation itself actually is, or what an appropriate 'result' of linguistic research would be like. For this reason, criticisms of generativism often amount to little more than juxtapositions of the theory's foundational positions with those of the critic. In this situation, generativists' indifference to the views of anyone who disagrees with their basic presuppositions may not be the unhealthy ostrichism that it may seem, but can more charitably be understood as a *de facto* commitment to theoretical pluralism, and a recognition that alternative paradigms are best left to pursue their own explanatory agendas in peace, until such time as they have results which can be meaningfully contrasted.

In 'Grammaticality as evidence and as prediction in a Galilean linguistics' (henceforth GEP) to which López-Serena (henceforth LS) has taken the trouble to reply, I argue in favour of this theoretical pluralism. Both generativists and their critics have good reasons for their theoretical preferences, and since the empirical and methodological issues over which their disagreements crystallize are far from straightforward, there are no obvious or self-evidently correct answers to be had. It is, I believe, simply unclear what the right side of the generative/anti-generative debate is, as is frequently the case with ongoing research in all kinds of empirical inquiry (a term whose appropriateness to linguistics LS would reject). The best attitude, it seems to me, is to allow both camps to pursue their investigations, in the hope that things will gradually become clearer. In the current fog of uncertainty, and in the face of as intractable, poorly-understood and complex a phenomenon as natural language – however 'natural language' is to be precisely construed – there is simply no place for stipulation about what the correct direction for linguistic research is. We are not, in other words, at the stage where a choice between generativism and its alternatives is desirable. The intellectual fertility of linguistics as a discipline will only be diminished if avenues of investigation are prematurely cut off by a categorical insistence on what the right way to study language is.

This is not to say, however, that either side should be immune from criticism. LS and I seem both to share the belief that there are serious problems with the generative approach to language as currently practised. But we differ significantly both in our general strategies of criticism, and in the details of our objections, and it is these differences which motivate LS's reply to GEP. LS's article contains two related lines of argument. First, it calls attention to various claimed deficiencies in GEP. Second, it defends an alternative set of theoretical concepts in order to deal with notions like grammaticality, acceptability, and other key terms of the generative data debate. These two aspects of LS's article are connected: it is, apparently, only if LS's own theoretical framework is substituted for the one native to GEP that the contradictions and insufficiencies of the latter are revealed. As I argue here, while the comparison LS undertakes between her own theoretical frame of reference and that of generativism is welcome, her criticisms of GEP itself are all unsuccessful.

LS claims to discover various basic 'contradictions' and 'misconceptions' in the argument made in GEP, concluding that the position defended there 'makes no sense' (p. [5]). If LS is right, this would be rather a devastating criticism. Fortunately, however, it will not be hard to show that these claims of contradiction and misconception are entirely mistaken. It is only because LS has imposed an alien and inappropriate theoretical frame of reference onto the basic terms of the generative data debate that the case made in GEP strikes her as incoherent. LS consistently refuses to accept the presuppositions of the grammaticality/acceptability debate as it is usually construed, including in GEP – indeed, as it *has* to be construed, if it is to be a debate about grammaticality and acceptability in generative linguistics, rather than one about similar notions elsewhere in the discipline. One would have thought that it is only worth criticizing a particular contribution to a longstanding controversy if one is prepared to accept at least some of the ground-rules. LS, however, prefers to argue not in terms of the standard concepts which define the grammaticality/acceptability debate in generative linguistics, as reflected in a well known and voluminous literature, much of it cited in GEP. Instead, she bases her criticisms of GEP on a baffling array of unrelated substitute notions. Thus, against the background of a rejection of the very possibility that linguistics ever *could* be empirical science, she redefines the generative notion of *grammaticality* as 'correctness' (p. [5]), and that of *acceptability* as 'rationality' (p. [7]); she substitutes 'normative non-observational facts' for the *empirical facts* which generativists typically see themselves as studying; and, following Itkonen (2003), substitutes a view of data-gathering through intuition-consultation as 'reminding oneself of norms' (p. [3]), for the generative notion of such data-gathering as *experiment*.

This mirror-world of alternative notions is so remote from the debate as it is standardly framed that it is unclear to me why LS thinks it gives her a basis for her criticisms. LS certainly has every right to try to change the terms in which the debate about norms in linguistics is conducted, and there is, of course, interesting theorizing to be done using the concepts which her reply defends. It is quite possible that the framework she promotes will show, or already does show, greater promise than the generative one. Nothing in the argument made in GEP precludes that possibility. But the fact that LS sees so much wrong in both generativism itself and in criticisms which are prepared to concede some ground to it makes her reply to GEP entirely beside the point. To start this rejoinder, I will clear the air by briefly recalling the argument of GEP and correcting several unambiguous mistakes which LS makes in her characterization of it (section 2). I will then discuss her failure to

distinguish positions which are being conceded from those being argued (section 3), refute her claims of incoherence in both GEP and in Chomsky (section 4), and consider the relevance of the theoretical frame of reference she wishes to substitute for the one assumed in GEP (section 5). Finally, I will discuss her attitudes to intuition and to scientificity (section 6).

2. Clearing the air

The argument offered in GEP is that, whatever the validity of generativism's foundational assumptions, its methodology is not consistent with its own stated requirements. The argument establishes 'a way of couching the criticism [of generativism's use of grammaticality intuitions] which does not ignore Chomsky's original definitions of grammaticality and acceptability, and which does not as a result fail from the very outset' (p. [12]). The aim of this is to 'supply both critics and defenders of the generative use of intuitions with a more useful focus of discussion' (p. [12]). GEP argues that if there are as many bad stars in generative argumentation as some critics think, then this is a problem for the theory's predictive power and, as a result, a direct challenge to its claims to be 'Galilean' science. This is a significantly different argument from the one usually mounted: typical criticisms of generative methodology interpret the bad star problem as a problem not about the theory's predictive power, but as one about its evidentiary base, a criticism that can easily be evaded by appealing to Chomsky's original distinction between grammaticality and acceptability and to his claims about the role of idealization in empirical research.

The profitability of theoretical disagreement depends on the participants accurately representing each other's position. Since LS offers what she sees as an improvement to the argument of GEP, it comes as something of a surprise to discover that she has misconstrued this in basic ways at a number of points, making numerous errors of fact and interpretation. In this situation, there are two possibilities. Either the original author has been so careless in mounting their exposition that any reasonable reader would interpret the argument incorrectly, or it is the critic who has not been careful enough. In what follows, I will assume that the latter is the case. The reader, however, should obviously consider both possibilities. Most of LS's misconstruals call for some discussion, which will be found in the following sections. A few, however, can be dismissed summarily, by demonstrating that LS has simply missed the point of the argument in GEP, or that all that is at stake is an elementary misreading or error of fact. These errors are scattered throughout LS's reply, and any attempt to correct them risks being piecemeal. The exercise is, however, necessary: facing a claim of incoherence, an author has the obligation to point out occasions where their critic has made basic interpretative or factual mistakes. This is particularly so since the mistakes in question concern fundamental aspects of the argument of GEP: the relation between grammaticality and acceptability, the definition of acceptability itself, the extent to which generative linguistics is claimed by GEP to be Galilean science, the status of intuition in current generativism, and the role of 'theoretical intentionality' in GEP. I deal with these in turn; subsequent sections of this rejoinder take up many of the issues in greater detail.

Does GEP advocate 'eliminating' the grammaticality/acceptability contrast?

LS states at a number of points (pp. [1, 5, 18]) that GEP promotes 'eliminating' the distinction between grammaticality and acceptability. This is manifestly not true. As plainly and prominently stated at many points (e.g. in the heading of subsection 4.1, p. [11]), GEP argues that there is no way of *operationalizing* the notion of grammaticality other than as

acceptability. (Note that LS, by contrast, thinks that ‘grammaticality cannot be confirmed at all through any evaluation criterion other than grammaticality itself’ (p. [6]).) This is completely different from the claim which LS attributes to me, namely that the distinction between grammaticality and acceptability should be ‘eliminated’, entailing that the two notions are *identified*. The argument made throughout GEP (e.g. p. [2, 11, 13]) is that ‘the only way we can check that a theory’s predictions of grammaticality are accurate is by assuming that they will align with acceptability judgements more often than not in those cases where we have no reason to suspect the influence of extrinsic performance factors on acceptability’ (GEP, p. [13]). There is no ‘elimination’ of the difference between the notions here. Indeed, only if the two are different can they ‘align’ most of the time. Grammaticality remains a theoretical notion, applying to I-language; acceptability is a notion relating to E-language. That the two concepts are distinct is obvious in the sentence from GEP just quoted: if ‘extrinsic performance factors’ can influence acceptability, provoking a clash with grammaticality, clearly the two must be different. That LS fails to realize this constitutes, it seems to me, a serious error.

What is acceptability?

The concept of acceptability itself is obviously central to GEP. It is crucial, then, that it be properly defined. But LS does not consistently argue in terms of the Chomskian definition of acceptability established in section three of GEP (p. [9]), which forms the sole basis for the ensuing discussion there. She states, for example, that ‘[f]rom some statements in Riemer (2008), it could be inferred that he equates acceptability with stylistic appropriateness’ (p. [7.]). There is no warrant in GEP for this inference. The sentence from GEP which LS quotes to justify this claim concerns the *difference* between ‘stylistic intuitions of correctness or felicity’ and grammaticality/ acceptability intuitions (GEP p. [4]), so in no way justifies her conclusion of an *equation* between acceptability and stylistic appropriateness. Indeed, the very next sentence of GEP draws an explicit distinction between these two notions: ‘Many constructions demonized in the prescriptive grammatical tradition as incorrect or bad English are in fact bad *only from a stylistic point of view*, and perfectly *acceptable in contexts (like informal spoken language) where stylistic considerations are unimportant*’ (emphasis added). LS’s claim that I conflate acceptability with ‘stylistic appropriateness’ is even more puzzling since she herself quotes another sentence where I precisely distinguish the two notions. Thus, I ask for a distinction ‘to be drawn wherever possible between intuitions of acceptability and intuitions of preferred style’ (GEP p. [4], quoted by LS, p. [7]). The only claim made in GEP is that stylistic considerations *may, mistakenly*, be conflated with considerations of grammaticality or acceptability, and that this conflation should be avoided.¹

Generative linguistics as empirical science

¹ On another occasion L-S attributes to me, without the least justification, an entirely erroneous classification of acceptable sentences. In one of the long quotations from Coseriu in LS’s section 2.2., LS interpolates the claim that I would class the sentence *The five continents are four: Europe, Asia, and Africa* as unacceptable. There is not a shred of evidence for this incorrect claim. On the standard Chomskian notion of acceptability as discussed in section 3 of GEP, the sentence is acceptable, but contradictory, and nothing in GEP suggests any different interpretation.

The extent to which generative grammar can legitimately be seen as empirical science is another central concern of GEP, and another instance where LS erroneously summarizes the argument presented there. The problem comes in her statement (p. [28]) that ‘Riemer seems to accept that – as generative scholars are willing to state – even in this respect [the use of grammaticality intuitions] there is a coincidence between generative linguistics and Galilean sciences’. This is an alarming misinterpretation, going in the opposite direction to the entire argument of GEP. The passage from GEP which LS quotes (p. [28]) to justify her interpretation summarizes the standard generative view on intuitions as an idealization strategy comparable to experiment in natural science. This summary constitutes the statement of the coincidence between generative and scientific methodology which, far from *accepting*, GEP goes on to *argue against*: the absence of any process of deidealization in current generative methodology means that ‘[generative] theory is absolved in principle from the kinds of predictive consequences to which explanation in the physical sciences is typically held responsible’ (p. [17]). As if this isn’t unambiguous enough, the clearly stated conclusion of GEP (p. [18]) is that generative linguistics ‘should be Galilean. It should just balance the roles of idealization, observation and de-idealization in the same way that Galilean science does.’ Obviously, this implies that current generative practice, which lacks the deidealization component, is *not* Galilean science. This is an unambiguous conclusion.

On the related question of the bad star problem, GEP concludes that *if* this problem is as serious as some people claim, then this is another challenge to current generativism’s scientificity. GEP argues that if grammaticality assignments are generally unreliable, ‘their use conflicts with both the obvious theoretical goal of Minimalist syntax, and with its orientation as ‘Galilean’ science’ (abstract, p. [1]). The contrast between current generativism and empirical science is restated frequently. Thus, footnote 30 argues against the ‘coincidence’ between generative methodology and empirical science, by commenting that ‘[g]enerativists’ description of the elicitation of grammaticality judgements as experiments obscures a fundamental disanalogy with experiment in the physical sciences.’ The only thing that GEP concedes is that the idea that generative grammar, suitably modified, *could be* Galilean science, is not an ‘epistemological impossibility’, as LS believes (p. [31]). This is the only type of ‘coincidence’ between generativism and science that GEP endorses. That is very different, however, from the view that LS attributes to me, namely that current generativism *already* coincides with Galilean science. In claiming I hold this belief, LS has invented the exact opposite of the view clearly stated throughout GEP.

Do generativists want to ‘get rid’ of intuition?

In several other places, LS’s criticism of the argument in GEP is the result of her simply missing the point. In her conclusion, for instance, she denies the central claim of GEP, which is that ‘[t]he only problem with generativism’s use of intuition is the possible compromise it brings about to the theory’s predictive and explanatory power’ (GEP, p. [19]). Here is her criticism:

Hence “the only problem with generativism’s use of intuition is [not] the possible compromise it brings about to the theory’s predictive and explanatory power”, *but rather that both generative linguists and their critics insist on not realizing that we cannot ‘get rid of’ intuition in the field of autonomous (not necessarily exclusively generative) linguistics*, and that all claims about empiricalness in this domain have to be abandoned, in relation to both grammaticality and acceptability judgements. (LS, p. [30]; emphasis added)

But generativists would be the last to claim that intuition could be banished from linguistics! Despite the fact that, as discussed in GEP, *some* generativists are increasingly turning towards corpora for their data, this is not conceived of as a wholesale alternative to intuition-based data, which, as also noted in GEP, ‘remain the ‘preponderate data’ of generative research (Collins, 2007, p. 652)’ (GEP, p. [2]). No one in generativism is advocating ‘getting rid’ of intuitions: this would be a return to the behaviourism the theory rejects. Featherston (2007: 277), for instance, does ‘not wish to condemn introspective judgements as a data type. The position that we are adopting here is that judgements are indeed a fully valid way of making generalizations about syntax, but they must be used with more care and paid more attention to’. Again, Wasow and Arnold (2005: 1481) castigate generative grammar for its ‘*overreliance*’ on introspective data (emphasis added), a formulation that clearly excludes the possibility that introspection/intuition could have *no* role to play in syntactic theorizing. Wasow and Arnold and Featherston’s position is typical. LS has thus based her argument on a basic misconception about what the different sides of the generative data debate are arguing.

Theoretical intentionality and what generativism sets out to explain

Another basic error concerns the role of the concept of ‘theoretical intentionality’ in GEP. In section 5.3, GEP argues that scientific theories are intentional, in philosophers’ sense of this term: they have to be theories *of* something. As a result, they are responsible to empirical evidence: only if a theory is responsible to empirical evidence is there any possibility of checking its predictions against observation. This has important consequences for generativism’s attempt to claim the status of empirical science. Since there is no way of operationalizing the notion of grammaticality other than as acceptability, the more acceptable sentences the theory counts as grammatical, the better. If *all* of the sentences the theory counted as grammatical were not acceptable, there would be good reason to doubt that the theory is a theory of anything real. As it stands, any sentence which is acceptable, but which the theory counts as ungrammatical, requires further explanation: faced with an acceptable but ungrammatical sentence, a complete theory needs an account of what it is that makes the sentence acceptable, despite its ungrammaticality. LS sees a problem here: ‘The problem is that generative linguistics does not aim to explain the speakers’ whole communicative competence but only a part of it, namely linguistic competence –which Coseriu calls ‘idiomatic competence’ or ‘idiomatic knowledge’ (LS, p. [11]). But this fact isn’t a problem for my account, which acknowledges – indeed, *depends on* – the truism that generativism is not intended as a theory of *communicative* competence, performance or any other dimension of language except *linguistic* competence. This point is made explicit on p. [17] of GEP, where it is stated that ‘the force of the canonical distinction between competence and performance is that language use – performance – is not amenable to scientific investigation’; hence, performance cannot be anything that generativism sets out to study. There is, then, nothing for LS to fault here: she has missed the point of the broader argument about theoretical intentionality into which the passage she criticizes fits. The more grammaticality and acceptability diverge, the less powerful generative theory is as an explanation of a real object. The fact that generativism doesn’t set out to explain performance is thus not a ‘problem’ for the argument of GEP, but a necessary presupposition of it.

3. Arguing *vs* conceding

It seems clear to me that the main prospects for any sort of successful challenge to unsatisfactory aspects of the generative programme derive either from the development of

explanatorily satisfying paradigms elsewhere in linguistics which might provide an alternative model for generativists' own theorizing (systemic functional grammar; cognitive grammar; functional syntax; connectionist approaches; Culioli's 'Théorie des Opérations Énonciatives'; Coseriu); or from a careful demonstration that generative models are inadequate on their own terms. There is little to be gained from the mere confrontation between alternative models, given generativists' typical conviction that their own theories observe higher standards of justification than their competitors'. The history of controversy over generative linguistics shows, I believe, that in the absence of any fully worked out alternative syntactic models which accept enough of generativism's presuppositions to be considered genuine competitors to it, it is essential to confront generativism on its own turf. As noted above, generative linguists often feel entitled simply to ignore objections emanating from other theoretical orientations. This means that the best chance for stimulating any revision to generative practices comes in the first instance from challenging the theory from within. If one rejects the competence-performance distinction, does not believe that language needs to be treated as a distinct, modular cognitive faculty, with syntax as a special component within it, rejects the characterization of linguistics as an empirical science, and emphasises the social or discourse determinants of grammatical structure, then the fact that one also finds fault with the generative presentation of the grammaticality/acceptability contrast will surprise no one. In this situation, there are so many things to argue about that any one of them is easily neglected, and it is understandable that the generativist simply wants to agree to disagree.

Accordingly, the argumentative strategy adopted in GEP was to concede, for the sake of argument, as many generative presuppositions as possible. GEP mounts an argument against generative methodology from within the perspective of the theory. This is obviously consistent with the validity of critiques from other perspectives, a fact LS seems not to appreciate. Even though I was careful to emphasise at a number of points (e.g. p. [6], p. [9], [10]) that GEP makes as many concessions as possible to the generativist position, LS consistently fails to acknowledge this. Instead, she regularly assumes that I am actively arguing for, rather than just conceding, generative claims. For instance, she often conflates my own position with that of Chomsky, thereby presenting criticisms of the latter as criticisms of my own position. For example, she attributes [p.20] to me the characterization of intuitions as the 'voices of competence', even though this phrase is, in GEP (p. [3]) explicitly attributed to Michael Devitt, and introduced as a characterization of the generative position which provides the basis for the discussion. This instance is symptomatic.

The most obvious example of LS's failure to distinguish positions being argued for from positions being conceded for the sake of argument, comes in her discussion of my position in section 4. On p. [25], we find the statement that GEP 'claims [*sic*: 'calls for' would be a more accurate term] a de-idealization process in generative linguistics similar to the one that takes place in Galilean sciences'. LS uses this as basis for the charge that 'Riemer (incorrectly) makes no difference between idealization in the natural sciences and idealization in autonomous linguistics' (p. [25]), part of her broader claim about the necessarily non-empirical character of linguistics, which I discuss in section 6 of the present rejoinder. This is an unfortunate and unwarranted misinterpretation. Section 5 of GEP explicitly outlines an epistemology for linguistics, generativism included, in which its value does *not* depend on its scientificity. Since I argue there that 'there are, arguably, ways for the empirical study of language to be legitimate and at the same time fail to attain the same level of certainty as the experimental sciences' (p. [19]), LS's claim that I precisely fail to distinguish linguistics from

empirical science with respect to the central question of idealization is mysterious. (I defend a similar non-scientistic epistemology for linguistic semantics in Riemer 2005.)

The role of the discussion of deidealization in GEP is to show that a crucial part of the process of scientific inquiry *as generativists present it* is not to be found in generativist research. Generativists' justification of the role of intuition as an idealization strategy can only be enlisted in support of the Galilean character of their method if it is also accompanied, as were Galileo's own researches, by a process of deidealization – exactly what is missing in the generative research programme. This argument is valid whether or not linguistic methodology can legitimately be compared to that of the physical sciences. If linguistics is not anything like empirical science, as LS maintains, then generativists are *doubly* wrong. But even if it is, the rationale of the argument about deidealization in GEP is to show that generative research lacks what is usually understood as an important component of 'Galilean' method, and that generativists' own claims of scientificity are thereby undermined. There is nothing here to justify LS's statement that I fail to differentiate idealization in science and in 'autonomous linguistics' – or, at least, whether I fail to do so or not is entirely irrelevant. The discussion of deidealization merely accepts the presuppositions of the generative programme, and shows that its own practice does not follow through on its own claimed methodology. This argument is perfectly consistent with the possibility that, in fact, linguistic methodology can have nothing to do with the methodology of empirical science.

The relations between linguistics and science are the source of more than one confusion on LS's part. In the same section in which she criticizes GEP for not making 'the type of objection that is relevant in this context' (p. [26]), LS claims not to 'understand why he [Riemer] continues to talk about prediction in generative linguistics and does not deny the Chomskian approach its proposed Galilean science status' (p. [27]). This is a doubly perplexing admission. First, the whole burden of the argument in GEP, as already discussed, is precisely to deny current generativism the status of Galilean science. What GEP does *not* deny is the *possibility* that a science of language which would be entitled to claim the status of Galilean science *could* emerge from the generativist tradition. As a matter of fact, I find this unlikely, but GEP's policy of conceding as much to generativism as possible means that my personal reservations about this possibility are not relevant. What makes LS's position doubly hard to understand is that she herself then quotes the very sentence of GEP which states this strategy. LS claims not to understand why I do not 'deny the Chomskian approach its proposed Galilean science status, *even if he tries to justify this by saying that "in order to argue against an aspect of the methodology of the generative enterprise, it will be more effective to proceed in a way that respects as many of the programme's general aims and assumptions as possible"*' (p. [27], emphasis added). Frankly, I don't really know what to say here. In the first instance, LS criticizes me for not adopting a position – denial of scientificity to current generativism – which I do, in fact, clearly adopt. She then professes not to understand why I do not adopt this position – which, in fact, I do clearly adopt – in the same breath as quoting the very sentence which should resolve her misunderstanding. In the face of this, I feel rather like the farmer whose fields overflow with only the soundest organic wheat, but who has to undergo criticism for *refusing* to grow organic crops from an environmentalist who has the certificate *attesting to* the organic nature of the wheat in question in their pocket.

4. Is GEP incoherent? Is Chomsky?

If so, it mightn't be such a terrible thing. I would happily second an analogous claim about incoherence to this claim of Feyerabend's about incompetence:

I have no objection to incompetence but I do object when incompetence is accompanied by boredom and self-righteousness. (Feyerabend 1999: 185)

As it happens, however, GEP is no doubt self-righteous and boring, but it is not incoherent – or, at least, not in the ways LS claims. GEP argues that *if* there are as many bad stars as some critics believe, then this is a problem for the predictive character of generative models, and an obstacle to the scientificity which its proponents claim for it. LS says that this argument 'seems to make no sense' (LS, p. [5]):

if, as seems to be the case, there is a difference between acceptability and grammaticality, as criteria for judging the appropriateness of an utterance and the correctness of a sentence respectively, Riemer's complaint seems to make no sense. That is why he has to try to eliminate the difference between grammaticality and acceptability. (LS, p. [5])

As I have already emphasized, GEP does not advocate 'eliminating the difference' between grammaticality and acceptability. Leaving that aside, LS's reasoning is that, qua different notions, we should not *expect* grammaticality and acceptability to coincide. Notice, however, that her characterization of grammaticality and acceptability substitutes LS's *own* definitions (correctness and appropriateness, respectively) for the standard Chomskian ones. On the accepted definitions, for most investigators it is uncontroversial that acceptability (as revealed by intuitions) and grammaticality (as assigned to sentences by the theory) should coincide more often than not. GEP reports statements from within generativism to this effect from Horrocks (footnote 21), Cann (footnote 26), and Chomsky (p. [14]). It is certainly true that grammaticality and acceptability are different notions. But the force of GEP's argument about the operationalization of the former as the latter is precisely that there should be a coincidence between the two if generativism is to be predictively successful. LS's criticism of this argument only works if we substitute her own definitions for those which are actually in question. This is surely an unacceptable argumentative strategy: *any* argument can be revealed as flawed if alien definitions are foisted onto its principal terms.

LS also claims that GEP rests on a 'typical performative contradiction' (p. [29]):

Moreover, if Riemer can object that many sentences which appear in generative works as ungrammatical are acceptable in his opinion, it is precisely because he can rely on (his own) intuition in order to make this judgement. Otherwise, what other kind of evidence is there to support his claim of acceptability? (LS, pp. [3–4])

it is precisely on his own intuition that Riemer bases his lack of agreement about the ill-formedness of the examples he discussed. That is a typical performative contradiction. The only thing Riemer considers sure is his own intuition... (LS, p. [29])

First of all, let's note that GEP is perfectly clear on the fact that a clash between my own intuitions and those of generativists is in question: 'In all cases, my intuitions (as a native speaker of Australian English) do not reveal the sentences as ill-formed' (GEP, p. [6]). What this clash shows is that intuitions of well-formedness are not consistent among speakers of the same language. Where is the contradiction here? There would indeed be a contradiction

if I were either advocating the elimination of intuitions, or claiming, on introspective grounds, that the *my* intuition-derived grammaticality assignments, not the attested ones, were correct. In that case, I would be arbitrarily preferring my own intuitions over others', a particularly naive mistake. But it should be obvious that that is not what is being argued. GEP makes no claim about what the correct grammaticality status is for the sentences in question. It merely draws attention to a *difference* in different speakers' intuitions about the same sentences, and notes that this creates something of a dilemma for generativism. Intuition clash is a commonly acknowledged problem in generativism. There is no contradiction to speak of.

LS has something of a penchant for finding contradiction. It is also present, she claims, in Chomsky's own classic *Aspects* definitions of grammaticality and acceptability, quoted on p. [9] of GEP. Here is LS's diagnosis:

There are, however, two different concepts of 'acceptability' in these quotations. On the one hand, Chomsky is prepared to consider utterances that are simply comprehensible and not bizarre or outlandish as 'acceptable', even if he does not specify whether this would include ungrammatical utterances from non-native speakers which would be comprehensible to all native speakers (probably not). *On the other hand*, Chomsky specifies that 'acceptability' belongs to the study of performance and grammaticality to the study of competence. (LS, p. [9], emphasis added)

Whatever one may think of the Chomskian definitions, they are not contradictory, *pace* LS. There is one concept of acceptability, not two, at work in the quoted passages, and LS's 'on the other hand' is entirely unjustified. It is quite consistent that, as a performance-related notion, acceptability is related to immediate comprehensibility and absence of bizarreness. Comprehensibility and absence of bizarreness are, precisely, properties of *utterances* and thus belong to the domain of performance.

5. Coseriu, Itkonen and the prerequisites of theoretical progress

One of LS's main points is that the theoretical frame of reference for the questions in GEP needs immediate changing:

Riemer (2008) was only the starting point for the reflections contained in this paper, which was not conceived merely as a review of or reply to Riemer, but rather as a call to reflection by English-speaking linguists who are concerned with the topics of intuition and grammaticality/acceptability judgements. As I have attempted to show, no worthwhile progress in these subjects can be achieved by ignoring the relevant work of Itkonen and Coseriu, which has seen so little in-depth reception in the study of either linguistic competence or the epistemological status of grammatical accounts of linguistic structure. (LS, p. [31])

LS is certainly right to say that Coseriu and Itkonen do not usually figure in generative debates about the status of introspective data. Coseriu strikes me, impressionistically, as almost *never* referred to. As for Itkonen, I was unable to find any reference to him in such recent contributions as Sampson (2007), Featherstone (2007), Wasow and Arnold (2005), Fiengo (2003), or Gordon and Hendrick (1997). The only reference I have found, on an admittedly cursory examination of the literature, is Carr's (2003) reference to Itkonen (1978). LS's complaint on p. [2] of her reply that 'important contributions by Esa Itkonen' to the intuition debate are missing from GEP thus equally applies to most of the generative literature on the subject.

I am grateful to LS for having drawn my attention to Itkonen's work, which I will read with interest. However, her stipulation that Itkonen and Coseriu are *necessary* for any 'worthwhile progress' on these questions strikes me as unwarranted and unfortunate. Speaking for myself, it's not at all clear from what quarter the next bit of 'worthwhile progress' will come: it could come from *anywhere*. Erecting one's preferred theorists as the *sine qua non* of any further theorizing is a counterproductive and pointlessly dogmatic move, whose only effect would be to constrain research into questions which exist independent of any single author's formulation of them (except, of course, for the original definition by Chomsky of the terms *grammaticality* and *acceptability*, with which the debate still operates). It is not whether an article *refers* to Itkonen, Coseriu, or whoever, that matters. What are important are the *ideas* that are deployed in the process of inquiry, not the authors from which they come. Judging from LS's summary of Itkonen and Coseriu's ideas – and from that alone – I don't think I share her belief in their relevance to the present debate, whatever their no doubt considerable interest and importance from other points of view.

I will explain in the next section why LS's use of Itkonen does not strike me as pertinent. In the case of Coseriu, consider LS's comments in the following passage:

In addition to these considerations, that both Chomsky's and Riemer's accounts of the difference between grammaticality and acceptability involve a misconception can be seen if we look into Coseriu's differentiation between 'elocutional knowledge' (*saber elocucional*), 'linguistic knowledge' (*saber idiomático*) and 'expressive knowledge' (*saber expresivo*), to which so little attention (if any) has been paid in the English-speaking world (see Spence, 1960 for an exception). (LS, p. [11])

There is, however, nothing in LS's description of Coseriu's distinction which isn't able to be captured in a generative one, or in one compatible with generativism. LS approvingly quotes Coseriu's (1985: xxvi-xxvii) statement that problems of semantic incompatibility, logical contradiction and idiomaticity, as well as 'many others', 'can be solved or can at least be correctly formulated with the help of [Coseriu's] distinction' (quoted by LS, p. [13]). But none of these distinctions is unavailable to the generativist who hasn't read Coseriu. Similarly, the distinctions between '(1) language in general; (2) particular languages; and (3) language as individual discourse' (Coseriu 1985: xxviii, quoted by LS p. [14]), or that between 'the universal level, the historical level, that is, the level of languages of historically constituted communities, and the individual level: this or that fragment of language' (*ibid.*) is also hardly unavailable in generativism, which has simply chosen to make a different, though overlapping, set of distinctions.

Given the nature of the context, LS could not have been expected to discuss Coseriu's ideas in detail. Nevertheless, Coseriu's distinction between universal and particular features of language, as summarized by LS, is open to an obvious generative objection. As represented by LS, Coseriu frequently assumes that the very questions which generative linguistics studies have already been answered. LS reports Coseriu's criticism of the competence/performance distinction:

It was clear to Coseriu that the distinction between competence and performance (most recently redefined as the opposition between I-language and E-language) fails to distinguish and establish a level of analysis corresponding to the universal features of language, and thus regards them incorrectly "as the features common to all languages which may first be observed within the

grammar and the semantics of a particular language as belonging to its unitary system. In other words: the possible distinction between universal features which are independent of the structure of the various languages, and those features which can be observed in the structure of all known languages is neglected from the start” (Coseriu, 1985: xxvi). (LS, pp. 12–13)

LS does not tell us whether Coseriu has an independent account of ‘the universal features of language’, which would substantiate the argument that generativism fails to analyze it correctly. This is unfortunate, since everything depends on it. To Coseriu’s complaint, a generativist can legitimately reply that they have precisely a theory of the ‘universal features of language’, developed after concerted efforts, and that the onus rests on their critics to offer an equally or more compelling alternative. Without such an alternative, the generativist can simply reply that *they* have proposed the most complete account to date of what is universal in human language, and that Coseriu simply has no right to the phrase ‘the universal features of language’ in the absence of a more convincing account. If Coseriu *does* have an account, the generativist can say that he needs to *argue* for the superiority of his own account, not just assume it. In a similar vein, LS certainly has no right to the evaluation that generative syntax ‘incorrectly’ divides the particular from the universal, unless she can show an equally or more explanatory theory. There are other instances of this problem elsewhere in LS’s reply.

6. LS and Itkonen on intuitions and empirical science

The question of the implications of intuition-derived data for the epistemological status of generative linguistics is, clearly, the most important area of disagreement between LS and me. For LS, the argument of GEP is fundamentally undermined by a whole set of mistakes about the real character of linguistic research. The most basic of these is that linguistics is an empirical venture comparable in some ways to physical science. This is not the place to develop a full response to the many interesting questions LS raises. I will limit myself to defending GEP against LS’s criticisms, and briefly indicating why I do not accept that the perspective LS, following Itkonen, promotes, is the only possible one for discussion of these questions, as she claims.

Can linguistics be empirical? Does it make sense to speak of it as making predictions? Of course! — as long as this is not taken to exclude other ways of studying language. LS denies this. For her, no school of ‘autonomous linguistics’ (LS, p. [19]) is or could be empirical science: this is ‘not epistemologically possible’ (LS, p. [31]), an impossibility which applies to generative linguistics as much as to any other. As a result, it is inappropriate to describe generative theories using the same categories as those used for empirical science. This particularly affects the notion of prediction. For LS, prediction in the empirical sciences can only be of ‘spatio-temporal’ events: ‘predictions in the empirical sciences are and must be predictions of spatio-temporal occurrences’ (p. [21]). LS argues that linguistics, like logic, is a ‘non-empirical science’ (LS, p. [26]), oriented towards ‘non-factual evidence (i.e. evidence independent of time and space)’ (LS, p. [26]) obtained by means of intuition. For LS, autonomous linguistics, of which generativism is a part, simply does not make any predictions whatsoever. Since generative linguistics doesn’t make any predictions, there cannot be any threat to it from the bad star problem with intuition-derived grammaticality assignments, as GEP claims. ‘Better questionnaires’ (p. [19]), LS is confident, will help sharpen the inconsistent intuitions currently found in syntactic research. But the

development of these questionnaires (about which LS gives no details) seems not to be very urgent, since, as she summarizes the argument in her abstract,

even if a huge proportion of “(bad) starred sentences” (i.e. wrongly starred as ungrammatical) are judged to be grammatical by however many native speakers of a language, this proves nothing about the predictive failure of generative linguistics, since autonomous linguistics has (and cannot have) any predictive character at all. (LS, p. [1])

Unfortunately, this is another instance where LS disregards the argumentative tactics of GEP. As I have stressed a number of times already, GEP is prepared to concede the *possibility* that generative grammar is not completely on the wrong track, and that it is conceivable that it might, one day when suitably modified, give rise to a theory with the status of Galilean science. The main way for this to happen, however, is if it tries to make its ‘predictions’ more accurate, by aligning grammaticality-assignments with subjects’ acceptability judgements (or, perhaps, with other means of characterizing the well-formed or otherwise explanatorily basic sentences of a language). Generative linguistics thus does indeed, for GEP, have a ‘predictive character’, in the weak sense that its grammaticality assignments can be usefully compared against ‘observational’ data (intuitions of well-formedness). Conceding to generativists that empirical science is, at least, not an impossible status for their research is simply the device that gets the argument of GEP off the ground. For generativists, generative linguistics is a *natural* science (cf. the new generativist journal *Biolinguistics*), and it’s on that basis that I have couched the argument. (The fact that, personally, I don’t believe it, will be obvious to anyone who has read my (2005)).

LS, however, does not accept that any sort of linguistics could be empirical science, and finds fault both with generativism and with GEP’s critique of it accordingly. But the fact that one rejects a presupposition of a debate is no reason to deny the validity of an argument made by one of the sides in which this presupposition is conceded. LS, apparently, is only interested in criticisms of generativism which are compatible with her own theoretical outlook: according to LS, it would seem, for an argument to play a useful role in theoretical controversy it must not only be valid, it must also be *sound* (based on true premises). This requirement is far too strong: since the community of linguists does not agree about which ‘premises’ of linguistic argumentation are true (is there a separate language faculty? is grammar innate? are syntax and semantics separate? are semantics and pragmatics?), soundness is an impossibly high standard for theoretical debate in the discipline, and would preclude any sort of discussion between differing points of view. LS should acknowledge the value of critiques which confront generativism on its own ground — if, that is she wants any generativists to listen. As it is, she rejects the critique of generativism made in GEP on the grounds that *only* a stronger critique is appropriate. For the reasons I have outlined, this strikes me as counterproductive.

Quoting Itkonen, LS insists on two points: (a) science is concerned with ‘predictions of spatio-temporal occurrences’ (LS, p. [21]), and (b) linguistics does not study, or predict, spatio-temporal occurrences. These are deep waters of the philosophy of science, into which I’m reluctant to venture very far. However, both of LS’s positions seem, to say the least, questionable. With regard to (a), summary statements about the essential or definitional properties of science have long been avoided by philosophers of science, who are no longer inclined to appeal to any single criterion (falsifiability, predictive success, etc.) of scientificity.

From this point of view, which is regularly reflected in basic introductions to the topic like Bortolotti (2008) or Godfrey-Smith (2003), LS's stipulation about the necessary features of scientific research seems old fashioned (I make a similar point in connection to semantics in my 2005). In any case, it is simply mistaken to insist that, as a matter of fact, 'predictions in the empirical sciences are and must be predictions of spatio-temporal occurrences' (LS, pp. [20–21]): many empirical studies involve predictions of the *properties* of physical systems. It certainly seems to be true, as stressed for example by Lipton (2001), that predictive explanation often concerns *causes*, and thus has to do with sequences of occurrences. However, as Cummins emphasises (1989), much scientific explanation proceeds by way of *property-attribution*, not the tracking of causal sequences of occurrences, and there is nothing unempirical about it. As a result, prediction in science can well be prediction of properties. There is nothing unusual in describing genetics, for example, as 'predicting' that all humans have X chromosomes, Kepler's theory of planetary motion as 'predicting' that the planets have elliptical orbits, or embryology as 'predicting' various properties of fertilized cells (in virtue of which a step by step understanding of their development into full organisms can be achieved). These are predictions of properties, which do not have any necessary relation to predictions of occurrences. Scientists do, in fact, regularly speak in just these ways. Indeed, sometimes scientific investigation is precisely directed towards the discovery of properties, rather than sequences of causes. As Iain Giblin (p.c.) points out, science knows a great deal about the *causes* of various types of pain, but is much less clear on what pain actually *is*: here, a property theory is exactly what scientific explanation is aiming to achieve.

However this may be, does linguistics, as a matter of fact, study and make predictions about spatio-temporal occurrences (point (b))? LS answers with a categorical 'no'. To me, it seems less clear. If we understand linguistic ability as a disposition to produce certain linguistic structures in certain circumstances, then linguistics does indeed study 'spatio-temporal occurrences', at a certain level of abstraction: it studies *classes* of spatio-temporal occurrence. For instance, to study the syntactic structure of a language can be described as the study of what classes of utterances are well-formed in various circumstances. These circumstances can be thought of as defined semantically, i.e. in relation to their meanings, and it is this which anchors them in actual occurrences: meanings are the link between abstract structure and concrete use. For example, the 'prediction' that **Who is a book about being read by the class?* (Davies and Dubinsky 2003: 1) is ungrammatical, can be understood as the 'prediction' that, given the *desire* to express the *thought* 'What person is the class reading a book about?', speakers of English will not, all things being equal, *produce* the starred sentence, and hearers will not *understand* it readily. The reference to the italicized psychological facts about speakers makes the prediction's connection to spatio-temporal occurrences obvious. This is not, of course, an interpretation that would be endorsed by orthodox generativists, for whom considerations of performance are irrelevant. But it is a possible interpretation, and one which many other linguists would accept. Certainly, many linguists see themselves as participating in a project which will eventually be able to be related to brain science, and which, as a result, is obviously about people's actual linguistic behaviour in actual circumstances. This is, of course, a variety of 'spatio-temporal occurrence'.

In my opinion, this is just as well. If linguistics wasn't, or couldn't be, in some way concerned with things that happen in space and time, there would be much less interest in it. Why does one engage in linguistic research? There are, no doubt, many valid reasons. For

many researchers, however, one important reason – though not the only one – is to understand something about the world: people’s ability to use language. There may be many other interesting and valuable intellectual projects which take language, in some sense, as a starting point. But there is clearly a difference between a project which sets out to understand why the utterances people utter are as they are, and one which aims to study people’s beliefs or intuitions about which utterances are ‘correct’. Both studies are interesting, but only the first, I believe, tells us about people’s actual linguistic abilities, and the reason it does so is that it concerns the utterances that do in fact occur (in space and time).

This orientation has implications for the way this kind of linguistic research should proceed. Since this kind of linguistics is interested in people’s ability to use language, it must characterize this ability correctly. In particular, it must describe our ability to produce the utterances we *do actually produce*, not just the utterances we are prepared to count as ‘correct’. What we want an account of is the ability we do actually have, not the ability we think we have, or the one we would like to have. As a result, we have to study the utterances that occur, whether or not these are in line with our intuitions. For this reason, I cannot agree with the statement LS quotes from Itkonen:

... there is an indefinite number of **incorrect** sentences which have been or will be uttered, i.e. which **must not** be accounted for by the grammar (in spite of the fact that corresponding utterances have occurred or will occur in space and time. Therefore space and time are irrelevant to grammatical analysis... (Itkonen, 2004 [*sic*], chap. 4, quoted by LS p. [22]; emphasis original)

‘Incorrect’ utterances that have or will be uttered are precisely among what a theory of language (one may not want to call it a ‘grammar’) *must* account for, if it is to be informative about human linguistic ability. Intuitions of meaning and well-formedness serve to specify the meanings and, derivatively, certain structural features of utterances. It is intuitions which supply the background ‘theory’ which make observations of linguistic structure in actual utterances meaningful. But the data which intuitions interpret must be obtained observationally, and intuitions cannot function as a substitute for this data itself.

This isn’t to say that ‘actual utterances’ are the *only* interesting thing to study about language. The project LS refers to, an exploration of linguistic intuitions, may well be rich and intellectually worthwhile; certainly the onus should be on anyone who claims it is not. But this project, just like any other, has no right to claim any monopoly over linguists’ legitimate activities, as LS apparently wants. An analogy here may be helpful. Moral behaviour may be studied from two points of view. Either we can study what decisions people actually make in different situations, or we can study people’s intuitions about what the morally appropriate choices in these situations are. Clearly, these two studies are separable. Of course, in order to understand people’s moral behaviour in actual situations, we will presumably have to appeal to their – and our – ‘intuitions’ about the characterization of the choices with which they are confronted. To explain why someone is opposed to stem-cell research, or to abortion, for example, it will be useful to know what their intuitions are about whether stem cells or foetuses are ‘life’, or ‘human beings’, or whatever, as well as to their intuitions about a host of other questions. But, clearly, the study of these intuitions is no substitute for the study of what people actually do, and we will not get anywhere in understanding people’s *actual*

behaviour if we *confine* ourselves to their intuitions about moral situations, *ignoring* the moral behaviour which they actually demonstrate. The same is true in the case of language.

For LS, however, the study of language is not concerned with utterances, and linguistics is not an empirical undertaking. What is it, then? Following Itkonen, LS argues that, as a human science and not a natural one, ‘autonomous linguistics’ is concerned not with empirical facts, but with normative ones of rationality and correctness. But this is exactly the perspective which both GEP, and many generativists, would refuse. Linguistics needs to be concerned with the linguistic structures people do produce, not with the ones we think it rational or, even less, correct, to produce. Intuitions are indispensable as a way of ordering the observations we make of utterances; but they do not make these observations redundant. This is, perhaps, why LS finds that GEP does not pay enough attention to issues of normativity. She complains that I do not seem to be conscious of the normative dimension of intuitive judgements (p. [25]), and that I ‘misconceive the real character of this kind of linguistic idealization’. Here is the full passage containing the latter claim:

However, I think Riemer misconceives the real character of this kind of linguistic idealization when he says that idealizations in generative grammar consist in *abstraction from real performance data*. If this were the case, it would be possible to gradually ‘ascend’ from factual or statistical description of performance to structural description of the language system mastered by the linguistic competence of the native speaker. Chomsky has explicitly denied this possibility... (LS, p. [24])

This is a dismaying misrepresentation of the argument of GEP. I do not argue that idealizations in generative grammar ‘consist’ in ‘abstraction’ from real performance data in the sense that LS claims. Indeed, LS has put words in my mouth: nowhere in GEP is idealization said to ‘consist’ in abstractions from performance data. LS seems to think that GEP argues that the sentences predicted as grammatical by generative theories are somehow arrived at through a process of *statistical* abstraction or induction from actual utterances. On this picture, the investigator would start with real performance data, and gradually refine them in order to reach the predicted well-formed sentences, much as Michelangelo chips away the imperfections of the surrounding marble block to ‘reveal’ the statue of David hidden within. If I made it, this claim would imply exactly the Chomskian objection which LS endorses: that one cannot ‘ascend’ from real performance data to the sets of well-formed sentences which the theory takes as grammatical. One only needs the most passing acquaintance with the standard issues of the philosophy of generative grammar to realize that the introspectively grounded grammaticality assignments of generative methodology are obviously not ‘abstracted from real performance data’ in this sense of ‘abstraction’: they are designed to make abstraction of this kind unnecessary. It is only because, Chomsky argues, one *cannot* proceed in this way that some other method, like introspective judgements of well-formedness, is necessary.

Instead of arguing on the basis of the plainly false views LS attributes to me, let’s look instead at what GEP actually argues:

By consulting speakers’ intuitions about what is and is not a well-formed string, the generative syntactician can effect the necessary abstraction from real performance data, idealizing the facts to be explained in a way which offers some hope of theoretical tractability. Intuitions supply the investigator with a ‘readily accessible and informative’ (Chomsky, 1986, p. 36) class

of data from which irrelevant interference has been removed, and which can consequently reveal the properties of I-language. (GEP, p. [3])

Any reasonable reader, I believe, would interpret this passage as saying what it says — namely, that intuitions ‘effect’ an abstraction *in the sense of an idealization from performance data*, not that they *consist in* an abstraction *in the sense of a statistical approximation of performance data*. In case this seems hair-splitting, it is just as well to develop the implications of the two different phrasings. LS proceeds as though GEP claims that grammaticality assignments ‘consist in’ abstractions from performance data, and that the predicted well-formed strings are an abstraction in the sense of a ‘statistical approximation’ of the actual strings attested in real utterances. But this is not what GEP argues at all. What GEP claims is that introspectively derived grammaticality assignments *effect* an abstraction, i.e. that such assignments *play the role* of an abstraction, in the sense of an idealization, from performance data. This is a perfectly standard interpretation of the role of intuitions in generativist literature, supported by the quotation from Chomsky (1986), among others. To effect an abstraction in the sense of an idealization, via intuitions, is clearly not the same thing as to consist in an abstraction in the sense of a statistical approximation. GEP argues the former, LS interprets it as arguing the latter. Once again, an analogy may be helpful in pinpointing LS’s mistake. The sentences which generative theory predicts to be well-formed effect an ‘abstraction’ from real performance data in a similar way to that in which the abstract of an article ‘effects an abstraction’ of that article’s words. It is clear, of course, that this summary can ‘effect’ the abstraction without containing any ‘statistical approximation’ of the actual words used in the article: the abstract may convey the argument in a completely different way.

7. Conclusion

There are a number of other aspects of LS’s reply which merit discussion, but which I have not had time for here. In particular, I have had very little to say about her use of ‘rationality’ and ‘correctness’ as substitutes for acceptability and grammaticality respectively, in spite of the rich array of questions this choice opens up. Further discussion will have to be postponed for now.

It is always quite a shock to discover that anyone has actually read one’s work, and I am grateful to LS for the attention she has devoted to GEP. I have been rather critical, in this rejoinder, of her arguments, but this is purely a result of my concern to show that her claims of problems in GEP are unfounded. This demonstration should not, however, obscure the very real interest of the theoretical ideas on which LS’s article relies.

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