Muriel Norde & Freek Van de Velde, eds. *Exaptation and Language Change*. [Current Issues in Linguistic Theory 336] Amsterdam: John Benjamins, 2016. viii, 411 pp. doi:10.1075/cilt.336

Reviewed by Roger Lass (University of Edinburgh)

1. Background¹

The term 'exaptation' was first introduced into historical linguistics in a paper of mine called "How to do things with junk: Exaptation in language evolution" (1990). The title was a slightly disingenuous allusion to J.L. Austin's famous linguistic philosophy paper (1962), as well as to an important and at the time puzzling phenomenon in genetics. The term 'junk' (chosen at least partly for catchiness and to generate controversy) turned out even more controversial than I expected, and its denotatum is argued by many of the authors in this collection not to exist. It was however at that time (and indeed still is, but in a somewhat different way) a standard term in genetics. It is rather odd to review a book of essays on a concept that I introduced into historical linguistics over a quarter of a century ago and substantially revised seven years later (Lass 1997: §6.4), not only because so much excellent work has been done on the subject (including workshops at conferences in 2011 and 2012, from which the book under review partly originates), but because the biological framework underwriting it has become much more sophisticated than it was then. Being caught between reviewing the book qua both book and an exegesis and criticism of my own work is going to make this rather more self-referential than your typical review.

There were two distinct threads in my attempt to introduce the concept of exaptation into historical linguistics: (a) the idea, already established in evolutionary biology by Gould & Vrba (1982), that structures evolved for one use can be co-opted for another and that this is novel and necessary; (b) my claim that, in language change if not so much in biology, at least some structures that are exapted are not directly refunctionalised; rather they survive with null function as 'junk', parts of the language *in posse* until they are exapted; and that the key

^{1.} I am grateful to Roly Sussex and Margaret Laing for helpful comments.

idea in linguistic exaptation is that such junk plays a role in change. The term 'junk DNA',² and more importantly the notion that complex systems could contain useless elements, had already been standard in genetics for at least a decade by the time Gould & Vrba's paper appeared. It referred to the vast amount of DNA in almost every genome examined which did not appear to do what DNA was canonically supposed to – code for protein (by current estimates only 2% of the 6 billion base-pairs in the human genome does this). It also turned out to be the most controversial part of my revision.

There is a 'Central Dogma' in genetics (apparently invented by Francis Crick): DNA makes RNA makes protein. It was also recognised early that some DNA did not code but was functional, e.g., as 'punctuation', like the stop codons that mark the ends of genes. Proper junk DNA is typically in the form of moveable elements (transposons) that 'jump' from place to place through the genome but do not code; remnants of the DNA of historical pathogens (the human genome is full of inactive viral and bacterial DNA); apparently meaningless, often lengthy repeats of nucleotides;³ and pseudogenes, structures very similar to functioning genes and clearly with common ancestors but which are not expressed. These were also called 'selfish DNA', because it seemed that the only purpose of transposons and similar non-coding DNA strings was to reproduce themselves. But it also became clear that much of this apparent junk DNA was highly conserved (i.e., very similar in organisms as far apart as insects and humans, which suggests great antiquity).

So originally the term was used disparagingly for DNA that did not code for protein (via coding for amino acids) and seemed to be functionless. Eventually, especially during the work leading to the sequencing of the human genome, it became clear that a great deal of apparently non-coding DNA has other functions: regulation of gene expression, control of function rate within the organism – and above all making RNAs of different sizes and functions, which themselves largely run the engine built by DNA.⁴

It was also clear by the 1970s that genes are discontinuous. When RNA makes amino acids, sequences of the gene are edited out and not made part of

^{2.} This was invented apparently by Ohno (1972).

^{3.} These repeats are actually often functional, if not in a desirable way, e.g., the gene for the fatal Huntington disease normally has 6–29 CAG triplets (coding for glutamine). Above this number the chance of the disease is greater, and patients with the disease have 36 to hundreds of repeats. This repetition is due to a specific junk producing mutation; see Budworth & Murray (2014). These STRs (short tandem repeats) are also one of the primary discriminators in DNA 'genetic fingerprinting'.

^{4.} See Carey (2012). This is a semi-popular but very professional account of recent developments in the study of the function of RNA and other non-coding but vital parts of the genome.

the subsequent protein (introns), while other segments that code for parts of the amino acid chain remain (exons).⁵ In the process of making an amino acid which is later to become one of the elements of a protein, the DNA double helix unzips at the locus of the gene, and a similar but distinct nucleotide RNA (ribonucleic acid) is made. This forms a strand complementary to the relevant one already there through unzipping. When the new double strand is formed, other RNAs gather together the nucleotides making up the gene, and, to simplify, the introns at this point are edited out (splicing), and only the RNA carrying the exons exits into the cytoplasm of the cell to a protein-and-RNA assemblage called the ribosome, which 'reads' all the edited DNA to produce strings of amino acids. These are assembled into functional proteins (enzymes, structural protein like muscle, etc.). It would seem that introns are the quintessential junk DNA; but in fact they produce enormous numbers of different RNAs with important control and regulatory functions. But even with the discovery of function in non-coding DNA, a good deal of undeniable junk remains, and the term is still used freely.⁶

This in short is the biological background for the notion of junk in an otherwise functional system. So the story in genetics has been finding ever more vital functions for much of what had previously been thought of as junk, though the idea of junk is still important and empirically well supported. Clearly there has been no such trajectory in historical linguistics; what proved to be controversial was the idea of junk itself, which was fiercely attacked as early as five years after publication (Vincent 1995). Now to the book.

2. The main issues

The essays in this collection are rich and complex, but they deal overall primarily with four major conceptual questions:

a. Is the attempt I had begun in 1990 to transfer concepts and terminology from biology to linguistics valid or useful? In other words, is a transfer of terms or concepts across disciplinary boundaries either a category error, in this case apparently (but not intentionally) a lapse into a kind of Schleicherian organicism,⁷ or simply a feeble and contentless metaphor?

^{5.} See Berget et al. (1977).

^{6.} For a very clear and accessible introduction to the technical genetic concepts, see Clancy (2008).

The construal of languages in one way or another as natural organisms: Schleicher (1859, 1863). See the elegant discussion in Morpurgo Davies (1998: chs. 4, 8, especially \$8.3).

- b. Is there such a thing as linguistic junk, or is that claim too a category error?
- c. Is exaptation an independent concept or just another kind of refunctionalisation?
- d. Assuming that the idea is valid, can it be extended beyond morphology, which is where all my examples came from, as is done by a number of the papers in this collection, to syntax and phonology?

The greatest virtue of this book is its critical argumentation: it gives both positive and negative answers to these questions and extends the conceptual range that might be treated under the heading of exaptation (for the negative see especially Joseph, Gaeta, this volume).

My main claim (Lass 1990:81–82) was that languages may contain junk elements, which can be recycled or exapted for other purposes, and that junk could figure in major processes. I later repaired an important conceptual error. That is, that processes that look very like (and that I would call) exaptation do not presuppose prior loss of function, as such processes do not in the standard examples in biology: my main instance was the use of the non-junk 'lative' morphemes *-s-, *-l- and locative or lative *-t-, *-k in constructing the Finnish case system. There the exapted material was functional and simply fitted into the already existing agglutinative template. There was also a tendency in that chapter to confuse exaptation and *bricolage*, or at least to see them as a single natural kind, which is probably not the case; but certainly exaptation may result from *bricolage*.

3. Cross-disciplinary conceptual transfer

Does conceptual or terminological transfer fail because it mistakenly implies ontological transfer? In fact, what is transferred in the case of using 'exaptation' in historical linguistics while it was developed for historical biology is solely the Darwinian MODEL, not its ontology. That is, the use of the term and concept presupposes a category 'Darwinian History', a process of

failed replication ('mutation') of an item in the system (base pair, chromosome, phoneme, morpheme ...) > variation between failure and original > gradual selection of the failed replicandum > (usually) 'fixation' of the mutated version in the relevant population of objects.⁸

(The term 'replication' of course derives from Dawkins 1989 and his subsequent usage.) This in fact summarises one aspect of what seems to me properly conducted variationist historical linguistics (see §4 below).

^{8.} See CoNE Introduction, Lass (1993), Lass & Laing (forthcoming) for this model.

This issue is discussed in Van de Velde & Norde (pp. 7–9) and in detail in von Mengden (this volume, *passim*). While they urge caution in such transfer, they end up with a measured conclusion (8), that if not a direct approval of the programme suggested above nevertheless sees it as potentially useful:

In our view ... using terms from an established field of evolutionary biology (or rather evolutionary theory), like exaptation, should not be dismissed out of hand as a mere fad, as a way to let linguistics dissolve in biology. Rather it may enhance cross-discipline communication with a general evolutionary theory of superordinate structure, of which biology and linguistics are both instantiations without one taking precedence over the other

I think that 'historical sciences' exist and have their own modes of working, and that this variation/selection model could apply for instance to art history or music history as well. It is certainly the case that cladistic methodology has already been successfully transferred from biological phylogenetics to historical linguistics (see McMahon & McMahon 2013).

4. Linguistic junk

The idea of linguistic junk was condemned on the grounds that languages are semiotic systems so there cannot be signs that do not signify (the 'integrity of the sign' which many linguists seem to consider a property of the world rather than a potentially falsifiable empirical claim – see, much earlier than this collection, Vincent 1995). Another criticism is the stronger and apparently logic-based claim that a junk morpheme is a *contradictio in terminis* (Norde & Trousdale, p. 164). I think this is not the case. Not every object in a linguistic system has to be a sign, and there are situations in which stable morphs are not underwritten by any semantic content.

One solid example of a junk morph (Norde & Trousdale are of course right to condemn the notion of a junk morpheme) is the development of the Germanic prefix * γa – (collective, intensive, perfective = Continental West Germanic past participial *ge*-) in early English. Aside from its optional participial use, this prefix (at first usually spelled *ge*- = [jə]) apparently had a number of senses in Old English, still controversial (see OED s.v. y-, *prefix*). There it appears to act sometimes as an aspectual discriminator, often marking perfective (as it probably does when used as a participial suffix). The first steps toward junk status were taken in Old English, as there are many forms which can appear with or without the prefix with no semantic difference, e.g., *(ge-)genga* 'fellow traveller' *(ge-)genian* 'meet'.

In Middle English such possibilities become increasingly rare, but the prefix persists, (outside of the North, where it vanished early), usually in the forms *i*-, *y*-,

mainly as a past participle marker. It is robust in this function and persisted in the Midlands and South as late as Chaucer in the late 14th century (though his compatriot Gower does not use it). But it had pretty well vanished by the beginning of the 16th century. It was revived in verse *metri causa* by Spenser and other archaising poets at the end of the century, and continued into the 17th century in forms like Milton's *yclept* 'called'. All the historical material treated here is of course written, but, since the work of McIntosh (1956) and the following tradition, it is no longer possible to consider written language as mere 'representation' of or parasitic on spoken; it is as 'linguistic' as speech. And attestations in verse are particularly 'linguistic', as the very notion *metri causa* presupposes anchorage in the spoken language.

Another example I take seriously, but that does not seem to have achieved much success, is the coupling of case-syncretism with junk-formation leading to exaptation. Take for instance what happened to the aspect system of one type of Indo-European verb in Germanic. This is the kind that has the ablaut pattern present *e*-grade, perfect sG *o*-grade. One thing that may have caused some abreaction is that I never made clear (and it was not clear to me then) that syncretisms must be interpreted as built on variation. That is, at least two categories, one of which is to be lost, must coexist to avoid 'catastrophic' change. I was not clearly aware in the earlier formulations that in fact a genuinely variationist account of linguistic history must make provision for junk. Thus in Germanic strong verbs the Indo-European perfect took over the new function of preterite, and there was a syncretism in which the aorist was lost. This would seem to me to be a simple case of exaptation in the original sense, in which one of two categories is evacuated of meaning, and disappears, while the remaining one takes on its meaning and becomes an aspect-free past tense. And conceptual innovation, which some writers here find difficult, would seem to be exactly what happened: the aspectually complex IE verb system was reduced to two tense-coding members, and the result was an aspect-free system (essentially present vs. aorist, a better term than preterite or past) where what was once an aspect marker came to mark tense. The aorist was evacuated of meaning, but this must have been a variable process over time, and during this period, assuming that catastrophic deletion of morphology doesn't occur, the perfect would have been junk.

Overall the idea of linguistic junk seems to have been the most controversial and, I gather from the tone of a number of the essays here, irritating aspects of the package I introduced in 1990. As Narrog (p. 97) points out, the idea has "met with general disapproval". It seems to me now, rethinking the topic and reading this collection, that while exaptation *sensu stricto* does occur, it is probably rare. And by portraying it in a rather naive catastrophic/Neogrammarian way and not building it into a variationist theory, I was clearly wrong. But it has served, partly through its annoyance value, as a trigger for a great amount of very fine research, which this volume shows.

5. Is exaptation a stand-alone concept?

This seems to be one of the most difficult and interesting questions raised by these authors. I took my cue from Gould & Vrba, who referred to exaptation in their title (1982: 4) as "a missing term in the science of form". I thought I was doing more or less the same thing, but with an addition: the notion of refunctionalisation or whatever term you want to use for functionless or defunct material being co-opted.

As the editors point out (pp. 10–11), there are many terms denoting at least phenomena very like exaptation, all with different nuances, starting from Jespersen's 'secretion' (1928). Among the others are regrammaticalisation, degrammaticalisation, hypoanalysis, refunctionalisation and functional renewal (for references see the cited pages and their discussion in §4). Here they sharply anatomise my argumentation and conclude, citing an enormous amount of literature including the collected essays, that the original necessary properties of exaptation in my treatments have been oversimplified and that exaptation is at least a much more complex phenomenon than I had originally suggested.

They discuss thoroughly the properties I had originally assigned to exaptation, and in each case they show the extent to which I may have simplified or gone wrong. One of the properties subjected to critical discussion is 'unexpectedness of the new function', which is a slight caricature of my original suggestion, since one of the primary examples I used in 1990 and returned to in 1997 was the merger of aorist in perfect, which is not 'unexpected', though it is more often the aorist that vanishes and is replaced by the analytic perfect, at least in West Germanic (some South German dialects, Afrikaans, Yiddish). But the process is still the same and I would say counts as genuine exaptation in my sense. They also discuss other properties like 'novelty' and 'junk status'. They conclude on the basis of the collected essays that, "Ever since its introduction, first in biology, and later in linguistics, exaptation has been a controversial notion, and many of the controversial issues surrounding that notion still haunt both fields after several decades of research" (§5, p. 27).

6. Exaptation beyond morphology

One thing this book makes clear, and it is perhaps its most significant contribution, is that, whatever has been made (or not) of my original notion of exaptation, there is a very important contribution to language structure beyond inflectional morphology, which is where all my examples came from, and where exaptation-like phenomena occur. The editors have a detailed discussion (§3.3) and treat a number of examples not referred to in the papers here, and they are generally skeptical or at

least reserve final judgment. Some of their examples, as they say, may not be true exaptation, such as the apparent defunctionalisation of the Dutch negative clitic *en* and then its later occurrence as a subordination marker with positive polarity (p. 12). This as they say may not be true exaptation but as in this case where the *en* occurs in clauses with another subordinator, we may rather have simple refunctionalisation leading to multifunctionality (or 'degeneracy'). But see also the discussion of the exaptation of the *was/were* distinction in the standard English dialects in Willis (pp. 213–214).

One of the major difficulties is that, given the relative rarity of 'true' exaptation (in my original sense), there are a number of change types that show some similarity, so that there is no agreement on what its defining properties are, and whether it is a type of degrammaticalisation, and what its relation might be to the frequently discussed notion of linguistic adaptation (Narrog, von Mengden, Norde & Trousdale, all this volume). The instances of exaptation in anything like the sense it was originally proposed are vanishingly rare. As von Mengden (pp. 121–122) remarks,

> Once a technical term is established ... it begins to live a life on its own, independent of the original intention behind it. A technical term can be enriched by additional meanings and connotations, in principle every time it is used, just as happens with expressions for everyday concepts. This is irrespective of whether a term was first introduced with the intention to promote a new concept, or whether it was just coined in passing.

This is not a bad thing; in transferring the term from biology, I had no essentialist agenda.

One intriguing innovation that sets exaptation in a wider conceptual framework and at the same time narrows its denotative range is that of Gaeta (this volume, 87), built on a duality of adaptation and exaptation:

Adaptive changes, i.e. changes matching the three requirements of social value, complexity and distinctiveness ... are essentially oriented ('vertical') and originate from widely attested processes of variations corresponding to a general design of economy and plasticity. In contrast exaptive changes are normally non-oriented ('horizontal') and result from manipulation of features already existing in the speech signal which are subsequently refunctionalized to serve a different purpose.

While I find the vertical/horizontal distinction unclear, and do not think 'economy' is a useful historical concept (see Lass 1980, *passim*), I find this, like most of the other papers in the book, clear and intelligent enough to be worth arguing with, and that is something to be desired.

Every paper here has something of real interest to say, and there are many sub-themes that arise repeatedly, especially the relation (if any) between exaptation and grammaticalisation and whether exaptation can be considered 'secondary grammaticalisation' (Norde & Trousdale, von Mengden and others, this volume). Discussions of morphology, both inflectional and derivational, constantly recur along with discussions of grammaticalisation and related phenomena. There are some very interesting treatments of exaptation in syntax, especially the construction grammar paper by Norde & Trousdale. There are some discussions of possible exaptation in phonology, but no full-length treatments. The only one I know is McCully (2002), which deals with possible exaptation (or at least major refunctionalisation) in the history of the English stress system. As the editors remark (Van der Velde & Norde, p. 29), even readers "reluctant to accept a new term in linguistics" will find that this collection "has a lot to offer, as the plethora of changes that the authors present are often difficult to account for in well-known types of change like grammaticalization, and lay bare the intriguing dynamics of linguistic change". I would agree whole-heartedly with this assessment.

References

- Austin, J. L. 1962. *How to do things with words: The William James Lectures delivered at Harvard University in 1955.* Oxford: Clarendon Press.
- Berget, Susan M., Claire Moore, Phillip A. Sharp. 1977. Spliced segments at the 5' terminus of adenovirus 2 late mRNA. *Proceedings of the National Academy of Science U.S.A.* (74). 3171–3175. doi:10.1073/pnas.74.8.3171
- Budworth, Helen & Cynthia T. Murray. 2014. A brief history of triplet repeat diseases. HHS public access author manuscript. http://www.ncbi.nim.nih.gov/PMC3913379 (accessed October 12, 2016).
- Carey, Nessa. 2012. The epigenetics revolution: How modern biology is rewriting our understanding of genetics, disease and inheritance. London: Icon.
- Clancy, Suzanne. 2008. DNA transcription | Learn science at Scitable Nature. http://www.nature.com/scitable/topicpage/dna-transcription-426 (accessed October 18, 2016).
- Dawkins, Richard. 1989. The selfish gene, 2nd edn. New York: Oxford University Press.
- Gould, Stephen J. & Elizabeth Vrba. 1982. Exaptation: A missing term in the science of form. *Paleobiology* 8. 14–15.
- Jespersen, Otto. 1928. Language: Its nature, origin and development. London: George Allen and Unwin.
- Lass, Roger. 1980. On explaining language change. Cambridge: Cambridge University Press.
- Lass, Roger. 1990. How to do things with junk: Exaptation in language evolution. *Journal of Linguistics* 26. 79–102. doi:10.1017/S0022226700014432
- Lass, Roger. 1993. How real(ist) are reconstructions? In Charles Jones (ed.), *Historical linguistics: Problems and perspectives*. London: Longman.

- Lass, Roger. 1997. *Historical linguistics and language change*. Cambridge: Cambridge University Press. doi:10.1017/CBO9780511620928
- Lass, Roger & Margaret Laing. Forthcoming. Q is for what, when, where?: The 'q' spellings for OE *hw-. Folia Linguistica Historica*.
- McCully, Chris. 2002. Exaptation and English stress. *Language Sciences* 24(3). 323–344. doi:10.1016/S0388-0001(01)00036-5
- McIntosh, Angus. 1956. The analysis of written Middle English. *Transactions of the Philological* Society 55(1). 26–55. doi:10.1111/j.1467-968X.1956.tb00563.x
- McMahon, April & Robert McMahon. 2013. *Evolutionary linguistics*. Cambridge: Cambridge University Press.
- Morpurgo Davies, Anna. 1998. *Nineteenth-century linguistics*. In Giulio Lepschy (ed.), *History of Linguistics*. Volume 4. London: Longmans.
- Ohno, Susumu. 1972. So much 'junk DNA' in our genome. *Evolutionary Genetic Systems: Studies in Biology* 23. 366–370.
- Schleicher, August. 1859. Morphologie der Sprache. *Mémoires de l'Académie des Sciences de st-Pétersbourg* 1. 1–39.
- Schleicher, August. 1863. Die Darwinsche Theorie und die Sprachwissenschaft: Offenes Sendungsschreiben an Herrn Dr Erst Häckel. Weimar: H. Böhlau.
- Vincent, Nigel. 1995. Exaptation and grammaticalization. In Henning Andersen (ed.), Historical Linguistics 1993: Selected Papers from the 11th International Conference on Historical Linguistics, Los Angeles, 16–20 August 1993 (Amsterdam Studies in the Theory and History of Linguistic Science, Series IV: Current Issues in Linguistic Theory 124), 433–445. Amsterdam: John Benjamins. doi:10.1075/cilt.124.34vin

Reviewer's address

Roger Lass Linguistics and English Language Dugald Stewart Building, 3 Charles Street EDINBURGH, EH8 9AD United Kingdom *lass@iafrica.com*