Genetic Metaphor in Historical Linguistics

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1. Metaphor and Knowledge

We talk in metaphor, usually without knowing it. We grasp ideas, see the point of an argument, our past is behind us, our future ahead. In scientific disciplines too our speech is loaded with metaphor. Sometimes it is obvious and designed: evolutionary arms races, selfish genes, the Red Queen scenario, hawks and doves. Often it is not so much designed as virtually natural growing out of the way we perceive the processes or objects we describe. Metaphoric usage may be nearly coercive: macrophages roam through the body looking for foreign objects to devour; other immune cells take in antigens, snip out peptides, present them on their surface so that others can recognize them, cells are recruited for immune defence, neoplasms are detected by immune surveillance, DNA strands are unzipped.

This is merely obvious, though it is worth noting if only as a reminder of how saturated our most 'objective' disciplines can be with the poetry that underpins ordinary human speech and thought. Many people mistakenly think that metaphors are 'mere', not to be taken seriously, just 'figures of speech'. On the contrary, metaphor is often a potent, field-defining device; a good metaphor can open new mental spaces, suggest heuristics, and even do really useful (virtually) empirical work. So de Preez (1991:69) says: 'a metaphor is a conjecture about the nature of reality; a significant metaphor is an insight into unexpected properties of the object of an inquiry'. Metaphors delimit conceptual spaces, particularly in that a higher-order metaphor generates what has been called an 'M-language' (Paton 1992): the metaphor itself calls up natural associations, which can be used to flesh out its applications. Consider for instance the evolutionary arms race (parasite/host or predator/prey). Once the M-domain 'Military Competition' is invoked, associations arise, like outflanking the enemy, getting round its defences, recruiting soldiers to the battle, etc. (For discussion and references see Lass 1997:§§1.6-7)

The use of metaphor in supposedly empirical disciplines however can lead to two classes of epistemological problems:

(a) Domain-internal: how 'real' is a given metaphor, and what might happen to you if you started believing it?

(b) Domain-external (my main concern here): What happens when metaphors that look really good in one field get transferred to another, as they so often do?

There is a certain danger that good heuristic or other metaphors that seem in one field to have a solid physical or otherwise well-defined substrate can be hijacked (another metaphor of course) by others; and such hijacking can induce the creation of misleading or vacuous neometaphorical pseudo-domains. Linguists for instance constantly use metaphors from other disciplines, not to mention everyday life: rules are recursive, constituents move, fill gaps, have landing sites, bear labelled brackets which can be erased, rules apply cyclically, clitics have hosts, parasite vowels get into clusters, tones spread left to right (or the reverse), linguistic family trees branch, mother languages have daughters who are each others' sisters. This certainly makes our metalanguage more lively; the question is whether it does anything else. The fundamental issue is whether the terms of the metaphor actually have referents, or at least can point to some ontologically specifiable domain.

2. Historical Linguistics as a Metaphor-hijacker

Historical linguistics has at various times—now more than ever—borrowed from genetics. But why should it want to do this? The motivation belongs perhaps as much to the sociology of science as to the discipline itself: at certain theoretically stressful times self-descriptions and praxis are driven by what are perceived to be prestigious field-external models. At least since the mid nineteenth century linguistics has borrowed terminology and conceptual domains from other sciences (in the loose, non-Anglophone sense of 'Wissenschaften'). The richest source has been biology, at points when it seemed to be the 'hardest' science that linguistics could reasonably hang itself onto[1]. A good-sized subset of linguists seems have always wanted to be natural scientists. Some actually were: the nineteenth century August Schleicher was a botanist, and the contemporary William Labov was an industrial chemist. One major spur to this development in the nineteenth century was Darwin: linguistics in the 1850s was primarily a historical discipline, and Darwin was probably the first theoretician who succeeded in turning one of the classical historical sciences into something approaching synchronic natural science by sophisticated (if informal) modelling[2].

Subject-envy is not uncharacteristic of relatively new disciplines, especially ones whose ontological substrate is ill-defined. Linguistics has tried at various times to make itself look like biology, physics, mathematics, cognitive science, or all of them. And when in the 1930s and 1940s it looked like becoming about the best thing of its kind around, less secure subjects like anthropology began to borrow some of its concepts, imagery, and terminology. Linguistics is certainly the source of the fruitful notion of an 'emic' level of organization, or what we might call 'structuralism' in general. The spirit of Saussure brooded over the face of the waters for a long time, and social scientists like Lévi-Strauss and Piaget were certainly among those who felt the (cold or warm) breath of linguistics.

But why should we linguists keep doing it, since others are apparently so enchanted by us that they have borrowed *our* lexicon and imagery? The answer I suppose is that we are not (or not yet) an empirical or hard science; we seem to rest uneasily somewhere among the 'human sciences' or 'Geisteswissenschaften', if with a widespread yearning for a different status. Even the universities that hire us are not always sure where we go: before we reached our theoretical puberty we used to get attached to

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[1] See for instance Percival (1987); Davies (1987); and Wells (1987) on the 'organic/organismic' metaphorical domain in linguistics.

[2] Darwin himself was a great fan of metaphor: his work is full of good ones taken from elsewhere like the 'tree of life', and original ones like the famous 'tangled bank' and ecological 'wedges' (Darwin 1859).

anthropology, then to language departments. In two universities I have taught at, linguistics is in the Arts Faculty; in others it is among the social sciences; at my present university it is in the Faculty of Humanities (along with Arts and Social Sciences!); but it always seems a bit of a misfit wherever it is. My own view is that linguistics is not entirely suited to cohabit intimately with either 'arts' or 'social sciences' *sensu stricto* (at least as the boundaries are currently drawn). Since on one interpretation our concern is human cultural products, I rather think we are (or ought to be) a biological science, as the 'social sciences' ought to: culture in the end is inseparable from and based on biology (Tooby & Cosmides 1992; Wilson 1997:105-126). But most humanists would regard this as heretical reductionism, and biologists would laugh at our pretensions. The problem has been nicely summed up by E.O. Wilson (1997:111-112):

> The principal theoretical difficulties of the social sciences are two in number. First, in the study of culture there are no 'natural kinds', basic atomic equivalents to genes, cells, and organisms that can form the base of permutational operations in analysis. The lack of natural kinds guarantees the second difficulty, 'nomic isolation'. Each major discipline—anthropology, sociology ... and so forth—has been required to develop its own conceptual base and language.

In the context of an interdisciplinary discussion involving geneticists, linguists and archaeologists, I think we can be honest about our envy. But the situation is complicated. One thing that has both helped and hindered us is that biologists want to talk about language and similar things as well, and we have been given (without asking) some new concepts that may be more trouble than they are worth.

3. An Example of Hijacked Metaphor

The problem Wilson raises, which upsets me enough to make me write this article, can be stated another way. Historical linguistics, unlike genetics or evolutionary theory, presently lacks a physical or quasi-physical basis for construing the objects it assumes to be passed down through time via variation and selection. We would have loved to have linguistic genes, nucleotides, 3rd codons, base-pairing, even phenomena as messy as polygenes, pleiotropy or incomplete penetrance. (I'll suggest later that maybe we might, in a speculative reconstrual of our domain.) At least we would like to have ways of imaging things like this rationally, in a way that makes us less 'eccentric' and more 'mainstream' with respect to other sciences.

This brings me, finally, to my subject: the use of hijacked genetic metaphor in discussing characteristic 'genetic' preoccupations like descent, modification, copying errors in transmission, propagation of errors, lineages, status of taxa, cladogenesis, and assignment of lower-level entities to clades. Let us begin by seeing what happens when we as historical linguists appropriate, in what is at the commonest working level a nonphysical discipline, terms that represent (more or less) physical objects in another subject.

It is a curious fact that the first connection between genetics—at least historical genetics—and historical linguistics was established before either discipline actually existed. In the eighteenth century, the first important formulations of genealogy in terms of lineages defined by shared errors was developed by textual critics, in the course of work aimed at establishing archetypes or originals of major classical and biblical texts (Cameron 1987). Cladistics, as it were, was invented and used in an intelligent and sophisticated way for a century before evolutionary theory arose. And of course, until the rediscovery of Mendel's famous paper at the turn of this century, and the invention of the idea of 'mutation', even geneticists didn't have much idea what 'copying errors' might be. Genes themselves, in the early days when both they and a mutational theory of evolutionary change were first accepted, were not much more 'real' (or non-metaphorical) than viruses were to Koch or Pasteur[3]. There had to be something there to do what was being done, but it was another half-century before we had a clear notion of what it was. And however much we don't

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[3] In fact well into this century viruses were defined negatively: invisible pathogenic residues left behind in ceramic filters when everything visible like cells was filtered out. Hard (sensory) evidence for viruses was only available in the 1940s when the electron microscope provided wavelengths short enough to visualise them.

know about it still, at least there is something physical there, those helixes coil and separate and form templates for the attraction of complementary nucleotides, triplets code for proteins, DNA sequences make RNA that makes proteins by stringing together amino acids.

Now that genetics is in most ways a much more sophisticated operation than filiational historical linguistics, we are borrowing like mad from geneticists, and indeed the more genetics I read the more I wish I was a geneticist, so I could avoid the intractable epistemological problems that face me as a historian of linguistic lineages. Or that I had a really clear sense of precisely how I use genetic metaphor (assuming it is really metaphor and not something more precise) in my work.

The problem is this. Genetics, especially in combination with the so-called New Synthesis, provides a very attractive conceptual framework for describing linguistic change and language diversification, and for phylogenetic taxonomy. But unlike the geneticists, we lack a material basis for our model-building, Alternatively, maybe we have one, but it is too far away from what we are actually modelling to be of much use. (I make the tendentious assumption that somewhere at the bottom of all we do as linguists, if not directly manifest in our practical work, lies a set of Darwinian processes in the brain (Dennett 1992; Calvin 1996; Churchland 1996—more on this below.)

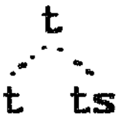
A characteristic example of how we would use genetic imagery in historical linguistics might clarify things. In the past our terminology has been different from that of historical genetics (which is I think a fair construal of phylogenetic systematics), by and large, even when we are engaged in similar activities like establishing filiations, family trees, etc. I am going to use a mix of old linguistic and new genetic terminology to illustrate both the kinds of things we do that impinge on a cross-disciplinary relation, and the problems this raises.

Take for instance the typical treatment of what we call 'correspondences', our primary tool in reconstruction and filiation. As a simple case, every time a native Germanic word begins in English or Dutch with a *t*-, it begins in German with a *z*-, phonetically [t] vs. [ts]. This correspondence is regular and replicable, as well as generally semantically transparent enough for us to recognize both the English and Dutch *t*-items and the German *z*-items as clearly 'descendants of a common ancestor'. (The arguments for ruling out convergence are good, but not relevant at this point). Some typical cases:

English	*Dutch*	*German*
tongue	tong	Zunge
tooth	tand	Zahn
ten	tien	zehn
tip	tip	Zipf(el)
to	te	zu
two	twee	zwei
twelve	twaalf	zwölf
twenty	twintig	zwanzig

This looks like a clear example of a language family in which two members (English, Dutch) constitute a subgroup, with German as an outsider. The relationship is patent, as is the subgrouping. Dutch is 'closer' to English (in this respect) than German. In standard linguistic comparative method we would ask ourselves which represents the older or 'ancestral' state, *t* or *z*? For various reasons not at issue here (including the presence of *t*-forms in other closely related languages belonging to a different subgroup, e.g. Swedish *tunga, två, tann = tongue, two, tooth*), we would reconstruct the *t*-group as representing the proximate ancestor or protolanguage, and call German innovatory in this respect.

Looking at this through the eyes of a different metalanguage (to produce a rather poor metaphor), we could say that *t*, *s* are character-states in a character-field; and English and Dutch (and Swedish ...) constitute a lineage showing one character state, and German another. Accepting that English and Dutch *t* are not homoplasious, our question is whether they are a synapomorphy or a symplesiomorphy; or reversing the coin, whether German *z* is an apomorphy or a primitive character. Cladistically, whichever alternative we choose, the result is the same: there is an event of cladogenesis, there are two sublineages which 'diverge' at the point where we might say (taking the usual view) that '*t* becomes *z* in German'. Regardless of which metalanguage we happen to use, the conceptual basis is the same in biosystematics and historical linguistics. It is some version or another of a bifurcation like



So: from the point of view of character-inheritance (and there is no reason, mathematical or other, why a *t* isn't just as much of a character as an eye), historical geneticists (systematists who are not antihistorical 'reformed cladists') and historical linguists are doing the same thing: studying lineages in time, and looking for clues in the internal makeup of character profiles of lineage members that can date or order events with some reliability, establish ancestries, closeness of relation, taxa and groups of taxa.

4. Beyond Systematics: 'Units of Inheritance' in Linguistics

Until recently, the use of genetic metaphor and imagery in linguistics was largely restricted to stemmatic/cladistic matters. Up to a certain fineness of resolution this poses no problems; no matter what the subject, the characters, atoms, elements, or whatever can be taken, if necessary, 'agnostically' as fictive points or nodes in trees, and the usual catchall terms like 'clade', 'taxon', 'primitive', 'derived' can be used interestingly and uncontroversially.

But since the 1970s things have changed somewhat; a new field of discourse and epistemological dismay was opened up for exploration. Earlier, the problem of 'units' in the social sciences, including linguistics, was not taken seriously except at the synchronic or 'structural' level. Linguists didn't generally seem to ask questions like 'Just *what* is it that is passed from generation to generation, that children acquire, that changes?'[4] Or if such questions were asked (as they were after Chomsky's various definitions of language as a 'mental organ', a computational module, etc.), the answers were not given, or thought about, in terms of anything

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[4] Some of us did, actually, but the answers were not 'hard' in a biological sense, and bypassed the whole 'inheritance' question. See for instance the rather unsatisfactory discussion in Lass (1997:chs.1,6-7).

'heritable' in a proper sense. In the standard Chomskyan model, the child uses its internalized Universal Grammar module to deduce from linguistic input what language it is being exposed to (the 'child as little linguist' model), and 'acquires' it[5]. But strictly speaking, nothing material 'passes' or 'is transmitted' between generations; each performs computations on the output of the previous one, and somehow establishes the results of those computations in its left brain.

The first serious attempt at an answer to the 'units' question came in the middle days of early sociobiology, in the 'culturgens' of Lumsden and Wilson (1981). These were conceptual or cultural units (structure and/or substrate not specified) that constituted the 'heritable material' of cultural evolution (or the cultural side of gene/culture coevolution in slightly later terms). But the soil this notion fell on was stony: in the 1970s human sociobiology was not very Politically Correct. There were also problems with Lumsden and Wilson's maths, the reviewing community was nearly uniformly hostile, and maybe the *Zeitgeist* was just wrong, or the presentation, and in any case the undertaking was tainted by association with Wilson in the influential left-wing circles dominated by figures like Gould and Lewontin. For some reason (largely careless and tendentious reading, I think), the early attempts at theoretical sociobiology got caught up in the 'nature' vs. 'nurture' debate, confused with vulgar genetic determinism, and generally misunderstood[6]. So at least an opportunity was missed.

But a proposal in the later 1970s took off rather better, and has led to the growth of a new field or subfield of combined rational enquiry and flakiness. This was Richard Dawkins' invention in 1976 of the *meme*. Would he have done it if he had known how much trouble it would cause? Of course being a biologist he could simply drop it in the path of the humanists and social scientists, and let them get on with the exegesis and their own

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[5] For a standard treatment of this view, but with a rare biological sophistication, see Pinker and Bloom (1992).

[6] There was a good deal of paranoia about the apparent right-wing totalitarian tendencies of sociobiology; partly because people who ought to have known better, like Stephen Jay Gould, saw in its genetic focus the (unintended) shadow of eugenics. See Gould's rather obtuse review of Lumsden and Wilson 1983, reprinted in Gould (1988:107-123.)

proper work, using the concept if it seemed appropriate—which is more or less what happened.

Anyone interested in organism/culture relations, and as literate and cultivated as Dawkins, was bound to see the possibility of heuristic analogy, and to want something more or less parallel to the gene which could play the part in cultural inheritance, structure and change that the gene does in the biological realm, and further would embody as many of the standard Darwinian-replicator-system properties as possible[7]. That is, a cultural 'lineage-making system' should display at least:

☞ The presence of replicators.
☞ Relative fidelity of replication, plus the possibility of copying error.
☞ Selectional bias or 'editing' in the course of historical transmission.
☞ The possibility of going to stabilization in a population.

And, to add Dawkins' special personal take on genes and their relation to the organisms they occur in and are blueprints for, the possibility of 'selfishness'. Twenty-odd years on it is still not entirely clear what memes are, where they live, or what kind of internal structure they might have.

I suppose that if memes are like genes, they are rather more like the broader definition in *The extended phenotype* (1982): units of whatever size or constituency that produce coherent phenotypic effects. The typical meme may be more like a polygene on the one hand, or a highly pleiotropic single gene on the other. But this is too early in the story to get into that.

One characterisation, that of John Hyde Bonner (1980), is so loose and insightful as to be really tempting. He manages to use the concept very elegantly without ever telling us precisely what it is. First he notes uncontroversially that 'Dawkins has not attempted a rigorous definition of a meme' (though we would all agree that he gives enough examples for an ostensive definition in terms of 'things like ...'). Then Bonner (1980:17) provides his own: 'any bit or collection of bits of information passed by behavioural means from one individual to another'. This somewhat skimpy definition provides him with a beautiful framework for comparing behaviours and

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[7] This would, if successful, unify both 'mental' and 'physical' phenomena under the rubric of 'universal Darwinism' (see Dawkins 1983).

elements of what we may call 'culture' from nearly all organizational grades in the animal kingdom, and constructing something of a historical narrative.

Memes have all the correct Darwinian properties, and another, normally reserved for pathogens, which by some is taken as their real secret: 'infectiousness'. Dawkins sees memes (especially ones he doesn't care for, like religion) as 'parasitising' minds, like cultural tapeworms. This would appear to make them rather more like whole organisms with their own 'interests' than genes (which though having interests, are generally not thought of as parasitic). But whatever, we do have a problem of specificity; if memes are nothing more than just 'bits of information', the concept is too vague to be of much theoretical use.

The lack of specificity is indeed troubling. At least the following have been considered, in one place or another, as reasonably prototypical memes: lines of verse, proverbs, novels, tunes, chord-progressions, symphonies, phonemes, syntactic constructions, political systems, kinship systems, modes of agricultural production, incest taboos, religions ... in fact all the potential inhabitants, of any size or degree of structural complexity, that might be found in Popper's equally vague 'World 3'. Popper of course carefully avoids most of the hard epistemological problems by claiming first that World 3 objects are 'ideal', and second that they 'exist, but exist nowhere' (Popper & Eccles 1977:450). I don't think this is good enough. Perhaps we should look at a recent proposal about what memes might 'really' be, how they could be given a physical basis, and might be replicated, inherited, and changed.

Nikolaus Ritt (1995) has made a brave if problematical attempt to specify, within a theory that allows for both memes and biological realism, what he calls 'linguistic genes'. This appears to be the first proposal for a micro-level characterisation of what a meme might look like, how and where it might be organised, and what its replication would consist of. That is, he presents a first approximation of a physicalist and reductionist linguistic version of memetics, with a quite biological-looking substrate and *modus operandi*.

His primary example of a linguistic replicator is a speech-sound or phoneme. He suggests that successful acquisition by a child of a speech sound of its native language ought to be considered from a radically nontraditional point of view. That is, rather than looking at the child as an 'agent', using his wired-in computational language module to process input (though of course this is part of the process), Ritt takes the viewpoint of the acquir-*ee* (the meme) rather than the acquir-*er*. Since children as potential speakers can't control their language acquisition, it might be reasonable to look at the acquired items, fed to children by the speech community, as parasitising them and replicating in their brains, and see the acquisition process from *their* point of view. Thus the language learner becomes a Dawkinsian vehicle for the propagation of linguistic 'genes' = memes.

'Language itself' then (whatever this really means) may not be physical, but it has a physical substrate. It is cognitive, which in a non-dualist epistemology means neurological. Ritt appears to take it for granted (as I do) that all storage and activity in the brain are in some sense material or physical—on grounds of parsimony if nothing else. Occam's Razor alone (even if there wasn't a wealth of better philosophical and empirical argument) would militate against Cartesian dualism or Ghosts in the Machine[8].

In this provocative if preliminary paper[9], Ritt tells a story of the 'neural Darwinism' type, in which memes compete in the cerebral arena for space, and those that are ultimately selected win out, and are established in the speaker as part of his 'linguistic competence'. Ritt suggests as the substrate for this process something like a Hebbian cell-assembly (Hebb 1949), a neuronal cluster or unit of high connectivity and specificity, with projections to and from all centres involved in speech. Thus the phoneme /t/ would in the adult speaker be stored rather like any other small-scale (accessible, 'declarative') memory.

So in the mature user a /t/ or any other unit is hypothetically some kind of small discrete assembly. This is not at all out of line with recent theorising about the nature of storage units, e.g. Calvin's diffuse network of neocortical pyramidal hexagons as the basic units of the 'cerebral code' (Calvin 1996). We assume then, following this speculative but sensible line, that in the mature speaker each such assembly has afferent projections (e.g.

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[8] For some of the better arguments see Dennett (1991); Damasio (1994); Cairns-Smith (1996); and Churchland (1996).

[9] For further commentary on Ritt's paper, from other points of view, see Lass (1996).

from the auditory cortex) and efferent ones (to the motor cortex)[10]. There are then two sides to this assembly's behaviour:

A. *Productive*. Each time the motor cortex triggers this assembly, it 'fires' or activates or is represented, much as in Calvin's view memories are activated in the course of identifying an object. Thus when I say a /t/ (this is my interpretation, not Ritt's), it is within the model the linguistic 'equivalent' (a metaphor) of an instance of gene-expression[11].

B. *Receptive*. Each time the auditory cortex perceives and mediates a token of this unit coming in from the environment, it is identified in the first instance *as* such a token via afferents to (presumably) the medial temporal lobe, and from there further identified as a 'code unit', and processed by the more 'dedicated' language centres in the posterior temporal and temporoparietal regions ('Wernicke's area').

But how would such memetic assemblies get established in the first place, so they can serve as substrates for these hypothetical 'genetic' units? Ritt suggests differential reinforcement of synaptic connections through stimulus-repetition and some kind of accompanying 'reinforcement'. Thus the mechanisms behind the establishment and use of the stored meme would be entirely consonant with neurobiological principles. Consistent perception or activation in the motor mode strengthens specific synaptic connections (presumably in the first instance by Long Term Potentiation of neurons in the hippocampus: Squire 1998); and a stable collection of communicating and functionally integrated synapses forms the internal representation of a meme. Thus we can get cross-cortical 'cloning' of copies of the meme, which replicates itself within the speaker in competition with all other similar memes (e.g. weaker inputs from other 'alleles' or output variants in

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[10] Ritt does not go into much neurological detail; speculations about parts of the brain active in the processes he suggests, and mechanisms, are my addition, designed to flesh out the picture a bit more.

[11] This would presumably occur through afferents from the 'language centres' like 'Broca's area', presumably with retrieval of posteriorly stored material via the arcuate fasciculus.

the community). And in the learner, it is exposure to the meme that sets up the memory and retrieval circuits necessary to its later incorporation and regular production. So the acquisition story becomes Darwinian (in either the Dennett or Calvin sense): excitation of one assembly among many leads to synaptic strengthening, and other near-competitors are selected against, so that eventually the internal wiring comes to look more or less as it did in the parent, or the speech community, from which the meme propagates or attempts to.

The language system of the community then is a 'memome' (Ritt's term), which attempts to replicate itself, meme by meme, in the brains of learners. Successful language acquisition is the end-product of replication of a set of memes, just as successful birth of a normal foetus is of replication of the zygote's genome. In the case of language acquisition, the child is 'rewarded' with communicative competence by mastering or internally replicating—or better suffering the internal replication of—the memome. And of course there can be miscopying as well as variation, or minority 'alleles' can win out, and new lineages can be established by propagation of these variant copies, which will compete with the old ones. Thus language change is enabled by essentially the same mechanisms as acquisition—though not necessarily or even very likely at the same ontogenetic stage. That is, after acquisition later peer-groups can be foci for locally more prestigious variants, which would then presumably undergo the same kind of competition with established variants. In Ritt's (1995:53) words: 'speakers figure exclusively as the environments in which the replication of linguistic elements takes place'.

This is at the moment somewhat simplistic, underdetailed, and speculative, but no less valuable for that. Its great virtue is that it suggests a way in which we might construct research programmes incorporating and testing hijacked metaphor. This kind of thinking could just make our use of other folks' metaphors precise enough so we can find out if there is any genuine ontological or structural congruence. And that would be nice to know, whether the answer is positive or negative.

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