Against Thatcherite Linguistics: Rule-following, Speech Communities, and

Biolanguage

(final version, forthcoming in *Southern Journal of Philosophy*)

Abstract: According to Chomsky and his followers, language as a biological phenomenon is a property of individual minds and brains; its status as a social phenomenon is merely epiphenomenal, and not a proper object of scientific study. On a rival view, the individual's biological capacity for language cannot be properly understood in isolation from the linguistic environment, which it both depends on for its operation and – in collaboration with other speakers – builds and shapes for future generations.

I argue here for the rival view by demonstrating firstly its greater consonance with several themes in current biological theory, and secondly its ability to answer two well-known philosophical challenges to generative linguistics due to Quine and Kripke.

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According to Noam Chomsky and his followers, the only proper object of study for a biologically-informed science of linguistics is the I-Language; the idiolect realised by the mind/brain of each individual speaker. Let us – mischievously, perhaps – call this a 'Thatcherite' view of language; there is no such thing as linguistic society as far as Chomskyans are concerned, just individual men and women. And it follows on this view that linguistics is not normative; there are no external public standards against which a mature speaker may be judged to have erred in her speech.

This view runs directly counter to another, more collectivist attempt to take language seriously as a biological phenomenon – a distinctive part of the 'natural history' (Wittgenstein 1953, §25, §415) of the human species. Various arguments have been made for this alternative view of language, which are brought together here for the first time. On this account, language is an inherently social and public phenomenon, and is as such necessarily governed by normative rules; it is intrinsically the sort of thing that we can 'get wrong' by the lights of our peers.[1]

In this paper I identify tensions between the Chomskyan view and several current trends in evolutionary biological theory. I provide for the first time an outline of the collectivist view and, while I do not give new arguments for it here, I demonstrate that it sits more comfortably with the most plausible current hypotheses about how the human language faculty might have evolved. Following this survey of the landscape, I then show that the resulting 'neo-Saussurean' view of language also resolves two influential philosophical challenges to Chomsky's generative linguistics, due to W.V. Quine and Saul Kripke. Both Chomsky's signature project of a generative theory of linguistics, and his aspiration to a true

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[1] The distinction I draw here is not that identified by *e.g.* Kim Sterelny (2016, p. 173-4) between the Chomskyan, generativist view of language and its evolution, and a rival tradition treating it as 'a complex, coadapted system of communication' which in large part arises through social interaction and iterated learning rather than 'hardwiring' in the brain. The communitarian views I consider here remain generativist in nature and, though they modify Chomsky's nativist commitments, are similarly impressed by 'poverty of the stimulus' arguments; so this is an internal dispute within generativism. That being said, these views have clear affinities with those Sterelny discusses, and perhaps lay the ground for future rapprochement between the traditions.

'biolinguistics', I therefore argue, are best served by dropping his Thatcherite commitments to an individualist and anti-normative ontology of language.

I: The 'Thatcherite' Picture

The basic outlines of Chomsky's generative linguistics are well known, and need not be repeated here. What will be relevant to note is a pair of distinctions he introduced regarding the proper object of linguistic study. He distinguished initially between linguistic 'competence' and 'performance,' or the 'abstract knowledge of language acquired by the child and possessed by the adult' and the 'concrete linguistic phenomena produced and heard by the speaker' (Glackin 2011, p.202). This replaced, among generative linguists, the old Saussurean distinction between *parole*, which corresponded closely to performance, and *langue*, the common resource of public language (Saussure 1983, pp.13-15 & *passim*).[2] The study of performance, or *parole*, in the eyes of Chomsky and his followers, could not be

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[2] Saussure (1983), a posthumously-published selection of lecture notes, is not abundantly clear in stating the distinction. But roughly, *langue* is regarded as the proper object of study for linguistics, and is both 'a social product of our language faculty … [and] a body of necessary conventions adopted by society to enable members of society to use their language faculty' (p.9-10). It is an abstract resource; 'a fund accumulated by the members of the community through the practice of speech (*parole*), a grammatical system existing potentially in every brain, or more exactly in the brains of a group of individuals,' and 'is never complete in any single individual, but exists perfectly only in the collectivity' (p. 13). By contrast, 'there is nothing collective about *parole*,' which includes both writing and speech; 'its manifestations are individual and ephemeral' (p. 19); it represents the actual utterances made by individual speakers.

regarded as the subject of a properly *scientific* linguistics; its data were transitory, unsystematic, unrepeatable, unmeasurable. Following what was termed the 'Galilean' strategy, the proper object of linguistic study was considered to be the 'ideal' law-like language, independent of the imperfect observable data through which it manifested itself.

But this, in turn, was increasingly conceived of as a specialised neural module; as something biological rather than public. Accordingly, the Saussurean *langue* was soon likewise dismissed as part of 'E-language', at most an external, epiphenomenal result of the real scientifically significant phenomenon, the 'I-language' in the individual speaker's brain (Hornstein 1998, p.340). Accordingly, from Chomsky (1986) onward, competence and performance were superseded by 'I-language' and 'E-language', respectively. I-language is intentional, individual, and internal; the neural and mental structures possessed by an individual speaker which generate grammatical strings of words (Chomsky 1986, ch.2; Chomsky 1995, p.25ff.; Chomsky 2000; Isac & Reiss 2008). E-language is whatever in language is not I-language; it is extensional, external, and collective. Its correct definition does not matter; 'we can define E-language in one way or another or not at all, since the concept appears to play no role in the theory of language' (1986, p. 26). But it is meant to include both individual utterances (a la 'performance') and abstract public objects such as 'the French language'. Linguists would also be largely unconcerned with how the I-language states of the brain 'got there' in evolutionary terms, and rejected the idea that it was 'for' communication or anything else; such questions were simply not part of the new science of linguistics.

For Chomsky and his followers then, insofar as it exists, public language is simply an epiphenomenal product, composed of numerous individual speakers and their brain-structures. And in a strictly mereological and synchronic sense, this is of course correct. It is uninformative, however, about how these I-language structures came to be in human brains,

which Chomsky does not consider an interesting or fruitful area of scientific enquiry (*e.g.* Chomsky 1972, p.97 ; Chomsky 1988; Chomsky 2002, p.146; Fitch *et al.* 2005, p.189).

That it has something to do with the genes is not in question. Chomsky himself spoke of the neural 'Language Acquisition Device' (LAD) posited by generative linguistics as both a 'linguistic genotype' and a 'genetic program' (1980, p.75; p. 244. For a comprehensive view of the pervasiveness of this picture among generativists, see Lorenzo & Longa 2009, p.1302). The bulk of grammar, on the classic 'Principles and Parameters' generativist view, is genetically encoded, with the contribution of the environment only marginal or secondary. The later 'Minimalist Programme' adds an important third factor in the form of general principles not specific to language, but a significant shaper of language remains '(g)enetic endowment, apparently nearly uniform for the species, which interprets part of the environment as linguistic experience' (Chomsky 2005, p.6). But how this genetic endowment might have evolved does not interest the traditional generative linguist; 'Chomsky and others', as one supporter notes, 'have consistently resisted facile adaptationist stories of the sort that populates the evolutionary psychology literature' (Boeckx 2012, p.493).

The concept of genetic information at work here in Chomsky's theory is explicitly the 'program metaphor' developed by the likes of Ernst Mayr (*e.g.* 1961), Jacques Monod (1970), and François Jacob (1970); '(a)ll manifestations of development are controlled by genetic programs' (Mayr 1982, p.106). On this 'neo-preformationist' view,[3] the environment merely 'triggers' the otherwise inevitable development of whatever phenotypic properties the genes instruct. As the Minimalist Programme has downgraded the role of genes, Chomsky has in recent years pointed to links between his theory and evolutionary developmental biology

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[3] See Griffiths & Stotz (2000, p.34); for specific application of the position to Chomsky, see Lorenzo & Longa (2009, p.1305-7).

('Evo-Devo'), which stresses the importance of the role played by embryonic and later developmental processes, and modifications to them, in evolutionary change (2007a; 2010). But as Antonio Benítez-Burraco and Víctor Longa have argued (2010), the particular form of Evo-Devo which Chomsky is interested in preserves the preformationism of his earlier view; that is, following Seán Carroll (2005), development remains a matter of switching structural genes on and off at the appropriate times, a process itself predetermined by the genetic program (*e.g.* Chomsky 2007a, p.3).

II: The Communitarian Picture

In recent years, however, this 'genocentric' and reductionist understanding of evolutionary processes has come to seem rather less tenable to many biological theorists. Indeed, the very notion of 'genetic information' has increasingly been regarded as confused. According to Susan Oyama's influential 'parity thesis', it no more makes sense to posit that the inherited 'blueprint' for a given phenotype is encoded in the genes, considered independently of the environment in which they are expressed, than to locate the same blueprint in the environment, with the genes' role relegated to 'carrying out' the plan by building proteins as and when instructed (Oyama 1985). Thus, Developmental Systems Theory (DST) has stressed that the effect of genes – in and of themselves, 'among the most impotent and useless materials imaginable' (West-Eberhard 2003, p.93) – can be understood only within the wider context of molecular, cellular, organism-wide, and environmental interactions.[4] There is obviously a trivial sense in which, *holding these other factors constant*, genomic differences will determine differences in phenotypic outcome. But it is equally the

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[4] See *e.g.* Oyama (1985); Griffiths & Gray (1994); Richardson (2000); Robert *et al*. (2001); and the essays collected in Oyama *et al.* (2001).

case that, if the genome is held constant, any phenotypic differences must be due to the other factors; yet nobody supposes that the environment has 'programmed' what differences there are between monozygotic twins (Dupré 2008b, p.88).

A related trend in recent evolutionary theory emphasises the fact that the modification by organisms of their environment, and the consequent inheritance of environmental conditions, are important evolutionary processes in their own right. Beginning with the work of Richard Lewontin (1982; 1983; 2000), 'Niche Construction Theory' (NCT) has stressed the dialectical relation between organisms and the ecological 'niches' in which they reside. Thus, we should not regard natural selection as 'fitting' organisms to pre-existing environments; rather, organism and environment constantly re-shape each other in a dynamic and reciprocal fashion (*e.g.* Godfrey-Smith 1996; Laland & Sterelny 2006; Krakauer *et al.* 2009). The cultural environment of humans represents a key case in point; we are selected for our ability to thrive in an environment rich with culturally learned and transmitted knowledge, but that environment is itself the direct product of our and our ancestors' evolved behaviour (Odling-Smee & Laland 2012). So neither our genes nor their cultural context can be considered independently of each other; they evolve together in a relationship of mutual causal and conceptual dependence (*e.g.* Feldman & Laland 1996; Laland *et al.* 2001; Laland *et al.* 2010).

These closely-linked biological theories remain controversial, and I cannot present an argument for them in the space available here. However, the difficulties they present for the I-language perspective should be readily apparent. Chomsky and his followers conceive of the individual's genome as containing all the information necessary for construction of an LAD, which needs only to be 'switched on' by the appropriate linguistic environment for the phenotypic effect of I-language to be produced. But this is just the sort of picture which the parity thesis declares incoherent, and which the growing DST literature seemingly shows to

be at best deeply simplistic about how genetic differences are realised in phenotypic outcomes. Serious doubt is cast, too, on the 'merely mereological' picture of the E-language. While *some* collective and social phenomena may be understood simply as the sum of evolved individual behaviours, what NCT purports to show is that it is far more usual for the individual behaviour itself also to be simultaneously an evolved response to the already-existing social context.[5] There is thus typically a relation of cyclical and reciprocal accommodation between the two perspectives; and if human language conforms to this pattern, then a linguistics focussing exclusively on the I-language perspective would be a severely diminished and myopic explanatory science.[6]

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[5] Dupré (2008a, p.73) provides an instructive non-biological analogy; 'The [Albert Cuypstraat street market in Amsterdam] depends for its existence on the people who go there to buy and sell; but it is simultaneously the power of the market that attracts the people that constitute its continued existence'. *Cf.* Dupré (2008b, p.95).

[6] For what it's worth, I don't think rejecting the 'extended evolutionary synthesis' would do much to rescue Chomsky's position. To see why, consider Maynard Smith's (2000) classic, orthodox account of biological information. Defining information *causally*, as he writes, we get the parity thesis; 'there is no difficulty in saying that a gene carries information about adult form; an individual with the gene for achondroplasia will have short arms and legs. But we can equally well say that a baby's environment carries information about growth; if it is malnourished, it will be underweight' (p.189). This won't do for the Chomskyan, who regards the genes and not in any interesting sense the *langue* as carrying the information specifying the developmental path of the I-language. Nor would it do for Maynard Smith, who similarly wished to justify the use of informational language 'to characterize genetic as opposed to environmental causes' (*ibid.*). Maynard Smith's solution was instead to define biological information *semantically*, arguing that 'the distinction can be justified only if the

That language does conform to this pattern is just what the recent, strikingly convergent, accounts of language and its evolution that I have described as 'neo-Saussurean' urge. These views, developed by Stephen Anderson, Eva Jablonka and her collaborators, and myself, are all in essence generativist; they hold that the basic account of the structure and acquisition of grammar elaborated over the years by Chomsky and his followers is correct. That is, in brief, the infant's brain contains a specialised mechanism for acquiring language, which operates during a particular developmental window; the principles encoded in that mechanism, in turn, tightly constrain the grammatical rules permissible in any language

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concept of information is used in biology only for causes that have the property of intentionality… A DNA molecule has a particular sequence because it specifies a particular protein, but a cloud is not black because it predicts rain. This element of intentionality comes from natural selection' (p 189-90). But drawing the distinction in this way doesn't help the Chomskyan either, for two reasons. Firstly, if we draw the distinction in this way, it is insufficient to differentiate between genetic and social-environmental causal influences on the development of an individual's language faculty. That is to say – like DNA but unlike rain clouds – any given *langue* or E-language has the configuration it does precisely *because* it functions to shape the linguistic development of its eventual participants in the way that it does. Both genetic and environmental causes of linguistic development are therefore equally intentional, and carry information in the same way. Secondly, and inconveniently, Chomsky has specifically denied on many occasions that the I-language is an adaptation, positing instead that it is a side-effect of selection for cognitive recursion (*e.g.* Berwick & Chomsky 2016, p. 66, 71). If this is the case our genes do not carry semantic, but only causal, information regarding the I-language, and Chomsky's 'genetic programme' falls on the non-intentional side of Maynard Smith's distinction. I thank an anonymous referee for pressing me on this point.

which is learned during this window, creating the familiar stock of linguistic universals shared by all human languages. Where these views diverge from the orthodox Chomskyan picture is in the robust hypotheses they issue about the sort of evolutionary processes that are likely to have produced the LAD, given the specific details of the languages it produces,[7] and the implications of these evolutionary hypotheses for the relation between the linguistic capacity contained in the individual speaker's brain, and the linguistic environment in which it is formed. Chomsky and his followers, they urge, can avail of an updated view of evolutionary biology which had been regarded as unavailable, resulting in a *better* version of generativism.

All three theories make central use of the so-called 'Baldwin Effect', a quasi-Lamarckian process which allows learned behaviour to be progressively 'assimilated' into the genome. If a given learned behaviour significantly advances fitness, then there may be selection pressure for the ability to learn the behaviour with ease and alacrity. Thus, organisms whose initial, pre-learning neural structures need the least 'rewiring' to achieve the post-learning state begin to dominate the population, and the learned behaviour becomes more ubiquitous; this intensifies the selection pressure, setting up a positive feedback loop, which drives the population steadily towards 'already' knowing – or instinctively acquiring – the behaviour which their ancestors had to expend valuable cognitive resources learning and teaching.[8]

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[7] *e.g.* Glackin (2011, p.208-11) points out that Baldwin effects, by 'self-terminating', explain the otherwise puzzling fact that the principles of Universal Grammar have unset parameters and, consequently, the vast diversity of human languages.

[8] The essays in *eds.* Weber & Depew (2003) remain the most thorough overview of the field, though Sznajder *et al.* (2012) is more up-to-date.

Jablonka – a self-described 'neo-Lamarckian' – is one of the foremost theorists of epigenetic inheritance, or 'the transmission of phenotypic variations that do not stem from differences in DNA base sequence from one generation of cells or organisms to the next' (Jablonka & Lamm 2012, p.19). Older, gene-based models of heredity, Jablonka and her co-authors argue, cannot explain the evolution of learned symbolic and other cultural traditions – including language – among humans or other animals, 'because learning involves developmentally-mediated acquisition and transmission of variations' (Jablonka & Lamb 2008, p.393). So the development of the LAD must be understood as the result of a complex interaction between our ancestors and their cognitive niche; as language becomes ubiquitous in a population, the ability to master conventions for manipulating symbols becomes crucial, and a Baldwin Effect begins. As the cognitive resources needed for learning are freed up by this process, more complex behaviours become available to the learner, and as they become ubiquitous are themselves progressively assimilated in what Jablonka and her colleagues term the 'assimilate-stretch' principle (*e.g.* Dor & Jablonka 2000, p.46; Jablonka & Lamb 2005, p.290).

Anderson proceeds from a rather different set of – specifically linguistic – considerations. Human language, he observes, shows clear signs of being the product of evolutionary processes: it is (contrary to linguists' dogma – see Carstairs-McCarthy 2011) variable across the population, and that variation seems to be both heritable and adaptive (Anderson 2013, s.1.1–1.3; see also Pinker & Bloom 1990). But this claim is widely supposed to run counter to another widespread intuition about language, that its form and features are best explained in terms of its 'external', functional or processing requirements (Anderson 2008, p.795. See *e.g.* Bickerton 2011, p.459-60; Lightfoot 2011, p.316). The tension between the two explanations, Anderson shows, is illusory; via a Baldwin Effect, linguistic conventions may be developed and adopted for functional reasons, then subsequently assimilated into the genetically endowed language faculty. So a particular functional feature

of grammar, like conforming to the principle of subjacency, becomes adaptive not for its intrinsic features, but by virtue of its prevalence in existing usage (Anderson 2011, pp.368-9).

I have pointed out, in turn, that this sort of cyclical assimilative process is the only explanation that would not make the evolution of a highly functional Chomskyan LAD implausibly dependent upon evolutionary saltations (Glackin 2011, pp.204-6). Each new rule or stage of grammatical complexification requires a discrete advance; but this can be accomplished by innovations at the level of linguistic performance, governed by general intelligence, and thereafter gradualistically assimilated. One notable upshot of this hypothesis, I argue, is the dissolution of Chomsky's principled distinction between competence and performance, or I-language and E-language; I-language is now regarded as the product of a complex interaction between the E-language and an LAD which is itself composed of that part of the E-language which at some point in evolutionary history became sufficiently ubiquitous to generate its own selection pressures (2011, pp.217-8).[9]

A further consequence is the rehabilitation of that part of the E-language which Saussure termed the *langue*, the communal 'fund' of linguistic resources which 'is never complete in any single individual, but exists perfectly only in the collectivity … only in virtue of a kind of contract agreed between the members of a community' (Saussure 1983, pp.13-4). No longer just the mereological sum of individual speech-acts, *langue* is elevated by the Baldwin Effect hypotheses to a doubly central position in the causal understanding of biolinguistics. That is to say, it represents both the environmental niche which co-evolved with and today makes sense of the human language faculty, and the presupposed

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[9] There are instructive parallels between the fossilisation of these 'subsequently basic' elements of grammar and the idea of 'generative entrenchment' in evolutionary theory developed by Bill Wimsatt (*e.g.* Wimsatt 2001).

informational context in whose absence an inert neural LAD could not make any meaningful contribution to human development.

It is for this reason that I have termed the communitarian views in question 'neo-Saussurean' versions of generativism. Though they certainly do not share all of Saussure's assumptions, the key feature which distinguishes them from the more orthodox generativists is this understanding that language can neither be conceived as existing abstractly, outside the minds of individual speakers, nor as being entirely reducible to what *is* in their minds. *Langue* and *parole*, Saussure argued, are 'closely linked and each presupposes the other. *Langue* is necessary in order that *parole* should be intelligible and produce all its effects. But *parole* also is necessary in order that *langue* may be established' (1983, p.19).[10] Likewise, for the neo-Saussureans, I-language and *langue* both causally and conceptually presuppose each other. I-language is not a stand-alone faculty, but an adaptation to and for life in the linguistic milieu of *langue*; the same *langue* is both composed of the speech generated by all the relevantly proximate I-languages now in existence, and structured by their fossilised ancestors.

Linguistic competence as embodied in the I-language is indeed biological, then, and based in the individual brain; but to understand it, we can't ignore the 'non-biological' element of language. Or rather, and more accurately, we can't dismiss the social element as non-biological. Human biology is the study of a thoroughgoingly social animal, and the lesson we learn from modern rejections of the biological/social distinction is that biolinguistics, as such, must take account of the intrinsically social element of human language. The 'developmental niche' a child occupies is not just the external stimulus which

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[10] For clarity and consistency, I have restored the original terms to the translation; Harris uses the more natural, but more ambiguous, 'language' and 'speech'.

triggers the LAD, but its origin and source; the functioning of I-language can only be understood in terms of its cyclical interaction with *langue*.

I have given an overview of the existing arguments for the neo-Saussurean picture of language so far, without adding any new support. Nevertheless, the convergence between the conclusions of these independent arguments by Anderson, Jablonka and her collaborators, and myself, which has not been previously noted in the literature, provides additional abductive evidence for each of them, and for the picture as a whole. In addition, various other figures have made arguments supportive of this or similar positions; I can briefly review only a few here. The computational models developed by Reiji Suzuki and Takaya Arita have demonstrated that, given plausible assumptions about the selective advantages conferred by communicative ability, the sort of Baldwin Effects and similar assimilative processes hypothesised are indeed likely to have arisen (Suzuki & Arita 2013; Azumagakito et al. 2013). Kim Sterelny has developed in great and illuminating detail the idea of human culture – and language in particular – as forming an 'informational commons' created by and co-evolving with our ancestors as a way of sharing expertise in response to 'a heterogeneous and changing environment', in a classic piece of niche-constructionist theorising (2012a, p.76, 138: see also Sterelny 2008; Sterelny 2012b).[11] And Peter Ludlow posits what he terms 'the Ψ-language hypothesis', which holds that the psychological states which underlie language are internal to the speaker's mind/brain, as Chomsky and his followers claim, but nonetheless 'individuated in part by the embedding environment'; he thereby seeks to show the interdependence of E-language and I-language in what appears to be a broadly similar (but

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[11] To be clear, Sterelny's position is not a Chomskyan one. But it supports the claims made here about the role and importance of the community in language-acquisition processes, with which neo-Saussureans believe the Chomskyan position should be supplemented.

more formal than biological) manner to the neo-Saussurean view (2011, p.xv; see esp. p.44-63).

III: The Minimalist Evolutionary Hypothesis

In recent years, despite their distaste for causal-historical explanations of the language faculty, Chomsky and some of his followers have produced a rival evolutionary hypothesis in support of a Minimalist version of the Thatcherite I-language view against which I argue in this paper (Hauser *et al*. 2014, Berwick & Chomsky 2016; *cf.* Fitch *et al.* 2005). Since it does not form part of that view *as such*, I discuss it here separately, focussing on the recent monograph by Chomsky and computational linguist Robert Berwick. Each stage of the argument, I shall claim, looks problematic in light of the neo-Saussurean literature, with which its authors make no attempt to engage.

Berwick and Chomsky begin by invoking what they call 'Darwin's problem'; Darwinian explanation posits gradual change by the accumulation of tiny modifications, yet the human language faculty looks like a biological leap, not shared by any other animal and not functional in merely-partial forms. So what they describe as the 'first motivation' (p. 11) of their book is to effectively 'shrink' the explanandum. The language faculty that *did* evolve must be the kind of thing that *could* have evolved; therefore, they argue, since it could not have been produced gradualistically, and if we are not to invoke 'saltation', or large-scale

macro-mutations, the key elements of language must be small enough to be achievable by much smaller-scale macro-mutations.[12]

This requirement is the key departure point for Berwick & Chomsky's hypothesis; but the requirement is a spurious one. Since they regard E-language as merely epiphenomenal to the genetically-programmed I-language, Berwick and Chomsky reason as though functional advances in language must take place initially at the genetic level, or not at all. But there can scarcely be a feature of human language more familiar than our capacity for innovation and inventive play, which obviously does not require prior revision of the genome. So if some such novelties prove fitness-enhancing[13] they can spread throughout the linguistic community in *parole* or E-language, before selection operates, via a Baldwin Effect, on the ability to learn them. It is just 'when intermediates provide no benefit and a combination of simultaneous mutations needed to provide a functional whole is improbable,' the very situation Berwick and Chomsky describe, that Baldwin Effects become important (Bateson 2005, p.37). Thus, all necessarily saltation can happen at the level of linguistic praxis;

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[12] I won't discuss the (extremely controversial) arguments Berwick and Chomsky rehearse (2016, p.26-39) for the possibility of non-gradualist evolutionary change; as I shall argue, the possibility is moot, since a better gradualistic explanation is available.

[13] As Patrick Bateson points out, creative play may have a very significant role in producing the sort of spontaneously developing behavioural structures on which the Baldwin Effect operates (Bateson 2006, p. 344). Linguistic ostentation may have sexual-selective benefits, while slang's role providing in-group shibboleths could also have been adaptively significant (see *e.g.* Burling 1986).

subsequently the acquired behaviour can be genetically assimilated as gradually as one likes, without needing to be shrunk-to-fit.[14]

Berwick and Chomsky bolster this claim that only a 'minimal' language faculty would be evolvable by pointing to 'evolutionary theory's evolution' in recent years (p.16-39), in particular the importance which has increasingly been attributed to stochasticity. Both random drift and '*directed* stochastic variation in fitness, migration, and heritability' (p.19), they argue, may thwart the prospects for any gradual adaptive process to produce and maintain a usable large-scale innovation for long enough that natural selection can act on it; 'individuals with [novel] traits must first climb out of a "stochastic gravity well" not governed by selection' (p. 21), and that task is more tractable for smaller or rapidly-evolved traits.

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[14] This provides, in passing, a response to the charge that, by talking about the evolved language faculty as more than just the narrow capacity for recursive thought (the 'faculty of language, narrowly construed' or FLN) envisaged in Hauser *et al.* (2002), Fitch *et al.* (2005), and Berwick & Chomsky (2016), I am thereby equivocating on the term 'language', effectively 'changing the subject'. It is the supposed need for a saltationist account of the faculty's evolution which, per the 'first motivation', forces the FLN as subject matter upon Chomsky and his followers, so unrecognisably narrow from what linguists originally set out to study. With that need removed, generativists are free to investigate a much broader and more interesting target. I thank an anonymous reviewer both for pressing this objection and suggesting the solution.

This may present a problem for traditional, 'gene-led' accounts of adaptation, though the point is controversial.[15] But despite the surprising insistence that '*none* of the recent accounts of human language evolution seem to have completely grasped the shift … to [Darwinism's] *fully* stochastic modern version' (p.19, emphases in original), the effect of invoking a Baldwin Effect is precisely to obviate such concerns. In that scenario, the 'target' behaviour is established and held in view by usage; by *parole*. This is what allows it to persist stably across generations without being inherited, while natural selection acts on *variations in the ease of its expression*. Thus 'plastic modification [*i.e.* learning] within individuals might lead the process and a change in genes that influence the character would follow; the one paves the way for the other' (Bateson 2005, p.35). Stochasticity does not thwart the Baldwin Effect; in fact, a certain amount of it appears necessary for the effect to work. Stochasticity generates a selection pressure for developmental plasticity in the organism, and as long as there is not too much stochasticity the Baldwin Effect allows that plasticity to be 'traded' for improved efficiency in learning (Ancel 1999; Bateson 2005, p.36).

Based on these considerations, Berwick and Chomsky argue for a 'strongly minimalist' language faculty, consisting of just a single recursive operation – 'Merge' – and its interfaces with the existing conceptual and sensorimotor systems (2016, p.11 & *passim*). I don't want to debate the linguistic or non-biological merits of the minimalist hypothesis here; the neo-Saussurean account of language's evolution is capable of accommodating it (Glackin 2011, p. 214). But it is important to note that minimalism is not presented as an empirically testable conjecture (Hauser 2016; Hauser *et al.* 2014); so the case for it depends in large part upon the claims about its comparative evolvability, and if a non-minimalist language faculty is

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[15] The counter-argument is, typically, that drift and stochasticity cannot account for *complex* traits; Berwick and Chomsky respond, in effect, by positing that the trait in question is not complex.

perfectly evolvable via a Baldwin Effect, then the minimalist hypothesis itself may be unmotivated. In any event, the hypothesis does not provide support for the associated evolutionary conjectures which Chomsky and others have developed.

IV: Quine's Challenge

Two major philosophical challenges to the generative theory, to which Chomsky's answers were not wholly persuasive, concern the nature of the linguistic rules which speakers can be said to follow in their linguistic behaviour, or which can be neurally 'encoded' by their I-language states.

The first of these is due to W.V. Quine (1970a). Generativists hold, in effect, that the rules of grammar – or, at any rate, a large subset of them – are held innately in a neural module in the individual speaker's brain; and that the rest of the rules for a particular grammar 'grow' there during the language acquisition process. But Quine's worry concerns whether any *sense* can be attached to this claim.

Any given set of formulae, Quine showed, can be generated as theorems by any of a potentially infinite range of extensionally equivalent axiomatic systems. That is, for any set of sentences deemed grammatical, there are an infinite number of possible rules of grammaticality which would generate all and only the members of that set. So which are 'the' rules determining the grammaticality of, say, English sentences, which some speaker of the language knows, and which are consequently present in her brain?

For adults, or those learning English as a foreign or second language, the question is easy to answer. If a native Danish speaker has learned English from Otto Jespersen's *Essentials of English Grammar*, or from a traditional schoolroom textbook, it makes perfect sense to point to one or the other set of rules as they are written in the relevant text (Quine

1970a, p. 386). Under the aegis of general intelligence, the speaker learns a particular set of axioms, and thereafter applies them at the level of performance. But a child whose first language is English, and whose competence in the language is not tied to overall cognitive ability, is not following any *particular* set of rules in this sense; '(h)e just behaves in the way that the rules describe … any extensionally equivalent system of rules could be ascribed to him indifferently' (Quine 1986, p.186). We have, Quine concludes, no basis at all for saying that any set of rules – for it must be some particular set – is 'in the brain' in the sense that generativists require.

Chomsky responded as though the objection concerned the *underdetermination of theory by evidence* (*cf.* Quine 1951), observing that not having enough data to definitively favour one hypothesis over all others was simply par for the course in any scientific endeavour (*e.g.* Chomsky 1969, p.67; 1980, p.14; 2000, ch.4). But as Quine's somewhat exasperated response made clear (1969, p.302*ff*), his objection is really about another central theme of his work, the *indeterminacy of translation*; he held that there could be no 'fact of the matter' about which set of rules was being followed. That is, the problem was not that we don't know the answer to the question of which set of rules is being followed, but that there can't be one, even in principle. For Quine, behavioural evidence is *meaning-constitutive* for any such rules; so if we can't distinguish the rules on the basis of behavioural evidence, no distinction can be drawn *at all* (1970b, p.180).[16]

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[16] See also the elucidation in Gibson (1986). Quine usually makes his arguments about indeterminacy with *semantic* meaning, rather than syntactic rules, in the cross-hairs; however, he does explicitly target them at Chomskyan syntactic rules in Quine (1969) and Quine (1970a). Quine would later repudiate the view that syntactic grammaticality was indeterminate in the same way as semantic translation, accepting instead that it was

The dispute has recently been revisited by Peter Ludlow, who appeals to the distinction between 'strong' and 'weak' generative capacity in grammars. Grammarians, on this view, should be concerned with systems that generate not just the correct terminal strings or conventional sentences, but also the correct phrase markers. That is, simply producing words in an acceptable order is not enough if, as with 'flying planes can be dangerous', the underlying grammatical structure of the string is ambiguous. So a set of axioms for some language should not only generate the correct strings, but should assign them to the correct phrase structures (Ludlow 2011, p.103-104). A similar approach is sketched in Cain (2005, p.15).

Does this underlying structure give us the 'something more to go on' than mere behavioural evidence, which we can use to deduce which set of rules is being followed? Perhaps. Quine himself, attributing the idea to Peter Geach (1965, p.201), suggested that looking for 'the whole tree rather than just the well-formed strings that it issues in' could in principle persuade us that a particular set of rules was being followed (1970a, p.391). But this approach, he cautioned, would require identifying which *are* the correct structures, and it's not clear that we have any better way to do that – Ludlow suggests none – than canvassing the assent of native speakers, thereby returning us to the circle of indeterminacy. Nor is it obvious that this solution can sit comfortably with the idiolectical character that Chomsky's 'Thatcherism' ascribes to I-language.

'adequately determined by behavioral dispositions.' (1987, p.10). However, he nowhere states his reasons for this volte-face. Quine's indeterminacy argument has been widely criticised; but the more robust version 'reconstructed' in Lance & Hawthorne (1998) would present the same problems for generativist linguistics.

The neo-Saussurean view provides an alternative possibility for the 'more imaginative' and 'less direct' route that Quine sought to identification of the phrase-structure rules actually operative in the speaker's brain (1970a, p.392). This view posits that linguistic rules are *initially* learned by our ancestors as performance, or *parole*, similarly to the contemporary non-native speaker's acquisition of a second-or-subsequent language. New linguistic rules may be generated in public language via utterances, without ever being explicitly formulated, so that no resource such as Jespersen's *Elements* is available to the learner. Nevertheless, as – by definition – a non-native with regard to novel constructions, such a learner is clearly envisaged by Quine (1970a) as being able to hypothesise and follow some particular rule or set of rules, as opposed to any equivalent one generating the same sentences; and it is those particular rules which are used, and progressively 'sedimented' into the 'Universal Grammar' (UG) – the invariant core of common structural principles generativists believe to be shared by all human languages – until they no longer need to be learned, by subsequent generations. This leaves open the possibility that many speakers will formulate and genetically assimilate many different extensionally equivalent codifications of the same linguistic behaviour.[17] But it seems unlikely that the different neural mechanisms would continue, through several millennia of language change, to produce extensionally equivalent grammars for every subsequent human language, nor that all such mechanisms would be equally amenable to the 'grafting on' of subsequent linguistic innovations. So ongoing language change and language innovation generates a selection pressure for standardisation; one of the various neurolinguistic phenotypes would have coped better with the frequent changes in the linguistic environment, and spread across the population.[18]

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[17] *Cf.* Quine's own 'topiary' metaphor (1960, p. 8).

[18] Of course, this is merely a speculative hypothesis, requiring empirical confirmation. But the fact that it or something like it is an empirical possibility means that the theoretical

The 'hardwiring' of those rules by the Baldwin Effect can thus be thought of as functionally analogous to an adult learner writing down the rules he is taught for ease of reference in the future; but a particular set is ultimately written down, and it is not 'indifferent' which. So Quine's worry that there is no fact of the matter which set of rules a native speaker is using does not apply; the particular set may prove epistemically inaccessible, but its identity is a matter of history.

V: 'Kripkenstein's' Challenge

There is another, better-known challenge to Chomsky's conception of linguistic rules, which is described though not explicitly endorsed by Saul Kripke (1982), and based on Wittgenstein's celebrated discussions of 'rule-following' (1953, §§138-243 & *passim*; 1981, s.VI & *passim*). Though he is, like Quine, chiefly concerned with the possibility of determinate semantic meaning, Kripke likewise turns the argument's fire explicitly on Chomsky's syntactic theories. Words and expressions have meaning, Wittgenstein argued, only in the context of their *use* in a 'language-game'. The proper use of words and expressions is therefore governed by the *rules* of the language-games in which they occur. Meaning is consequently *normative*; it results from following a set of rules for correct usage,

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problem can be solved; it does not, as Quine thinks, render the generativist's claims incoherent in principle.

and is the kind of thing we can get *wrong*.[19] But this raises the question; how can we tell if we are applying a given rule 'correctly'?[20]

In a famous example, Wittgenstein asks us to imagine teaching somebody the rule 'add 2'. We teach him '0, 2, 4, 6, 8 …', and then tell him 'now carry on as before'. He does so, but upon reaching a thousand begins '1004, 1008, 1012 …'. This is wrong, we tell him; carry on the way you were before. But this *is*, he responds, what he was doing before (1953, §§185).

As Kripke develops the point, we may imagine two distinct rules in play here, the conventional one for addition (*plus*) and the non-standard one ('*quus*'). If all prior evidence – every computation I have made below one thousand, or some other hypothetically never-previously-encountered figure – is consistent with either function, then my belief that I have been following the *plus*-rule rather than the *quus*-rule all this time seems impossible to justify, to myself or others. Nothing will serve as evidence: I cannot point to my prior behaviour, since the two rules diverge only in novel cases; I cannot appeal to a more basic procedure, like counting, since this merely supports the rule with another rule (how can I know that I

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[19] See e.g. Lance & Hawthorne (1998); Blackburn (1984); Brandom (1994); McDowell (1989), ch. 11.

[20] The views of 'Kripkenstein', notoriously, are generally regarded as *not* reflecting Wittgenstein's own, principally because they conflate his discussions of rule-following and private language (Kripke 1982, p. 3); however, they do thereby stress the same relevant themes of the essential publicity and normativity of language. I focus the discussion here on Kripke's account rather than a more orthodox Wittgensteinianism largely for historical reasons; it was Kripke who first applied the rule-following considerations to Chomsky's generative linguistics, and Kripke's formulation of them to which Chomsky (1986) would explicitly respond. See note 23 below.

have not, all this time, really been *quounting*?); nor can my mental contents settle the matter, since these are generally neither necessary nor sufficient to fix meaning.[21]

There is another intuitive option; perhaps it is true that I have never tried to add a number over one thousand, but I was nevertheless in a state of *disposition* to do so according to one rule rather than the other. That is, my meaning *plus* rather than *quus* is constituted by the fact that *had I* attempted to add two to one thousand, I *would have* given the answer '1002' rather than '1004'. But this purported solution ignores what Wittgenstein and his followers regard as the fundamental normativity of language; we need a distinction between the answer I *would have* given and the answer I *ought to* give. Perhaps I am simply a tired, a distracted, or an incompetent arithmetician, and prone to making mistakes. Using the dispositionalist method, as Wittgenstein says, 'whatever is going to seem right to me is right. And that only means that here we can't talk about "right"' (1953, §§258).

The argument has inspired a voluminous literature, but we need not concern ourselves here with the numerous suggestions that subsequent philosophers have made for 'straight' solutions to the 'sceptical paradox'.[22] Most of this literature is concerned with the possibility or impossibility of semantic meaning, which is Kripke's principal target throughout the book. There is a secondary target present, however, and what is important for our present purposes is the way Kripke explicitly applied his 'Wittgensteinian' argument to Chomsky's linguistics, and the 'sceptical solution' by which he proposed to neutralise its force.

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[21] Nor are they, in any case, available to a Chomskyan grammatical competence, which must work both independently of general cognitive capacity, and according to rules of which the follower is typically unaware.

[22] See *e.g.* the essays collected in *eds*. Miller & Wright (2002).

If a speaker 'follows a rule' in isolation from any linguistic community, Kripke posits, then there is indeed no difference between his truly following the rule he intends and his merely thinking that he is doing so. Since meaning must be normative – the kind of thing we can 'get wrong' – a meaningful 'private language' would be impossible; an I-language could not be normative in this way. Rule-following can therefore take place only in a linguistic *community*, where *others* can judge if we are following the rule correctly. Ascriptions of meaning, such as judging that a speaker means *plus* rather than *quus*, thus have a utility; they allow us to distinguish between those we can and cannot trust in our linguistic transactions. The idea of linguistic rules therefore makes essential reference to the linguistic community; meaning is an intrinsically *public* matter.

Now, while Kripke is like Quine primarily concerned with semantic rules, he does explicitly discuss the argument's application to syntactic rules of just the sort that Chomsky discusses. And while Kripke is careful to state that he *himself* is supportive of Chomsky's position (1982, p.30-1, fn.22), he is also clear that the 'Kripkensteinian' argument causes problems for that view. 'Competence' in the sense that generativists seemed to use it, he notes, is 'not a dispositional notion. It is normative, not descriptive ... [it] is dependent on our understanding of the idea of "following a rule"' (*ibid.*).[23] And this means that '(m)odern transformational linguistics, inasmuch as it explains all my specific utterances by my "grasp" of syntactic and semantic rules … seems to give an explanation of the type Wittgenstein would not permit' (1982, p.97, fn.77; *cf.* McNally & McNally 2012).

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[23] Tellingly, it was following the publication of Kripke's lectures, and in the same work in which he responded to these (Chomsky 1986), that Chomsky first replaced the term 'competence' with 'I-language'.

Chomsky insists, in response, that grammar should be regarded as consisting not of rules, but of principles and parameters (*e.g.* 1986, p.688*ff.*; 1995, p.17). This has looked to some like philosophical stone-kicking, as though he were purporting to distinguish between normative grammatical rules and meta-rules for generating them which, for unexplained reasons, were not themselves normative. But that is a misreading; what is actually revealed by this move of Chomsky's is the much more radical view that there are no rules in language *at all*. This rejection of rules is part of his rejection of E-language as unscientific; language as an object of proper scientific study cannot be a set of abstract, formal norms, but must rather be concrete, 'a real object of the real world', 'as real as chemical compounds' (Chomsky 1993, p.39; Chomsky 1988, p.679. *Cf. e.g.* Katz 1981).

Language as an object of scientific interest, then, is not *normative* at all on Chomsky's view, but *natural* and *biological*.[24] And as such, it is a property of individual speakers' brains. That is to say, 'public languages' *do not exist*. 'People who live near the Dutch border,' notes Chomsky, 'can communicate quite well with people living on the German side, but they speak different languages in accordance with the sense of the term Dummett argues is "fundamental"' (1992, p.101). That is, so-called 'public languages' are distinguished only by factors of class, politics, or race; their differences are not linguistic. Nor are the brains of, say, English, Finnish, and Japanese speakers interestingly different (Chomsky 1988, p.677). So there is no abstract, differentiated object such as 'the French language', in which native speakers of French partake, and according to whose normative standards their linguistic performance is to be judged. All that exists, for the linguist, are substantially overlapping and purely descriptive *idiolects*; neural structures internal to the minds of individual speakers, which happen in many cases to be similar enough to permit communication between peers.

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[24] Millikan (2003) provides a nice dissection of this opposition.

This is the position I have described as Chomsky's 'linguistic Thatcherism'; there is, he believes, no such thing as society as far as a scientific linguistics is concerned, just individual men and women. And if it follows that no community is available to safeguard the Wittgensteinian normativity of language, then that is a bullet Chomsky is happy to bite. The individual speaker cannot be 'wrong' in his linguistic behaviour; he can only be – as a matter of fact – unable to make himself understood (Chomsky 2000, p.7). An empirical science is concerned with what *does* happen, and not what *should*; prescription 'plainly has nothing to do with an eventual science of language, but involves other notions having to do with authority, class structure, and the like' (Chomsky 1988, p.675; *cf.* more generally Isac & Reiss 2008, ch.12).[25]

That linguistic prescriptions have often illegitimately been used as tools of social control is not in doubt, though someone of more liberal inclinations than Chomsky might conclude that we simply need to be much more fine-grained and inclusive about the shifting, overlapping, large and small speech communities which we interpret as autonomous bearers of their own normative standards. As to the picture of I-language as individualistic and purely

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[25] It has become customary since the publication of Hattiangadi (2006) to distinguish between the claim that meaning is 'normative', so that a speaker *ought* to apply a rule in a particular way, and the weaker claim that it is 'norm-relative', so that she speaks *correctly* in some sense if she does so. Hattiangadi takes the first to be untenable (since it would *e.g.* commit a speaker to always telling the truth), and the second to be too 'anodyne' to support Kripke's meaning-sceptical paradox (p.7). Chomsky seems to me to be committed to denying both normativity and norm-relativity about syntax; certainly, in rejecting 'prescriptivism', he evidently denies the stronger claim. I am happy, conversely, to uphold it; even if Hattiangadi's argument is successful, it pertains only to *semantic* applications of the rule-following paradox, and doesn't affect normativism about the syntactic rules of grammar.

descriptive, it is at least suggestive to observe the tension between this and the classic

generativist account of Creole formation; children acquiring language appear to 'look for' –

and 'discover' – the parameter-settings of the target grammar in accordance with UG

principles, even in deformed and disordered linguistic environments *where no such settings*

exist (Bickerton 1983; Kegl *et al.* 1999). In other words, the development of a speaker's I-

language seems, in some sense, to 'expect' the presence of an E-language. Nor need this talk

be taken as merely metaphorical; just as John Maynard Smith justified the attribution of

intentionality to the information encoded in the genome as the result of natural selection, so

Ruth Garret Millikan has defended at length the teleology of 'proper functions'. The E-

language or *langue* represents the 'Normal conditions' required by the I-language to develop

properly so as to perform its evolved proper function; it 'has helped to proliferate the [I-

language] mechanisms … during the historical selection processes that formed them'

(Millikan 2017, p.85). There is therefore a biologically robust sense in which these intentional

and teleological claims can be cashed out.

Millikan's proper functions also provide a clear sense in which, contrary to Chomsky's

claims, biolanguage appears to possess at least some intrinsically normative features. The

features of the linguistic brain attributable to natural selection will have a proper function in

the standard way (Millikan 1989, p.288); these present features of our brains originated as

copies of the same features of our ancestors' brains in part because those features allowed our

ancestors to acquire and use certain grammatical structures, and these present features exist

because (causally and historically) of that acquisition and use. Brains that do not carry out

their proper functions – reproduce the appropriate grammatical structures – are thereby

dysfunctional; they are not doing what they are *supposed* to do.

The features of the mature linguistic brain *not* attributable to natural selection – the

particular parameter settings left open by the LAD and determined instead by the practices of

the linguistic community the child finds herself in – likewise have a *derived* proper function (*ibid.*). That is, these features originate as the product of some prior device – the LAD – that, given its circumstances, has the acquisition and use of grammatical structures as a proper function and that, under those circumstances, normally causes such structures to be acquired and used by means of producing a fully-developed linguistic brain. And more; the linguistic practices of the community, too, originate as the product of numerous I-languages and LADs that, given their circumstances, have the performance of various linguistic tasks as their proper function and that, under those circumstances, normally cause such tasks to be performed by means of producing such linguistic practices. And so – insofar as biolanguage is regarded as properly biological – an individual's failure to reproduce the grammatical structures specified by her developmental parameter settings and the practices of her community will likewise count as errors of a sort whose possibility Chomsky rejects.

The collectivist, neo-Saussurean view has a straightforward solution to Kripke's challenge, which 'saves the phenomena' by preserving our common-sense intuition about the normativity of grammar; the rules of grammar – both the ubiquitous rules which form the UG, and the parochial standards specific to a given speech-community – are part of the public *langue*, held not completely in the individual speaker, but in collective practice. That means that Kripke's problem does not arise; judgements of linguistic competence on this view *are* both public and normative. 'Following a rule' of grammar, then, is nothing more than conforming to community standards; and the LAD is best thought of as a specialised module for helping the speaker to align herself with those standards quickly and easily, without reliance on general intelligence.

As to the point that the judgements of competence we are concerned with here are typically political in nature, this is undoubtedly true. But once again, a fully rich view of human biology sees us as characteristically social animals, the most salient feature of whose

environment is our interactions with our peers and fellows. We are, as Aristotle recognised, fundamentally *political* animals; the idea that a political judgement cannot thereby also be a linguistic one, since linguistics is part of biology and politics is not, hangs on a deeply impoverished notion of the human animal and its typical forms of life, one I earlier described as 'Thatcherite'. No doubt it is *regrettable* that community standards are used to exclude and subjugate, but the appropriate solution to this is political liberalism rather than theoretical eliminativism; we should recognise a much greater number and variety of shifting, overlapping, and often ephemeral communities as the legitimate and autonomous bearers of linguistic norms, rather than denying that such norms exist, or that they are part of language.

VI: Collective Rule-following and the Arbitrariness of *Langue*

Here is an influential objection which Simon Blackburn has levelled against Kripke's purportedly 'Wittgensteinian' reasoning, which will help us draw further links between biological theory and Saussurean views of language:

> *If the presence or potential presence of a community of persons practising the same way enters as part of the truth-conditions, part of the analysis, of what it is to follow a rule, the sceptic who won against the private individual looks equally set to win against a community which has the benefit of mutual support* (Blackburn 1984, p.295).

That is, if a solitary speaker has no further, 'ultimate' basis for distinguishing between his following a rule and his merely thinking he is doing so, neither can the linguistic community Kripkenstein appeals to; they, too, lack the means to distinguish between *collectively* following a rule and merely thinking that they are doing so.

We can sharpen the point of this objection with a well-known insight due to David Hull and Michael Ghiselin (e.g. Hull 1976; Hull 1978; Ghiselin 1974; Ghiselin 1997). A species,

or a biologically or biolinguistically significant subgroup of the sort that sustains a collection of normative linguistic practices over time, can be regarded in simply mereological terms, as the class of its members. But – much as I argued that a merely mereological or 'Thatcherite' view of public language as the sum of individual speakers and their utterances left us unable to explain what seem to be some of its key features – Hull and Ghiselin argue that we better understand such lineages as spatio-temporal individuals, acting together with unity and coherence of collective 'purpose', and pooling the genetic, behavioural, and environmental resources of each constituent organism to create 'homeostatic systems … amazingly well-buffered to resist change and maintain stability in the face of disturbing influences' (Eldredge and Gould 1972, p.114; quoted in Hull 1978, p.343).

So one way to think of the linguistic community (ultimately, the species; but similarly for any of the cross-cutting and overlapping subsidiary groups providing speech-norms of greater or lesser currency and duration) is as an individual in its own right, which invents or modifies the *langue* as an 'artefact' or technology, and trains itself over time to its use. Blackburn's challenge then becomes; if nothing internal or specific to an individual organism can provide a normative standard for his or her use of language, what could provide the same standard for the *langue* as a whole, as used by the wider linguistic community-individual?

The objection looks successful, but not troublesome. It establishes, rightly, that there is no higher or external standard which could be appealed to in judging the grammatical standards of a community, or species, *as a whole* to be 'correct' or 'incorrect'. In that respect, the community- or species-individual is in the same boat as the individual speaker. But at the same time, it is hard to see what *sense* could be attached to any such judgement, as it pertains to a community or species, in the way that there is clearly something substantive and readily comprehensible for Chomsky to deny is predicable of idiolectical speakers. What could it

even mean to say that all French speakers, or indeed all human speakers adhering to universal principles like that of subjacency, were *ungrammatical* in the rules they observed?

In fact this question leads us back, once more, to Saussure. In the *Cours de Linguistique Générale*, he famously declares as 'the first principle' of linguistics, the fact that 'the linguistic sign is arbitrary' (1983, p.67). That is, no natural connection exists, even in cases of onomatopoeia, between any given sign, or signal, and the thing it signifies. But this is not merely an obvious point about vocabulary, Saussure maintains; it is 'the organising principle for the whole of linguistics, considered as a science of language structure,' and so applicable equally to grammatical rules (1983, p.68).

But to say that a signal is arbitrary does not mean that it is either conventional or volitional. The *parole* a speaker produces is volitional, in that it is the freely-chosen expression of an individual will; I may choose to say one thing, rather than another. Conventions, in turn, are 'practice(s) which people are free to adopt, adapt, flout, or change by mutual agreement' (Harris 1990, p.49). But *langue* is neither of these things; 'the individual has no power to alter a sign in any respect once it has become established in a community' (Saussure 1983, p. 68). There is a whiff of paradox here, certainly, since signs are established in *langue* just by their adoption in *parole*. But while *langue* may alter over time, it is equally anchored by continuity with past usage, and the new speaker so comes to and must experience its standards as inflexible (1983, p.74). Nevertheless, Saussure stresses, it is 'unmotivated'; so thoroughly does it lack any grounding external to itself that a 'community of language users' intent on reforming it could find 'no issue … to discuss,' no independent criteria at all for its revision (1983, p.73).

This leaves us just where our reflection on Blackburn's objection to Kripkenstein and the Hull-Ghiselin species-as-individuals thesis did. From the perspective of the individual speaker, the rules of *langue* are normative and unbending; though she may move between

communities, socially or geographically, her speech at any moment may be judged grammatical or ungrammatical by the lights of her current linguistic environment. But the community's use of these same rules is not normative at all, but incorrigible; from a collective point of view, any other set of rules might just as well have been chosen, and thus no normative standard exists to which the rules operative in any particular linguistic environment could even in principle be answerable.[26]

Of course, this does not really answer Blackburn's worry, which is that just as for the individual speaker, there will be nothing which constitutes a 'fact of the matter' whether a community is following the same rules as it did in the past, or merely thinking that it is doing so. But this is a separate question from that of whether a normative standard is available for judgements of correct or incorrect usage of grammatical rules. For the individual speaker, that standard can be given by community assent; for the speech-community, no such standard is available or necessary.

VII: Conclusion

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[26] The point I am making does not depend on a particular view of the metaphysics of species, and so can be equally well made in the language of kinds, though the issues are perhaps less clear: from the perspective of the individual speaker of language, a given set of practices at a time form a Nash Equilibrium, so no individual speaker can profitably unilaterally depart from those practices, though the community can; and the equilibrium is not stable, and is readily perturbed, which is why those sets of practices change over time. I am indebted to Kim Sterelny both for this observation and for this reformulation of the point.

If we take seriously the idea that a science of linguistics should be regarded as a branch of human biology, then consonance with the best current biological theories is an important desideratum. And the communitarian account of biolanguage, I have argued, scores far better by this measure than the individualist one. As I have shown, it also defuses two well-known philosophical problems for generative grammar.

Both the communitarian and individualistic theories substantially accept the 'core' or 'classic' Chomskyan, generativist account of how language is initially acquired by the speaker, on the basis of an interaction between the linguistic information present in her environment and her innate specialisation for linguistic behaviour. They share, too, a commitment to Chomsky's views about the substantially invariant structure of all natural human languages which results. But the two views differ radically in the relationship they posit between the individual speaker and the community and its practices which constitute his or her linguistic environment.

On the dominant view, that environment is simply a pre-existing theatre for the development of a speaker's idiolect. Chomskyans do not, of course, deny that an appropriate developmental environment is necessary if the child is to acquire language successfully; but in the presence of such an environment, which they hold amounts to nothing more than the sum of other idiolects in the vicinity, the process of development simply unfolds according to a genetic program. But this understanding sits very ill with the shift in recent decades away from genocentric, individualistic pictures of evolutionary and developmental biology. And though this shift remains controversial, Chomsky and his followers have professed their enthusiasm for it (*e.g.* Chomsky 2007a; Chomsky 2007b, p.18; Piatelli-Palmarini 2008). If any view of the biology in biolinguistics now seems 'facile', it is the orthodoxy.

The alternative view of biolinguistics emphasises collaboration and interdependence at all levels: between genetic and environmental information in guiding human development,

between humans and their physical and cultural environment in shaping each other over the course of their evolutionary history, and between individual humans and their communities and societies. It posits a deep, reciprocal, and historically-extended interplay and dependence between what 'goes on' linguistically inside, and outside, the speaker's head. This public, communitarian, and normative view of language repudiates the 'linguistic Thatcherism' of Chomsky's position, with its purely reductive view of human sociality; as I hope I have shown, it thereby far better reflects our best current scientific account of what it is, biologically speaking, to be human.[27]

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